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HERBERT S. ELEUTERIO

Transcript of an Interview
Conducted by

James G. Traynham

at

Wilmington, Delaware

on

25 February 2000

(With Subsequent Corrections and Additions)

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Herbert S. Eleuterio

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HERBERT S. ELEUTERIO

1927 Born in New Bedford, Massachusetts on November 23

Education

1949 B.S., chemistry, Tufts University
1953 Ph.D., chemistry, Michigan State University

Professional Experience

E. I. du Pont de Nemours & Company
1954-1959 Research Chemistry, Polychemicals Department
1959-1963 Research Manager, Industrial and Biochemicals Department
1963-1968 Division Head, Experimental Station, Explosives Department
1972-1973 Director, Experimental Station, Explosives Department
1973-1976 Assistant Research Director, Polymer Intermediates
1976-1977 Production Manager, Nylon Intermediates, Petrochemicals Department
1977-1985 Director, Research & Development Division, Petrochemicals Department
1985-1989 Technical Director, Atomic Energy Division, Petrochemicals Department
1989-1992 Director, New Technology Studies

1993-present National University of Singapore
Visiting Professor

Honors

1987 Chemical Pioneer Award, American Institute of Chemists
1991 Technical Excellence Award, DuPont Fellows Pederson Award
1995 Carothers Award, American Chemical Society
1995 Lavoisier Medal for Technical Achievement
1995 Singapore's NSTB Medal

ABSTRACT

Herbert Eleuterio was born in 1927 in New Bedford, Massachusetts. His parents had emigrated from the Azores, and he spoke no English until he began first grade. His interest in science was sparked by a middle-school biology teacher, and he decided to pursue chemistry after a customer on his paper route gave him a book on organic chemistry. He attended Tufts University, where he was positively influenced by his calculus teacher. He nearly went to graduate school for math, but the dim employment prospects for mathematicians led him to choose chemistry instead. Eleuterio received his Ph.D. from Michigan State, and married shortly after. He spent a year as a post-doc at Ohio State, then took a job at DuPont in Wilmington, Delaware. He was assigned polymer work even though he had no formal polymer background, and he was immediately very successful. His work there included propylene polymerization, olefin catalysis, olefin metathesis, and fluoro polymers, especially high-performance lubricants such as hexafluoropropylene oxide [HFPO]. Eleuterio also became interested in the theory and practice of knowledge creation and knowledge management through his work in exploratory chemistry at DuPont. He spent much of his later career discussing his concept of “degrees of freedom” and its relationship to scientific creativity. He was also a major proponent of creative teamwork in the laboratory. During his tenure with the Atomic Energy Division, which functioned mostly in South Carolina, he helped to create the Ruth Patrick Science Education Center at the University of South Carolina in Aiken. Eleuterio’s final project at DuPont was globalizing corporate R&D. After his retirement from DuPont in 1992, he began teaching part of every year at the National University of Singapore in the engineering and business programs. He also started the Process Analysis and Optimization Enterprise, which evolved into the Center for Process Engineering, in order to build links between industry and academia in Singapore. Eleuterio concludes the interview with a discussion of his family.

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.

TABLE OF CONTENTS

1	Childhood and Early Education Growing up in Massachusetts. Parents and cultural background. Early education. Choice of undergraduate college.
3	Higher Education Undergraduate education at Tufts University. Important influences. Graduate school at Michigan State. Marriage. Starting a family. Post-doc at Ohio State. Looking for employment in industry.
8	Early Career Achievements in chemistry. Research interests. Joining DuPont. Ziegler catalysis. Study of scientific creativity. Interest in chemical models. Polyhexafluoropropylene. HFPO.
22	Later Career Knowledge management. Creation of the Ruth Patrick Science Education Center. Globalizing DuPont's corporate R&D. Teaching at the National University of Singapore. Environmental concern. Role of future chemists.
30	Conclusion Family. Final thoughts.
35	Notes
37	Index

INTERVIEWEE: Herbert S. Eleuterio
INTERVIEWER: James G. Traynham
LOCATION: Wilmington, Delaware
DATE: 25 February 2000

TRAYNHAM: Dr. Eleuterio, I know from things that I have read that you were born on November 23, 1927 in New Bedford, Massachusetts. Can you tell me something about your parents and your early childhood there?

ELEUTERIO: Yes. My parents emigrated from the Azores. I was born in New Bedford, Massachusetts. We lived in a Portuguese community and we spoke Portuguese at home. I didn't learn to speak English until I went to the first grade. My given name that my parents gave me—my first name—was Umberto. The first thing that I recall is the teacher saying, "You're in the United States, and we're going to change your name to Herbert." So I came home and told my mother and she said, "Fine. If that's what the teacher thinks we ought to do, your name's Herbert." But the other thing that the teacher did—she said that by the time I was in the third grade, I would be one of their best spellers. So I started to be goal-oriented from a very early age because of one of the schoolteachers.

TRAYNHAM: What was your father's work?

ELEUTERIO: Well, my father came to the U.S., started a business and then lost it during the Great Depression. He went to work in the textile mills in New Bedford because New Bedford was a big textile-manufacturing center.

TRAYNHAM: Were you born in the United States?

ELEUTERIO: Yes, sir. I was born in the United States.

TRAYNHAM: All right. What were your father's name and mother's names?

ELEUTERIO: My father's name was Francisco and my mother's name was Donatilia. She was educated in the Azores to be a lady. Her parents were quite well-to-do. Women at that period

of time had to know how to sew, embroider, and whatnot. After coming to the U.S. she also worked. She was a seamstress and was very good at it. She would inspect and correct other people's mistakes. So both of my parents worked. Although my parents were Catholic, and so am I, I was brought up with a New England, Protestant work ethic very early on. That was imbued in me by the schoolteachers. Everybody that was healthy, not handicapped, ought to have a job.

Although I was not old enough, I started peddling papers on a newspaper route when I was nine years old. Everybody was supposed to have a job and I wanted to have a job. My parents wanted me to go to college. My father had one brother and one sister. His brother was a medical student in Coimbra University in Portugal, but died in his second year of medical school. So my father wanted me to have a college education. In fact, I think he really wanted me to be a medical doctor. But early on I got interested in chemistry. I couldn't stand the sight of blood anyway, so I wouldn't have made a very good medical doctor.

TRAYNHAM: Do you remember the circumstances under which you became interested in chemistry?

ELEUTERIO: Yes. As a matter of fact, I do. In the seventh grade, junior high school, I had Miss Cady, who was a biology teacher. I had saved some money and bought a microscope. I got curious about all the little organisms that existed in water when you look at them under a microscope. So that was my first interest in trying to understand life that you can't see with the naked eye. She also did one other thing for me. Miss Cady got me a special permit so I could go to the public library and go to the room that kids weren't allowed in, which was histology. So I got interested in making slides and that sort of thing.

But my real hero in chemistry was—I told you, Jim, that I had a paper route. My real interest in chemistry was really perked by one of my customers, who was moving away. He had a bunch of books in his attic and he asked me if I wanted them because he knew I liked to read. He gave me a book on organic chemistry by Dr. [James Bryant] Conant (1). I read the book, memorized formulas, and tried to make soap and acetic acid and all that sort of thing. Obviously, I didn't really have a good understanding of what I was doing. But I was going to become an organic chemist from about the eighth grade on.

Another important influence in my life was in high school. The chemistry teacher was a great person by the name of Dr. Marvell. Dr. Marvell knew that I was interested in chemistry. Every once in a while, he'd get laryngitis and he'd let me lecture in high-school chemistry. Of course, I was then hooked. So I wanted to be an organic chemist and I wanted to be an organic chemistry teacher. When it came time to choose a college, I looked at several colleges. Quite frankly, I eliminated MIT [Massachusetts Institute of Technology] very rapidly because I was told by the high-school counselor that MIT graduates weren't interested in people.

TRAYNHAM: That was your high school counselor?

ELEUTERIO: Yes. I looked at Harvard [University] and Tufts [University]. Yale [University] looked like it was too far away from New Bedford. I went to Tufts because my geometry teacher and dentist went there. On weekends, I worked in the Fletcher School library, at a time when some rather interesting people were using to the Fletcher School's law library, like Daniel Patrick Moynihan and [Bill] Goodwin. I got curious about some of the books in the stacks. So I learned a little bit about comparative religion, comparative economics, and got acquainted with a lot of people that later on were part of [President John F.] Kennedy's administration. But my interest was really chemistry. The other motivating factor was that I wanted to get an undergraduate degree before I got drafted into the army. So, essentially by going to summer school and taking heavy loads, I completed all the requirements for a B.A. in two and a half years. But then nineteen days before I was supposed to get drafted, the draft was terminated. I really loved my calculus teacher, Mrs. Graustein, and I decided to take some more math and physics and get a B.S. in chemistry instead of a B.A. So I graduated in 1949, having entered Tufts in 1946.

I really didn't enjoy undergraduate school, but I was influenced by two teachers. One of them was Mrs. Graustein, who used to come to the library every once in awhile and chat about interesting math problems and why she'd become a mathematician. She had a student by the name of Sutherland Frame, who was head of the math department at Michigan State [University]. So she wanted to encourage me to become a mathematician. I told my father I was going to go to graduate school to study mathematics and he asked, "Well, what are you going to do for a living?" That was a good question because I found out I could get a teaching assistantship at Michigan State, and math graduate students didn't get assistantships in those days.

So I minored in math and physics but I majored in chemistry. I took a degree with Harold Hart at Michigan State. We did some interesting mechanistic research on the alkylation of phenols and the rearrangement of ethers. It was very fortuitous because without really appreciating it, we were breaking some new ground in mechanistic organic chemistry, which at that time—this is now the late 1940s, early 1950s—was just beginning to transition from synthetic chemistry to more physical organic chemistry. Harold was an absolutely great person. He had all the characteristics that you like in a professor. He was demanding, but fair, and expected you to work hard. He was a student of Penn State's [Pennsylvania State University] Joe Simon. Joe Simon was a very hard taskmaster, as most people knew. The first thing Harold asked me to do was take a vacuum pump apart, put it back together again, and if you screwed up, it was a mess and somebody had to clean it up—which is a good lesson.

The other person that influenced me a lot was Max [T.] Rogers. He was, I think, a student of [Linus C.] Pauling's. Max was a really terrific physical chemistry teacher. He had a terrible time remembering organic formulas, but he could derive the equations from first principles like you never saw anybody derive. It turned out that my lab space was next to his office. He'd come in and ask me for the structure or formula of well-known organic chemicals.

But, in turn, he would chat with me about quantum mechanics and statistical mechanics. Max gave me a couple books on quantum chemistry and suggested that I read certain parts then he would discuss the material with me.

So I got an education for free just by listening to Max. Both Harold Hart and Max were just super teachers—great role models. Now, the work that Harold and I did for a Ph.D. turned out to be significant because this was a period where [Donald J.] Cram and [George] Hammond were trying to change organic chemistry from being dogma and pot boilings. So our papers got a lot of favorable comments from both Cram and Hammond and these are cited in a number of organic chemistry books of that period. I was a curious person anyway, and it really just encouraged me to try to do pioneering things and try to understand them at the molecular level. I wanted to teach when I got my Ph.D. But teaching jobs—this was just before the Korean War—were difficult to get. I really wasn't interested in going into industry. So I decided to take a post-doc. I got accepted for post-docs by Mel [Melvin S.] Newman at Ohio State University, George Hammond at Iowa [State University], and [Herbert C.] Brown at Purdue [University]. Some of the post-doc appointments had caveats. For example, if granted, you were obliged not to take an industrial post but to go into academia.

TRAYNHAM: That seemed like an odd request.

ELEUTERIO: Well, in a way, it's probably not unreasonable when you look back at it. Because when I was in graduate school, professors frowned on people who were married because they thought wives took time away from the lab. It was only because the World War II veterans were around that they had to change their mind and found that if you're really very focused and goal-oriented, you know, wives are not necessarily all that much of a distraction. But this was a transition period in many ways. That was another one at the time.

TRAYNHAM: You were married at that time.

ELEUTERIO: Yes. I was married and I had a couple kids.

TRAYNHAM: I see. So you'd gotten married while you were in graduate school.

ELEUTERIO: Yes. In fact, I married one of my students. I was a TA [teaching assistant] in physical chemistry. In the class I had, there were three women and the rest of the class was male. Most of these students wanted to get into vet school or med school, so they'd kill for a grade. Much to my surprise, this very shy, red-headed girl kept ruining the curve. She would get the best grades. It really caught my attention that she would do so well. She also caught my attention because in making a pycnometer, she gave me the hot part of the glass. So I got

burned early. She turned out to be a very good student. We didn't date while she was my student. But she went to the same church that I went to in East Lansing and I got to know her. Eventually we married and had a family. We've been married for almost forty-eight years. She remains a very good student.

She became a genetics professor after having five kids—going back to graduate school after being a housewife for seventeen years. The first course they made her take was the second semester of biochem. In those seventeen years, biochem had changed from blood, urine, and sweat analysis to, in fact, physical organic chemistry. But she stuck it out. I have to give her a tremendous amount of credit. I really admire her. She got a Ph.D. in molecular biology and taught for over twenty-five years after raising children. I just can't tell you how proud I am of her.

TRAYNHAM: You mentioned that you had three offers of post-doc opportunities.

ELEUTERIO: Yes.

TRAYNHAM: What led you to choose the one you did?

ELEUTERIO: Yes. I had an offer from Hammond. At the time, he was at Iowa State. I looked at how far Iowa State was from Massachusetts, and it looked pretty far. The other motivating factor is that my wife had relatives both in East Lansing and in Dowagiac, Michigan, which is near South Bend, Indiana. I knew she was very close to her relatives, and so from Columbus, Ohio, it looked a lot easier to get to Michigan—I didn't have a car at the time. Also, it was an overnight train ride to Massachusetts. So I chose Newman.

TRAYNHAM: It's interesting that in both your undergraduate institution choice and your post-doctoral choice, personal factors other than consideration of chemistry was the major role.

ELEUTERIO: Yes.

TRAYNHAM: And you had good experiences.

ELEUTERIO: Well, great experiences. In fact, at Michigan State I had decided to work for Max Rogers because we got to be pretty friendly. He'd asked me to make some compounds for him, and actually one summer he paid me. So I got financially supported by Max. But I ran into Harold Hart in the line for supplies. He had a very charming personality. I hadn't really

firmly committed to Max. I hadn't signed anything yet. But I had really leaned pretty heavily toward Max. So Harold asked me if I'd made up my mind yet, and I said that I was thinking about working with Max. He said, "Well, if you haven't made up your mind, why don't you drop in? I've got a couple of problems you might be interested in." Max wanted me to do some dipole moment studies on some chlorinated compounds. I liked Max. I found him extremely personable. But somehow Harold, with his shy personality, sort of won me over. Of course, I knew of Newman's work because Harold really thought very highly of Newman. He also thought highly of Hammond. But it was true—it was personal factors.

The Newman experience—I had signed up for a two year post-doc. But this was 1953, and the Korean War was taking place. North American Aviation, if I remember, in Columbus, Ohio, was making Air Force planes. I had a next-door neighbor. We were living pretty sparsely. The next-door neighbor's wife kept telling my wife, "Why is your husband knocking himself out and not making any money? My husband can get him a good job at North American Aviation." We had a couple of kids and we were struggling. Part of the reason was that at the time, there was no faculty housing at Ohio State. You had to make do with whatever housing you could find and afford. Then because of the war effort, real estate prices went up and whatnot.

Newman had given me a problem that was really for two years. The first year I was to make sterically hindered compounds that couldn't be hydrolyzed except at very high temperatures. So that was the objective: to make a bunch of compounds, and if they could hydrolyze easily, then he wasn't interested in them. Then Newman took off for a six-month sabbatical and spent most of his time in natural products and sort of lost interest in my project. But at the time, much of my effort was synthesizing compounds and trying to make small, gold-plated reactors that could stand very high temperatures.

So I learned how to gold-plate little steel cartridges and whatnot. I became very friendly with Christopher [L.] Wilson, who was at Ohio State, and Harold Schecter. [Al] Henne sort of piqued my curiosity about fluorine chemistry because I really didn't know anything about it. You can tell by what I've said to you so far, Jim, that I'm a curious guy and I like to learn new things. So I learned a lot of fluorine chemistry, which later came in handy. But I didn't anticipate it at the time. Schecter was interested in mechanistic organic chemistry. Harold was a perfectionist. He liked to lecture and finish exactly on the minute up to fifty-five minutes. So I served as a guinea pig for Harold for a while. But I learned a lot. It was, again, a good learning experience.

But I kept racking up debts. Because I'm basically a pragmatic New Englander, that's one of my value systems. I didn't like to have debt. So I decided after one year that I'd better earn some money and support my family. I looked at a couple of teaching jobs—one at Swarthmore [College] and one at Cornell [University]. But they would pay me less than I was making on a post-doc. I couldn't really pay my bills with a post-doc. So I decided to interview industry. I had quite a number of interviews and I basically settled on two of them. One of them was [E. I.] DuPont [de Nemours & Co., Inc.] and the other one was Union Carbide [Corporation]. But it was Union Carbide in Orange, New Jersey. I didn't really want to live in

such a congested metropolitan community. DuPont was interesting for a couple of reasons. One, the offer was in a pioneering group, whose objective was to do exploratory chemistry, and again, the living areas were pretty affordable and accessible. So I came to DuPont on March 1, 1954, and I stayed until December 31, 1992. I was very fortunate.

In 1954, when I joined Frank Gresham's exploratory group, Ziegler polymerization was on the scene. In fact, my first assignment was not with olefin catalysis. But in March of that year—I joined in March—one of the chemists, who was a Swiss chemist by the name of Nick [Nicholas] Merckling, had accidentally discovered Ziegler catalysis.

TRAYNHAM: This was a DuPont employee?

ELEUTERIO: Yes. He was a DuPont employee. It's an interesting story, and I tell it in a number of lectures that I've given because Nick was very frustrated. He was trying to make what's called poly-housetops. He was trying to polymerize norbornene. Looking at norbornene, if you join the double bonds together, you come out with poly-housetops. He wasn't able to get anything organic from norbornene. So he decided to try copolymerizing ethylene and norbornene. Nick had just gotten a very negative performance review. He was single at the time, and went down to Baltimore to see a girly show and do what single young men from Europe do on weekends. He drank a lot of beer, and came to the Lavoisier Library on Sunday to sober up. So he wanted to figure out what experiments he was going to do on Monday. He ran into some work by Hans Fisher on aluminum chloride and ethylene.

On Monday morning, he looked on his shelf—what did he have besides aluminum chloride and aluminum? He had titanium tetrachloride. So he ran titanium tetrachloride and aluminum, along with ethylene norbornene, and carried out the experiment. He got mostly inorganic material after he worked the reaction product up. He got a little bit of resin and he took an infrared spectrum of it. One of the group supervisors by the name of [I.] Max [Maxwell] Robinson came by and asked Nick what was new. Nick said, "Nothing. I just get a bunch of crap." Max looked at the infrared and said, "What's that?" He said, "That's an infrared of this failed experiment." Max said, "You see that doublet at 12, 13 microns?" Nick said, "Yes." He said, "You know what you've done? You've made linear polyethylene." Max knew from earlier work at DuPont's Central Research Department that linear polyethylene would have a very characteristic doublet in that region of the infrared.

So Max took the infrared scan and went down to the section manager, all excited, and told him. So the next day, I don't know, fifteen people were assigned to try to polymerize olefins. At the time, we didn't know about [Karl]'s work. At the time, some of the senior managers apparently knew about some work at Standard Oil of Indiana. But they had a confidentiality agreement, and certainly none of the bench chemists knew anything about this. So they knew there was something there. Still no one knew about Ziegler's stuff until September of 1954. Ziegler made it public and offered anybody interested in polyethylene licenses. They still didn't know about [Giulio] Natta's work. What had happened with

propylene was that one of the senior research associates and one of the young chemists had been assigned to try to polymerize propylene. In fact, they were successful in polymerizing propylene, but it was pretty tacky and very greasy. Eventually they got some solid polypropylene.

It turns out later, in patent interferences, that they were a week late. Natta beat them by a week in filing for a U.S. patent. There's a very interesting story, and I've catalogued the events that have happened. It turned into a five-party interference—Standard Oil, Phillips [Petroleum Company], Hercules [Inc.], Montecatini, and DuPont. Interestingly enough, Montecatini, who had the earliest date on patents in the U.S., was declared ineligible because of fraud. So that left Hercules, Standard Oil, Phillips, and DuPont. Hercules dropped out voluntarily, at that time, so it became a three-party interference. Eventually, Standard Oil was beat out by Phillips. Phillips was declared the owner of the U.S. polypropylene patent because of the way the patent claims were written up. DuPont contested it. This patent interference wasn't settled until 1983, and Phillips was the final winner on the U.S. Patent.

TRAYNHAM: But Natta got the Nobel Prize.

ELEUTERIO: Yes. That's right. One of the reasons I'm telling you this story is that my first assignment on polypropylene was to try to come up with a process for highly crystalline polypropylene. Polypropylene that was made by Warren Baxter and [Gelu] Stamatoff had very low crystallinity. In fact, at the time, they could only measure a crystallinity index because the x-ray techniques were all calibrated on the basis of metals and polymers are non-metallic. Eventually all those crystallinity indexes were found out to be off by a factor of almost two, so they were very low. Anyway, I found out how to make high-molecular-weight crystalline polypropylene. My assignment was to come up with a process for making it. But I was very curious about the mechanistic aspects of propylene polymerization.

There were a lot of theories at the time about what was going on. One day my supervisor at the time, Art [Arthur W.] Anderson, came in. He said, "Herb, we've got a real problem. One of the patent attorneys was supposed to respond to an objection by the Patent Office on one of our polyethylene applications and he forgot about it. We've got to get this in by Friday afternoon. How about following this Standard Oil of Indiana patent and then you file an affidavit of whatever results you have and we'll get the affidavit to Washington by special courier."

So I made a molybdena on alumina catalyst and went through the process of making polyethylene and filing the affidavit. I had all this catalyst left over. We had been specifically told by the research manager that he didn't want a heterogeneous catalyst, he wanted a homogeneous-catalyst process to make polypropylene. So we were told not to use any heterogeneous catalysts. But I was curious and I had the stuff. So I ran propylene with this moly-on-alumina catalyst and I got a very surprising result. I got a lot of polymer. When I took an infrared spectrum, lo and behold there was the doublet of linear polyethylene.

So I went down to the high-pressure lab and told the operator there, “Lou, you have made a mistake. I wanted propylene and some ethylene must have gotten in.” He was about six foot seven, weighed about 350 pounds. He looked down at me, and said, “You little twit. If you think I screwed up, I’m going to take you and I’m going to put you up on that chandelier.” I said, “Lou, no, I don’t think you made a mistake. I think something’s happened and it’s very curious. So how about running this again, and take a sample of the exit gases after the polymerization is over. Let’s look at the exit gases. Make sure you take a sample of the entry gases, too.”

So he did. The entry gases were 99-plus percent propylene. Certainly no ethylene in them. The exit gases were a mixture of propylene, ethylene, and butenes. Obviously what had happened was that I had disproportionated propylene. Since this was a moly and alumina catalyst, which was the basis for the Standard Oil of Indiana catalyst, the inference was Standard Oil of Indiana catalyst couldn’t make polypropylene. They’ve got to make a copolymer with this mixture of ethylene and propylene. So there was a freeze put on all of this work. We weren’t allowed to publish. We didn’t patent it, which I think in retrospect was a mistake. But Art Anderson always gave me degrees of freedom and I was able to pursue what eventually became the olefin metathesis reaction, just out of curiosity.

TRAYNHAM: Did you continue that work, in spite of the freeze, more or less as extra time, or were you able to do it in the regular work time?

ELEUTERIO: No. I worked hard.

TRAYNHAM: You had other assignments.

ELEUTERIO: Oh, yes. I had a principal assignment and then—I don’t know where I got this idea, Jim, but I had three objectives. I had the assigned objective. Then I had a long-range objective to try to understand the mechanism of what was going on. Then I had a third objective, which was to follow up this very curious reaction. Now, at the time, we were not able to polymerize internal olefins. So I got some cyclopentene, and I got some cyclohexene, and tried to polymerize them. Cyclohexene, I got some kind of oil that was not very well defined. But from cyclopentene, much to my surprise, I got an elastomer. The infrared spectrum looked like the catalyst had snipped the cyclopentene in half and then carefully re-sewed it again.

TRAYNHAM: Took it out of the molecule.

ELEUTERIO: Yes. Right. So this was very unusual. But even with the first set of experiments on metathesis, the preferred mechanism for organic reaction at the time was four-membered rings. Professor Petit at Texas had been hot on four-membered rings. So everybody was trying to make four-membered rings out of everything. So the preferred mechanism at the time, favored by our academic consultants, was to make four-membered rings. But it didn't make sense to me. I thought carbenes were involved because the reaction occurred at very low, very modest temperatures. So Gardner Swain, Jack [John D.] Roberts, and Andy [Andrew] Streitwieser were all our consultants at the time. I told Jack what I thought was going on. You know Jack, I'm sure, very well. He said, "Christ, Eleuterio! That's thermodynamically impossible. Eastman sent you the wrong goddamn stuff." I'd been very careful to make sure it was cyclopentene. I said, "Jack, no, it can't be. I don't know what is going on, but it's not impure material—it's cyclopentene." So he got angry, actually. He stormed out of my lab and he went down to Gresham's office. He said, "Look, that reaction is thermodynamically impossible. Get one of your good physical chemists to do the thermodynamics on the reaction." So Don [Donald] Payne, who was from the University of Michigan and a very good physical chemist, was assigned to disprove what I had done. He had a problem because nobody had done this reaction before, so what assumptions are you going to make? Well, after three months, the conclusion was, so that we wouldn't offend Jack, that it was thermodynamically possible but kinetically it probably wouldn't be very significant.

So people lost interest in following up on the finding. But we filed a patent and we got it issued on what came to be known as polypropylenes. But I could never really get people interested in what were obvious extensions of the reaction. I got a patent issued and I actually had tried to get DuPont to file a patent on the disproportionation reaction of olefins to upgrade propylene. They came back saying, "We're not in the oil business." So the management didn't want to pay the filing fees. So we never did file that patent on olefin disproportionation.

I'm telling you this in some detail because there's a lot of confusion in the open literature on metathesis and ring-opening metathesis polymerization. The point I really want to make is that the confusion is part of the difficulty with our patent system and, you know, the constraints that you have in industry for industrial chemists. Academics don't have those constraints. A lot of academics will follow up leads that come out from industry and not attribute where those leads came from. There's nothing wrong with this in most situations, but it does confuse the history. What happens is that whatever gets published in the open literature gets cited as being factual and that isn't always the case. One of the cases involves the metathesis reaction, the ROMP reaction. The ROMP reaction is the ring-opening metathesis polymerization that Dick [Richard R.] Schrock at MIT, and Bob [Robert H.] Grubbs and his group have done a very good job of following up. But both Grubbs and Schrock erroneously attribute the reaction to Natta.

This is kind of an interesting twist because it was discovered at DuPont. We filed a patent, as I told you, in Europe. The practice in Europe was that if you file a patent in an area, they allow you to see what other applications have been filed. So Montecatini found out that we had been able to polymerize cyclopentene. Professor Natta came to the United States to negotiate some patents on polypropylene. He asked the polychemicals research director to see

me, by name. Now, I had been with DuPont three or four years. I was a bench chemist, you know. I was a nobody. So Frank Gresham called me in. When Frank was nervous, he'd chain smoke and he had a very squinty look in his eye. He'd look you in the eye and you knew you were in trouble. He said, "Are you Italian?" I said, "No." He said, "Why does Professor Natta want to see you?" I said, "I don't know." He said, "Well, he's coming to see Frank McGrew, and he specifically asked to see you." Frank's look made me feel like he thought I'd done something wrong. Well, to make a long story short, Natta's train from New York was late so I couldn't see him in person. But he gave me a call from downtown. He started to talk in Italian and I told him, "*No capisce.*" I mean, I could understand a little bit. So he spoke in English and he said, "Well, we found out about your polymerization and I want to congratulate you." He was very nice on the phone.

But because of the patent constraints that we had on this five-party interference, I couldn't talk about it to anybody. Well, Grubbs published an article in *Science* in which he incorrectly attributed the discovery of the reaction to Natta (2). He cited an article in the *Journal of the American Chemical Society* as evidence that Natta discovered the reaction (3). Well, the article had nothing to do with ring-opening polymerization. So I wrote Grubbs. I have a copy of the letter if you're interested. I said, you know, this is incorrect. I told him the circumstances of talking to Natta. A while later, Grubbs wrote me back and said, "I was wrong. I made some assumptions." He apologized. In fact, I didn't really remember the Grubbs apology until I was looking through some letters in preparation for this interview.

[END OF TAPE, SIDE 1]

ELEUTERIO: Schrock has never corrected the wrong attribution of where metathesis and ring-opening polymerization metathesis was discovered. So people read the literature and they quote him. Why go through all this? It's sort of ancient history. The reason is that there is a big interest in so-called knowledge creation and the knowledge management. People assume that the data is correct. When the data is incorrect, it can't be knowledge; it's misinformation. It happens in science as well as in other places. So because I teach a course in creativity and innovation and I've been lecturing along with Japanese professors on so-called knowledge management, I try to make the point that part of managing knowledge is making sure the knowledge has a sound database. I know, personally, a lot of examples where this is not true. This just happens to be one example that I was involved in where, clearly, it isn't true. Now, the reader has every right to make the assumption that what he's reading is correct. But other than scientists, not everybody really checks the data or can possibly check the data.

So, you know, there's a problem and I understand the difficulty. But glossing over real facts is not a solution to the problem. The other point that I think I would like to make has to do with what I call looking "beyond the horizon." There are very few leaders who really have foresight—even in science. You know some of them. Pauling obviously had a lot of foresight. But there are a number of people in industry that have foresight and never get credit for their foresight. One of these people was McGrew. Frank McGrew was one of Roger Adams'

students at Illinois and had a lot to do with the climate of Gresham's exploratory group. Since these are high-risk, low-probability success endeavors, you can get shot at very easily by the business types. It's relevant today because what I see happening in the chemical industry is people are using the expression 'targeted research'. There's nothing wrong with targeted research, but you may be trying to hit the wrong target and often it is a moving target.

One of the factors that's extremely important in innovation is degrees of freedom. Now, there are some management writers who simply talk about freedom and I think they're misinformed. What most scientists and researchers want is degrees of freedom. They want you to give them proper attention and respect for what they're doing. They don't want to go out in the wilderness and work unencumbered. They need support and they need people who understand them. That is, I think, one of the inhibiting factors about innovation today. A lot of people who are in charge of the funding don't really understand what the individuals are trying to accomplish. Now, it isn't a matter of short-range research, it's a matter of shortsighted research. There's a tremendous difference. You know, everybody knows bills come due and you have to find some way of paying them. Industry people have bills like everybody else has. So you have to do a certain amount of short-range research, but you ought to balance that with a percentage of longer-range research that you know is going to take longer to do, but the payoff may be much greater. The trends I see in a lot of industry in the United States and elsewhere is what I call shortsighted research.

TRAYNHAM: In that respect, do you have the feeling that current industry is significantly different from what it was when you were starting your career at DuPont?

ELEUTERIO: Well, it's a matter of focus and a matter of which industry. In the chemical industry, go back to [Wallace H.] Carothers' time. That was an issue of Carothers' time. The person who had the vision was Charles Stine. He was willing to support Carothers because he thought there would be a long-range opportunity. But when you read the history of Carothers, and particularly when you talk to some of the people, like 'Speed' Marvel, who were around at the time, there was a lot of pressure on Carothers to produce results. The kind of results he was producing were science results, and very important science results. In fact, if Carothers had been left to his own wishes, he would have worked on non-aromatic polyesters. He wasn't going to work on polyamides. The reason is that he could purify aliphatic polyesters and know their purity. He was trying to prove that Hermann Staudinger was right, that polymers were long chains and not a whole mish-mash of stuff. They were discrete chains. So he wanted to do very carefully designed experiments that in today's world look kind of simple, but he didn't have the tools to make them more complicated. So he was very realistic.

Nylon fiber was an example of serendipity. What happened is that people started to make polyamides and found out—in fact, they were horsing around at lunchtime. These polymers could stretch, and when they stretched they became stronger. Eventually that led to nylon fiber. Now, nobody, including Carothers, had fibers in mind. He had well-characterized molecules in mind that were a longer chain than usual. He never really switched from that view.

It was other people in his group that went beyond what Carothers was really interested in. But Stine supported Carothers. Unfortunately, Carothers was very dysfunctional as a person. He was a good scientist, but he was really dysfunctional. But he had some very good people working with him. For example, [Crawford] Greenewalt was instrumental in choosing the nylon that was commercialized, which was the 6-6. Carothers wanted 6-10 or 6-12—one of those. It would have been very difficult to make the higher diacids commercially feasible. It would have been expensive. It turns out, actually, that the 6-6 combination has probably the best combination of properties. So, you know, there are other people that had an influence on the success of nylon.

So one conclusion that I draw is that scientists need to communicate with entrepreneurs and others who have commercial interests. It's very unusual for the scientist himself to have all these attributes. In fact, most individuals that like to do pioneering work lose interest once they prove the concept. But it's extremely important to have people who can do the rest of the work. It's not work that's less valuable than the discovery, but it's a combination of things.

This is one of the points that I tried to make in the work that I did on fluorocarbons. I got curious about fluorocarbons from my work on polypropylene. The simple question that came to my mind is, supposing I replace hydrogen by fluorine, which is a larger atom. What will that fluorinated polypropylene look like? Much to my surprise, I found out by going through the literature that nobody had ever made high-molecular-weight [solid] polyhexafluoropropylene polymer.

TRAYNHAM: But at that time polytetrafluoroethylene had been prepared.

ELEUTERIO: Oh, yes. But polyhexafluoropropylene had never been prepared. In fact, the literature—if you believe the literature—said it would be impossible to make polyhexafluoropropylene. All the papers that were published were negative. People made attempts to make it, and then they couldn't make it. Then they rationalized why they couldn't make it. One of the things that I did early on, really with Harold Hart and to some extent Mel Newman, was get interested in models and molecular modeling. They were pretty primitive compared to today's molecular modeling. But, you know, I'd make models and look at them and try to figure out what they looked like in three dimensions.

So I made a model of polyhexafluoropropylene, and it looked like it was a very tight fit. But it looked like it ought to go together if you could make it stay together. One of the reasons that previous people hadn't been able to make polyhexafluoropropylene is it has a tendency to dimerize. It makes four-membered rings when you try to put heat to it under pressure. The other case where people tried catalysts, they all had a source of hydrogen or water, which will terminate the end group. So I got some specially prepared polyhexafluoropropylene that was ultra pure. I made some catalysts made that couldn't chain transfer to the polymer and terminate it. So I was able to make high molecular weight polyhexafluoropropylene for the first time. The interesting thing about polyhexafluoropropylene is that I made it and I reported it in one of

the weekly reports or monthly reports. Then I got a call from a British physicist who worked for DuPont at the time. He said, “Herb, I just read that you made polyhexafluoropropylene. It’s a rubber, right?” I said, “No, it’s not a rubber. It’s an extremely stiff polymer.” Oh, he was very upset. So he came over to my lab and he said, “No, no. The theory says that it’s got to be a rubber.” I showed him the evidence that I had that it was polyhexafluoropropylene and was extremely stiff. Well, what happened was that in the physics calculations they had used some wrong parameters. They guessed at them. They didn’t know what they would be. So they guessed at them from, in fact, polytetrafluoropropylene, which was the only model they had at the time. So they made a wrong guess about one of the variables.

It turns out that polyhexafluoropropylene is a very stiff polymer. People only have been able to make it at very high pressures, in spite of the fact that some of the Italians report that they’ve been able to make it with an organometallic catalyst. That’s not really true. Nobody can repeat that work. What polyhexafluoropropylene did for us is it finally gave us a way to figure out how much hexafluoropropylene was in some of our TFE/HFP (FEP)^R copolymers. Those have been commercial for a long time. But we were able to make HFP-rich TFE copolymers for the first time. They’re completely amorphous. Incidentally, polyhexafluoropropylene is also amorphous, but it’s very stiff. So for the first time, our people were able to get a standard to calibrate how much hexafluoropropylene was really going into these TFE/HFP copolymers. HFP-rich HFP/TFE copolymers is an area that I think still somebody is going to commercialize. It hasn’t been commercialized to date, mostly because of the high pressures that are required to make the copolymers.

But that got me square into fluoro polymers. One of my assigned objectives was to come out with a fluid that the Air Force was looking for in the early 1950s. Wright-Patterson had sent out this request for a proposal, you know, RFP [request for proposal]. But we never took any of their money because Crawford Greenewalt had been stung by the comments that were made about DuPont being the devil’s disciple—DuPont made ammunition for the military. He didn’t want government money. If it was worth doing, it was worth doing with private money. So we never got a dime from the government for this work. Well, my assignment was to come up with a fluid that would remain liquid from minus 60 to plus God-knows-what—a very extreme temperature range. There were no fluids of that type, but that was what the Air Force wanted. So I, along with some co-workers, tried to make completely fluorocarbon materials with no hydrogen in it. Because again, Gresham, the research manager, said, “That’s a weak spot. We don’t want any weak spots. It’s got to be completely carbon and fluorine.” We tried and tried. We could get some materials that worked at high temperature, but they would turn solid at low temperature. I was pretty frustrated, along with the chemist that was trying to do this.

One night, I was reading abstracts of an ACS [American Chemical Society] meeting from Florida. They reported they had made the fluorinated diethyl ether. It was thermally stable to 800 degrees or some real high temperature. Frank hadn’t been mandated that you couldn’t put oxygen in a backbone of a polymer. So I thought, “Okay. Well, how can I make ethers?” I looked at several routes, including fluorination and oxidation, and I made my choice strictly on the basis of safety. On that basis and knowing what I did about the susceptibility for

nucleophilic attack on perfluoro olefins, I thought, “Well, we have a shot at using alkaline hydrogen peroxide.” Because the OOH ion ought to attack the olefin since it badly wants some electrons.

So I proposed this as a project and it got turned down by a committee. The reason it got turned down was not really based on chemistry but because nobody had done it before. There was one guess that you’d end up with carbonyl fluoride. Because again, some of our academic consultants had arbitrarily predicted that such epoxides, perfluorinated epoxides, wouldn’t be stable. You might be able to trap them at very low temperatures, but you’d never be able to do anything with them. But much of that was really rationalization for failed experiments. Well, I got turned down. But by then I was a supervisor and had people reporting to me. We had always encouraged them to take a percentage of their time and do unassigned experiments. I would keep it quiet until we had something to say and then we’d talk about it. Both the supervisors at the time, Max Robinson and Art Anderson would look the other way. They knew what we were doing. As long as you worked on your assigned project, you could spend a part of your time doing non-assigned projects that were related to departmental business objectives.

So I convinced one of the people reporting to me by the name of Bob [Robert] Meschke, who was a very good laboratorian—he had actually gotten a Ph.D. at a pharmacy school. He was very good in the lab, but he wasn’t good in theory. But he knew that I had submitted a proposal and got turned down. I twisted his arm enough, in a nice way, to try one or two experiments. If it didn’t work, you know, we’d just chuck it. The first experiment he ran—we talked about it very carefully and made sure it was going to be done absolutely safely. The first experiment he ran, he trapped the off gases. He got mostly un-reacted perfluoropropylene. But he had one blip that looked like we might have made a perfluorinated epoxide. So we wanted to repeat the experiment. The second experiment he ran, he blew the whole apparatus to smithereens! So I had to tell Frank Gresham, the section manager.

TRAYNHAM: That must have blown your secret cover.

ELEUTERIO: Yes. So I had to confess to Frank what I was doing. He didn’t react like I thought he might react. He said, “Okay, go over and get stuff over to Carney’s Point and see if you can figure out what happens.” Carney’s Point was the explosives hazard lab. So we ran a bunch of experiments over there. We couldn’t get anything to blow up. So we then got set to make preparative amounts of the HFPO—hexafluoropropylene oxide. I chose hexafluoropropylene oxide rather than tetrafluoroethylene because tetrafluoroethylene is really pretty tricky to handle. In fact, [Roy J.] Plunkett was lucky that he didn’t blow himself up along with his technician when they first serendipitously made Teflon. If he had had a slightly larger cylinder with a little bit more air in it, he would have been a different kind of history. So he was fortunate that the cylinder was the size that it was.

I’m telling you this story because serendipity plays a fairly large role in a lot of discoveries, but it’s not merely a matter of luck or chance. I like to say that serendipity is like a

lightning rod. You can either attract or repel lightning. A lot of management practices repel lightning. Now, you want to control lightning—you don't want arbitrary lightning. Assignments are so focused that researchers don't have any degrees of freedom to follow interesting leads. They're supposed to produce results on cue. I think that's one of the biggest contributors to why a lot of people aren't more creative. You have to have a fair amount of self-confidence and you have to have an understanding management in order to do this kind of research. But if there's one message that I'd like to convey to the present managers, it is to give your people more degrees of freedom. I'm not saying freedom—I repeat that—but degrees of freedom. Anybody who does physical science knows that even in nature, there are degrees of freedom.

That's part of why nature is the way nature is. People need degrees of freedom. If you treat people like adults, they will behave like adults. If you treat them like children, they'll behave like children. But that can, again, be why you're not getting as much out of potentially innovative people as you should. One of the greatest satisfactions I've had in teaching in the last seven years, and one of the greatest satisfactions I had as a research director, is when somebody came up to me and said, "Herb, I didn't think I could do that." They don't call me Herb in Singapore—they call me Prof. E. I've had students come up to me and say, "Prof. E, I didn't think I could do that." I think that's really the ultimate reward that any teacher can get. It's very satisfying and I still love it. The HFPO story is also interesting for another reason. I document this in the Carothers Award lecture. We were looking to make fluids that were stable over a wide temperature range. We made them. They became a commercial product. They were used in satellites. They're used in nuclear reactors. They're used in a lot of places where you need very thermally stable materials.

Having made a simple molecule like hexafluoropropylene oxide, that has enabled lots of other people to do some very interesting chemistry that I never anticipated. Some of that chemistry turns out to be Kalrez elastomers: precursors to modern-generation Teflons that have much better properties than Teflon. Nafion membranes now look like they might have a real use in electric-driven automobiles. Nafion membranes are used in chlorine caustic manufacture. They replace mercury cells. Again, the message is that I think most chemists would like to produce what I call a Christmas tree—the top is a rather simple molecule like hexafluoropropylene epoxide that gives rise to a lot of branches.

The important message is that it takes a lot of people to make this happen. It takes not only pioneers, but it takes people with the patience to see that these products get through a whole bunch of hurdles into manufacturing. It takes people in manufacturing to make sure the process works. It takes marketing people and it takes real leaders with the foresight to take chances. HFPO, hexafluoropropylene epoxide, is a reality because Frank McGrew had the courage to defy the polychemicals department's general manager. The general manager didn't want any more long-range research and McGrew supported us. It actually cost him his job. He got transferred to Geneva, where he didn't want to go. But it was a personal sacrifice that McGrew made.

TRAYNHAM: Those unwanted consequences came to him even though the forbidden research produced money for the company?

ELEUTERIO: Yes. Because a lot of general managers, their attention span ends within the quarter or within the year. Many of them are more concerned about what this year's results are, what this quarter's results are, because they're driven by stock options. That's one of the reasons why I see more and more pressure on people to develop short-term results. Now, you must do some short-term, short-range research. But you should give researchers some room for longer-range, higher-risk type of research. Not only can it lead into very profitable areas that you can't really foresee, it also can give you some intelligence about what your competitors might be doing. It's not uncommon to have companies come to you, particularly the Japanese, and say, "I've done such-and-such. I want to license this technology. It will cost you a hundred thousand dollars to take a look." That hundred thousand dollars has now gone up to about a million bucks. Even universities. Professors will do something very interesting, but their universities have hired a lot of patent lawyers. They come and they ask for really unreasonable amounts of money. I'm absolutely for people sharing in whatever monetary rewards there are; if and when real money is generated.

But before you really know you have a golden goose, you'd better be sure you have a goose. That's not happening in a lot of places. You shouldn't be unfair, you know. People make contributions—you ought to make sure they get their fair share of rewards. But there's a real change happening, even in universities in the United States, where there's a tremendous amount of pressure on the faculty to produce money. I think that really takes away from being a good scientist. I know profs in the past who refused to take government money because they knew in their hearts that wasn't their intention to do what the government was going to give them the money for. There are a tremendous number of ethical people in science, like Andy Streitwieser. Anything he earns for consulting in industry, he supports his students with the money.

So, you know, I have a great deal of respect for much of the academic community. Unfortunately, like everything else, the rotten apples are the ones that get the publicity. That, I think, is something the American Chemical Society can do something about. They're reluctant to do it. But I don't think they take a tough enough stand on people who are really selfish and not interested in chemistry as a profession. I worry that there are a lot of bright people being turned off by chemistry. That's one of the reasons why I've been teaching for the last seven years. You might say, "Well, why did you go to Singapore? Why didn't you stay in the United States?" That's a good question. One reason why is that I had been writing, before I retired from DuPont, and lecturing about creativity and innovation. I felt I knew how creative people think in the West and solve problems. I wanted to see if it was also true for an Asian community. Incidentally, when I retired from DuPont, I was offered a chair at a "Big Ten" school to teach engineers creativity. I turned it down because they wanted me to sign a five-year contract and I didn't want to. I didn't know whether I was going to live five years or not. But I mostly turned it down because I was really curious to see—I'd read a lot about China and Asia—if there was a difference between the Asian mind and the Western mind. I've answered

that question for myself. I'm trying to write a book about creative expression. I went to Singapore to write a book on creativity. I worked on it for about a year and a half and then somebody in Britain published one exactly like the one I was doing. So I started again and I started three times. This time, what I've decided is not to write a book on creativity, because I think there are a number of great books on creativity. One of the things I like about the National University of Singapore is they have fantastic libraries. They have great libraries and professional libraries. It has been a real pleasure working with these folks. So I'm writing a book called *A Guide to Creative Expression*. I've changed the emphasis again, because I was writing a book that really didn't involve me as a centerpiece. The publisher wants me to do something similar to what I'm doing with you. He wants to make it more anecdotal and sort of autobiographical. I think I'm convinced now that's probably worthwhile, but I wasn't convinced a year ago.

So I'm going back to Singapore and that's one of the things I'm going to concentrate on. The other thing I've been doing in Singapore is I've been a technical advisor to some of the institutes over there. That's been fun. It's been interesting. So I've gotten to know a lot about the Asians. I started out by getting offers from Japanese universities. But after spending a couple of weeks there, I found out that the language skills that I'm familiar with—especially English—a lot of the Japanese are very limited in that language, at the level that I want to do. I thought, well, you know, you're sixty-five years old. I don't know how long God's going to grace you with life. If you spend five years, then you can talk and communicate at the fourth-grade level. So in Singapore, English is the language at the universities. It's the language of business. The professors speak it very well. Most of them have a British education under their belt, although more and more of them are coming to the United States. They're hard working. They're serious.

This probably will sound funny to you. But a lot of the Chinese Singaporeans remind me of Texans. They're "can-do" people. It's an island. It's a small island. People keep trying to find answers why they've been so successful. What I tell I people, I say, "Well, they've got the same psychology the Texans have. They believe they can do anything they put their mind to. Sometimes this comes out as being arrogant, but you've got to look at the track record. In fact, they are 'can-do' people." I looked at some other places. The last two years that I was with DuPont, I had an open-ended assignment. My group vice president came in one day and he said, "Ed [Edgar S.] Woolard—who was the CEO—wants me to globalize R&D. I don't know what the hell that means. You find out what it means and go do it." So I decided that probably the best thing I could do would be to go to those countries that DuPont was putting major investments in and get to know their research institutes and their research universities, so we could hire the best people. Make contacts with these people and try to exchange staff. So that's why I chose Singapore. I looked at Russia, looked at Spain, looked at Australia, looked at Japan. They didn't really have the spirit that the Singaporeans have. It's been a great experience. It's been hard work, but a fun experience. I've enjoyed it. My wife has indulged me. She's very homesick for the United States. I have to tell you, I'm not homesick, but I wouldn't want to live in Singapore forever. But I have enjoyed very much the time I have been there. But I've also been married a long time and I've always tried to be fair to my wife. So

even though I've been in Singapore full-time the last few years, I'm only going to be there part-time this year in order to try to wrap up this book.

TRAYNHAM: By part-time do you mean half-time?

ELEUTERIO: No. I'll be there four or five months. I'm also working here on what I'm doing.

TRAYNHAM: I believe, at least for some of the year, you've been there for nearly eight years now?

ELEUTERIO: Seven now.

TRAYNHAM: Seven years?

ELEUTERIO: This is my eighth year.

TRAYNHAM: Part of that time, at least, your wife was also on appointment in Singapore?

ELEUTERIO: Well, yes. She was a genetics professor at West Chester [University]. She retired two or three years ago. But then she took some leave of absence, and then she took summers and joined me. She's been doing some interesting research for the Institute of Molecular and Cell Biology on tuberculosis.

TRAYNHAM: This is in Singapore?

ELEUTERIO: In Singapore. Right now, she's doing research via e-mail with the group she's been working with in Singapore. We've been home since the first of December. She's written one paper on tuberculosis, which is a review article. So she's a very active person. We've been lucky because we've still got all our marbles and are reasonably healthy. She has some serious eye problems and she had a heart attack a couple of years ago while lecturing. But she survived that. She's a tough lady.

TRAYNHAM: During the seven years, then, she was with you in Singapore the whole time, or was it a long-range commuter marriage?

ELEUTERIO: Well, at first it was a long-range commuter marriage. Then she's been with me the last two and a half years full-time, after she retired. But she worked with the Institute of Molecular and Cell Biology even part-time while she was there. We have six children. She misses them. You know, she's a good mother. I like Chinese food. Some of the food they put in front of you is not identifiable. Although she does like American Chinese food. Not chow mein, but the mainland Chinese say Singaporeans don't make Chinese food like they do. We spent some time in China and it's true, they're different. It's just like, you know, Louisiana food is different than New England food.

TRAYNHAM: Yes.

ELEUTERIO: We've had a good life. I'm very grateful to chemistry. I wish that more bright young people would major in chemistry. Chemistry has a challenge. The challenge is—chemistry is an enabling discipline. As you know, the thrust toward biology is very dramatic. But I don't know why people stopped calling it biochemistry. That's a mystery to me. It's chemistry. I like to tease my wife, who's a biologist. I say, you know, all biology can be reduced to chemistry—which is not really true. But it rings her bell a little bit.

But you know, the things that people are doing in chemistry, I think, are fantastic. They really are. I mean, I read chemistry. I read a fair amount of biology. I get free lectures at dinnertime from my wife on genetics and molecular biology. But she's good at it because she was a very good chemistry student. She didn't take the easy road. Although she was a biology major and she could have taken the easy chemistry. She took the hard chemistry. That's why, after seventeen years, she could go to graduate school. She really knows the basics, and those have served her well for so many years. You know, there's a tremendous amount of stuff that's new, but the basics are still there. You've got to know kinetics. You've got to know thermodynamics. The physical laws have been modified a little bit, but those are usually in regimes—very special regimes. For example, I learned as an undergraduate that you can't violate the second law of thermodynamics, but that's not completely true. That's true in closed systems. If you have an open system you cannot only violate it, you can't work without violating it. But, you know, that's what makes learning fun.

[END OF TAPE, SIDE 2]

ELEUTERIO: I'd just like to make a couple of points—I don't like to hog all this time.

TRAYNHAM: It's for you.

ELEUTERIO: But I had some things to say for chemists-to-be, frankly.

TRAYNHAM: Yes.

ELEUTERIO: I gave a talk to senior professionals—the DuPont departmental fellows—in 1987, in which I tried to put a lot of my thoughts together under the banner of “Toward a Better Understanding of the Process of Scientific Discovery and Technological Innovation.” The abstract reads, “Advances in science and technology depend on human creativity. Cognitive and non-cognitive aspects of the scientific discovery and technological innovation process are discussed from an industrial research management perspective. It is concluded that the individual and institutional factors which foster human creativity in the basic and applied sciences are similar in kind, different only in degree of relative importance” (4). The reason I put this together is that I was trying to think through my own experiences in creativity and innovation that were they really unique—what could I pass on to people. I gave essentially this talk to some three thousand, thirty-five hundred, maybe four thousand people over a period of time. One of the things that I wanted to do in Singapore was to figure out if I’d missed anything.

So during this process, I had contact with Herb [Herbert A.] Simon at Carnegie Mellon [University]. He was kind enough to encourage me to keep going. In fact, he wanted me to either do a sabbatical or go work with him after I retired. He sent me some references. Herb Simon belongs to the Information Processing School of Creativity. I think it’s fair to say that at one time, he believed that creativity is a very rational process and it can be reduced to algorithms. He didn’t really buy into intuition. He certainly doesn’t buy into the idea that it takes geniuses to be creative. So he has some very definite views. He and his students tried to reduce creative events into what I call expert systems, where one of his students was able to discover some of the basic laws of nature, e.g., Snell’s law of refraction and that sort of thing. That’s a good exercise, but it makes a lot of assumptions about the problem. So it’s an example of syllogistic thinking and reasoning, where if I carefully define the assumptions, I can make the rest of the conclusions seem pretty logical. I know that’s one kind of discovery, but it’s not all kinds of discovery.

TRAYNHAM: Does he buy into the role of serendipity as you have discussed it?

ELEUTERIO: Well, I sent him a copy of this paper and he wrote back. He didn’t write that he disagreed. He just thought I should look into the subject further and asked if I wanted to look into it further with him. He’s since, in the ten years that have transpired, I think he’s softened up a bit. Even during this ten-year period in Singapore, I found some material that he wrote that wasn’t as cut-and-dried as what he said in a *PNAS* paper. But he had this tremendous debate with the University of Chicago School of Psychology because they buy into serendipity and

they buy into a much more human-based view than just an information processing view. More recently, I've gotten a chance to interact with a Japanese professor by the name of [Ijurio] Nonaka. He claims that Japan's strength is that the Japanese look at problem solving completely different from the Herb Simon point of view. The people at Chicago were by the name of Goetzels and [Mihaly] Csikszentmihalyi, and they studied artists and musicians and not scientists. In all fairness to Herb, he looked at scientists who were doing basic research. If you limit scientists who do basic research, much of what Simon says about information processing holds—except it leaves out for serendipity.

But at any rate, he published a three-page article in the *Proceedings of the National Academy of Sciences*, in which he enumerates characteristics of creative physical scientists (5). I agree with him, except he leaves some things out. He also doesn't talk about institutional creativity and innovation—institutional factors that can inhibit creativity in particular or can enhance creativity. So I've broadened this approach to include individual and institutional factors. In doing that, I got to correspond with Joshua Lederberg. He's been very kind to me, and set me on a track of a lot of material that he published about physical scientists and how they do innovative things. So I've expanded my viewpoint quite a bit in the last seven years. Lederberg wrote an article in *Nature* on the anniversary of his discoveries of bacteria and how they propagate and transmit genes and whatnot (6). A sociologist by the name of [Harriet A.] Zuckerman was a co-author of that paper. There were actually two papers. They talk about institutional factors. Zuckerman is a Brooklynite who taught at Columbia [University], but she has retained some of the ability of being very down-to-earth on what she thinks or doesn't think. So I've combined a number of these views, and this is what I want to do with this book—try to introduce these concepts to students.

My present activities focus on knowledge management and knowledge creation. What I've chosen as the context is material science. So this is the 1988 paper, and this is more recent work that I've been doing. What I've done is I've used the discoveries on olefin polymerization and catalysis and used them as a way of discussing what Professor Nonaka alleges. Nonaka makes the claim that the Japanese companies have been successful because of their skills and expertise at organizational knowledge creation. The centerpiece of the Japanese approach is that creating new knowledge is not simply a matter of processing—this is the barb at Herb Simon—objective information. Rather, it depends on tapping the tacit and often highly subjective insights, intuitions, and hunches of individual employees and making those insights available for testing by use of the company as a whole.

Well, what I've done is I've reconstructed the invention of Ziegler catalysis and the various turns that it's taken and compared that process to what Nonaka alleges. I conclude, as you might expect, in a couple of paragraphs. I use a *Harvard Business Review* article written by Nonaka (7). I say, "These quotes apply equally well to the situation that the Himont polypropylene joint venture found itself in while attempting to commercialize a new generation of catalysts. The Himont story is described in detail in the book that was mentioned earlier, entitled *Breakthroughs!* (8). Japanese knowledge-intensive industries create materials knowledge employing strategies which are similar to those discussed in this *Breakthroughs!* book. Japan has been unusually successful in the materials arena as continuous innovators.

Their success, however, can not be due simply to Japan's management style and its knowledge creating companies" (9).

Now Nonaka gets nationalistic in my view. But it's a little bit of a 'Japan über alles' kind of thing that the Germans did before World War II. "Finally, are there significant identifiable differences between the way the Japanese companies create knowledge and the rest of the world's multi-national companies. Except for cultural idiosyncrasies, and there are cultural idiosyncrasies, those multi-nationals whose leadership, corporate policies and management practices show that they nurture shared beliefs and values bring out self-imposed and group discipline. Using the will of everyone in the organization to succeed, those multi-nationals perform just as well in creating useful knowledge as the best Japanese company." This is one of the interests I've had. I've talked about several things. I wasn't sure whether you were interested really in just DuPont or broader topics.

TRAYNHAM: Well, the DuPont experience is the focus, but these other aspects of your career are very important, too.

ELEUTERIO: Well, one of the things that I feel good about is what we've done at the University of South Carolina in Aiken. We've got a science education center [Ruth Patrick Science Education Center] down there.

TRAYNHAM: This is a DuPont activity?

ELEUTERIO: Well, yes and no. Sometimes more and sometimes less. But what happened was I was asked to give a talk to the superintendents of schools in 1985 in Washington, DC. What surprised me is that all the attendees complained about the high-school physics teacher not being able to do the problems at the end of the textbook because they had degrees in home economics. This was turning off the kids. At one point in my career, I was the technical director of the Atomic Energy Division. I was based in Wilmington, but most of our activities were in Aiken, South Carolina. We had terrific guys at the laboratory. They were great nuclear physicists, mathematicians—top rate, absolutely top rate—most loyal DuPonters I've ever run into. But their product was tritium and plutonium, which is not very acceptable in some quarters. But they were great people. And they had bright kids. I thought if we're going to continue to attract this caliber of people, we've got to make sure their children don't have a second-rate education. These are first-rate employees.

So I wrote to the chancellor of the University of South Carolina at Aiken. I said, "Look, we've got all these guys down here who can help your teachers. I'm willing to give them time off to do that." That started the ball rolling. He agreed. He had some reservations, but he agreed. Then one of the guys in my group by the name of Richard Pryor, whose wife taught at the University of South Carolina at Aiken, said, "You know, this is a great idea. We ought to

try to get an exploratorium kind of thing in South Carolina.” I said, “Rich, the last thing we want is to entertain these kids. What we want to do is help the teachers so they don’t turn off these kids. Entertainment can come later.” So I said, “Let’s try to get some NSF [National Science Foundation] money and see if we can’t get something started.” Well, much to my surprise, some of the local people—my own people that reported to me—said, “Why in the hell should NSF give a dinky little place like Aiken money?” I didn’t listen to them; I went forward.

To make a long story short, we did apply for NSF money. We got turned down five-zip by the NSF reviewing committee. The reason they gave was, “Well, what you propose to do doesn’t look like it’s going to be exportable. We want something that’s exportable.” Do you know [Bassam Z.] Shakhashiri at the University of Wisconsin? He’s a chemistry prof out there. Shakhashiri was doing a stint at NSF. He said, “Look, I’ve got some discretionary money. I like your idea, but I can’t convince anybody. But I’ll give you some discretionary money. My suggestion is, hire a director with this money and get him to put some more meat on these bones and let’s see what happens.” Well, one thing led to another and we did get a good grant from NSF. Then I had trouble with my own management. My management, much to my surprise, said, “Well, if this is such a good thing to do, why the hell don’t you do it in Pennsylvania or Wilmington? Why the hell do you want to do it in the south?” I said, “Well, that’s where the opportunity is.”

So one thing led to another. That’s why I answered your original question “more or less.” I found some ways of not involving my management. DuPont’s a big, big place. If you have a will, there’s usually a way. I had met Crawford Greenewalt before; I found out that Ruth Patrick was a limnologist. She’s one of the first women to be elected to the National Academy of Sciences, I believe. Anyway, Ruth is an interesting lady. She’s a tiny lady with the most sparkling blue eyes and lots and lots of energy. Well, Crawford Greenewalt—before environmentalism was even a word—was worried that DuPont might pollute some of the waters where our plants are located. So he had Ruth do a study, which he funded out of his own money. Ruth should make sure we weren’t screwing up the rivers: the Guadeloupe River in Texas and Savannah River in South Carolina.

So Ruth had a longstanding relationship with DuPont in the environmental area. So we got some money from Crawford Greenewalt, and then we got some matching money from DuPont because we had gotten Crawford Greenewalt’s money. Then I got my bosses to cave in and give some more DuPont money. Anyway, this fifty-thousand-dollar risk that Shakhashiri took eventually has blossomed into a five-million-dollar complex down there. I feel really good about that because DuPont has been a very good citizen in the nuclear area. Again, Crawford Greenewalt insisted that DuPont make no money from the Atomic Energy Division. DuPont got one dollar that they took on this contract. We left Savannah River, and Westinghouse [Electric Company] took it over in 1989 because the Seagram [Company] people unfortunately read *The New York Times* all the time. *The New York Times* was after the nuclear facilities and included everybody. I mean, there were some things that were being done wrong by other parts of the complex, but they weren’t being done wrong by Savannah River. But the media included us in their articles.

So the first thing in the morning, Dick [Richard E.] Heckert, who was the CEO of DuPont at the time, would get a phone call from an early-rising member of Seagram's management asking, "Why the hell are you guys doing this? You're not making any money for DuPont"—on and on. "Why don't you get the hell out of it?" So we got out of that contract in 1989. I came back to commercial DuPont. I was there from 1985 to 1989. I wanted to retire when I was sixty-five, in 1992, and had made all my plans to do it. That's when I got this assignment of globalizing DuPont R&D. I learned a lot and I think we did DuPont some good. The other activity I spent some time with was with the chemical research organization that involves all the heads of departments of the chemistry and chemical engineering departments in the United States—Council of Chemical Research [CCR]. I don't know if you're familiar with that.

TRAYNHAM: Yes.

ELEUTERIO: I was on the Board of Directors and actually ran the Executive Committee for a year after Judd King ran it. I worked very closely with [Paul] Gassman because he was one of the people who was very involved. I think we did some good with CCR. So I was involved in that. I don't know how much of this stuff you want. I've got some material here on the National University of Singapore, and I have a copy of what I've been doing there. This is a letter I wrote to Joe [Joseph A.] Miller that I thought might be interesting to you. Because one of the points I'd like to make is that it bothers me about granting awards. It bothers me all the people you leave out. So Miller asked me to recommend—Joe Miller's a chief-technology officer at DuPont—to recommend some candidates for the Lavoisier Medal. You know, whenever you give somebody a medal, you make ten people mad. So I thought that the medal was really an anachronism, where teamwork in this day and age is really stressed.

There are some people that I thought didn't get any rewards. One of them was a vice president, who was a chemical engineer who grew up in Kentucky—great guy—[Everett] Yelton. He was really responsible for keeping the nylon business healthy. He did it because he had the foresight to look beyond the horizon. He would support people. He would make sure that the best people had the best equipment that he could possibly buy for them. He never got any credit for his foresight. I tried to get him an award from the American Institute of Chemical Engineers [AIChE]. My first problem was that he never wrote a c.v. [curriculum vitae]. He never intended to leave DuPont. So I had to scrounge and scramble to get people to tell me what he'd done. I knew what he did when I worked for him. I'm not a member of the AIChE, but I did get a high-up member of AIChE to sponsor him. He didn't get the award. He died, unfortunately. He didn't get the award because there was a group of people on the AIChE committee at that time who were anti-management. So they didn't want to give anybody in management an award. I thought that was sad. I still feel that way. But, you know, if you're going to give awards, for heaven's sakes, at least try to give them to people who deserve them. What I found over the years to be true—and I've been on award committees not only here but in Singapore, as a matter of fact—is what Harriet Zuckerman said, "Them that has, gets," okay. She's paraphrasing a quote from the Bible.

TRAYNHAM: Yes.

ELEUTERIO: I just want to end on that note. I don't know how to change it. The only thing that I know is, if I'm given an opportunity to nominate people, I try to nominate people who really deserve it and not people who politic for it. I find it incredible the amount of politicking that goes on—even for the Nobel Prize. You know, Charlie [Charles J.] Pederson got the award, along with Cram and [Jean-Marie] Lehn, for complex ethers. Lehn, the Frenchman, was trying to wait until Charlie died to get France to agree to the award. Because they didn't want to share the award with anybody from industry. The reason I know this is an absolutely marvelous guy—I don't know if you know Herman [E.] Schroeder or not. Herman was a research director at DuPont. Well, Charlie got leukemia and he couldn't go to Japan. So Herman went in his stead to give the talk at one of the Japanese meetings. After a bunch of sakes, Herman told one of his hosts, "You know, Charlie had a Japanese mother." The conversation stopped. They said, "Oh?" He then told them about Charlie's background. His father was Scandinavian and his mother was Japanese. So they talked about the tradeoffs that go on between countries on who they'll nominate this year in physics and next year in chemistry. The only thing that was said—translated—was: "We will take care of that." Sure enough, Charlie got the award the next year. Now, if you haven't read *Scientific Elite* by Harriet Zuckerman (10)—it's a twenty- to twenty-five-year-old book, I commend it to you sometime. Zuckerman interviewed a whole bunch of Nobel Prize winners. But interviewed them from the point of view of human beings with all the warts and all. There's this understanding that these are very real people with very real faults, very real pluses, and some minuses—and they're not all minuses. If you read some of the literature, they'll pick somebody who's had mental problems and write him up as if he's typical of everybody that does good science. You know, that's just not right.

You know, creative people are nerdy or they're nutty or something. But the kids pick up this stuff. That's why I'm telling you this. You know, the kids pick this up. The teachers pass some of this stuff on—not professors, but teachers at grade levels where the kids are easily influenced. They think, you know, the guy on TV who does all these crazy experiments that blow up or turn Frankenstein, you know. I hope we stop that. It's terrible. It's not fair.

I've passed on material that I thought you would want—I copied this to give you. I got that out of *Science*. Then there's a letter I wrote to Grubbs. You can do whatever you like with it. This is the reply I got from Grubbs. Candidly, I didn't remember that until I was looking through my files. You know, we made a lot of right guesses on metathesis and they were guesses. But chemistry advances on the basis of advances in instrumentation—theory is good, but there's nothing like direct observation. This is one of the papers that doesn't get cited very often, where [Jackie] Kress finally pinned down all of these damn things that we couldn't possibly pin down because we didn't have a powerful enough NMR [nuclear magnetic resonance machine] (11). It's a beautiful piece of work. I don't know whether Kress ever got recognized or not.

You know, one other activity that might be relevant. One of the things that makes me feel good about Singapore is that I started what was called the PAO Enterprise. PAO Enterprise stands for Process Analysis and Optimization Enterprise. The purpose is very straightforward. In Singapore, University people only talked to multi-nationals at cocktail parties. The multi-nationals will send somebody who's sociable but not knowledgeable, necessarily, about what's going on. This is crazy. Why don't we get professors to talk to people who have real problems and see if we can't get the university some money and some contacts? You start out the relationship on a generic basis. Then, eventually, if there's a consultant arrangement, the profs and the company can work out the details.

PAO eventually developed into CPEC, the Center for Process Engineering. It's being funded now not only by the university, but by industry. CPEC has ninety to one hundred and ten members. That's fun, you know. It's satisfying to build an organization. But the one lesson you have to remember is you've got to stay backstage when you're a foreigner. That's no problem.

I teach two courses. This is just the outline of one of the courses I teach in managing R&D. What I do, Jim, is different because I'm teaching people who are working for M.B.A. degrees or M.Sc. in technology degrees. The M.Sc.-in-technology students are all engineers. The M.B.A.-degree students are computer science majors and finance majors. So I tried to find a way that I can really give them an R&D experience. So I structured this course so the student becomes the case study. The purpose is: how can I do a better job in R&D? How can I understand R&D people? So we go through a series of open-ended exercises, which is the first time the students ever had an open-ended problem assigned to them, in most cases. They get very angry and upset because they always want to know: what is the question? Is it going to be on the final? What am I expected to do? I tell them, "Well, you're going to learn as you go along: how to define the problem and then you're going to redefine it. There are going to be periods of time when you're really in the dark. That's what every bona fide research guy goes through. He's in a tunnel and he's trying to find his way out. There are a few clues and some things he learns that he or she shouldn't do, but he really doesn't know how to get through that tunnel until, all of a sudden, there's daylight." I try to make the students understand that a lot of people that are working for them go through this tunnel regularly. You can beat them mentally and you can do all kinds of stressful things to them, but that's not going to help. So you've got to have confidence in the individual and encourage him or her the best way you know how. So they go through this experience and they write the exercise up. What have I learned about myself? What have I learned about managing others?

Now, I also give them a more traditional, structured kind of lecture. But I don't use textbooks. I might use one chapter out of a textbook. I use the original literature. I try to make them go to the library and dig some of the material out. What I found out about a lot of young people, even hard-working Singaporeans, is they don't like to dig the stuff out. They want somebody to do it for them. I resist that so that they can learn something about themselves. They learn something about the real world and solving open-ended problems.

I have not ever been in a position where I had to publish or perish. So I publish when there's a real good reason for publishing—if somebody wants me to do it. I have brought some copies of these things. I've given a lot of talks. Some of the material I've published and some of it I haven't. I've done a lot of things. I gave this at an ACS [American Chemical Society] meeting (12). This is a summary of stereospecific polymerization with some datelines on it. Professor Willy [William] Herman from Munich put together a series of mostly Germans who had made contributions to organic chemistry, and I filled in the rest, including DuPont and other people who had contributed. This is a cartoon that I made about the discovery of olefin metathesis position. The guy who's berating me is Jack Roberts.

TRAYNHAM: I recognized him.

ELEUTERIO: Somebody gave me a copy of this book on olefin metathesis (13). Let's see. I don't think you really need this. But this is a paper written by an Italian who worked at Montecatini on ring-opening polymerization, where he says that without a doubt, DuPont discovered olefin metathesis (14). Well, me, if you want to be specific.

TRAYNHAM: Yes.

ELEUTERIO: But again, that never seems to be cited. This is a paper summarizing the perfluoro epoxide work (15). Let's see. These things will help you on the metathesis and fluoropolymer research.

TRAYNHAM: Well, since we're back to the DuPont work and your career there, I have a question to ask. You've identified two major areas of your contributions to the DuPont story in polymers—olefin metathesis and the hexafluoropropylene polymers. The latter one was commercialized by DuPont to make some of the wide temperature-stable polymers. DuPont did not develop the former, but it was commercialized by other companies. What's your feeling, I wonder, about your breakthrough with the olefin metathesis that DuPont decided not to develop?

ELEUTERIO: Well, you know, you win some, you lose some. I thought we missed an opportunity. We did license some of that technology. Then oil prices changed and changed the availability of cyclopentene and they decided not to go forward. But my feeling—I'd like to have seen it commercialized. We did play a role, however, in the renaissance of metallo-organic chemistry, in particular. This is one thing that I find missing from a lot of history, you know. I've read [David A.] Hounshell's book about DuPont (16). The authors leave out a lot of the context that is true of the scientific community at the time. I've always been a great believer in what I call the community of practitioners, which is now called community of practice. It's

been redefined. The community of practitioners is a group of people that almost spontaneously get together because they have a common interest. You've had it in your area of interest, in catalysis. You only are a member of this group if the group accepts you.

At the time that olefin polymerization was pioneered, there were a lot of misconceptions about organo-metallic research. In fact, you know, the only reference I could find to aluminum alkyls when we first started to try to make aluminum alkyls was a paper, I think by Pitzer, with a description of some of their physical constants, but nothing of how he prepared the material (17). At the time, the only person who was really into organo-metallic chemistry in the United States was [Henry] Gilman at Iowa. There was very little known, really, about organo-metallics. Information was limited to Grignard reagents. I read a book by [Francis Harry Compton] Crick one time, and he had a great line in his book (18). An artist friend of his said, "It's a great feeling to be there when the picture is being painted." So that's the way I feel about olefin metathesis. I saw history being made. There's a lot of revisionist history that gets published. You just have to be realistic about it. You're not going to change the world. I'm just glad that I was a part of it. I've had a great career, absolutely great career. There are very few things that I would do different. It's just a circumstance beyond your control.

One of the important things about creativity is that you need the opportunity to use it. A lot of people don't have the opportunity for a whole variety of reasons, some of which we can identify. All I can say is I'm very grateful for having the opportunity of working with the people I've worked with—of being there while the picture was being painted. The other little bit of irony is that the German and Japanese scientific communities know more than a lot of DuPonters about what I did. Part of the reason is that I never felt the need to take credit for things. I get annoyed when somebody else gets the credit, but I don't care about getting the credit. But I do get annoyed when DuPont doesn't get the credit, because DuPont put up the hard cash. A lot of the people who are responsible for fighting for the budgets are never identified. They're never recognized.

[END OF TAPE, SIDE 3]

ELEUTERIO: One area we haven't touched on is the environmental area. I've done a fair amount of work in the environmental area. Incidentally, this is a German publication that talks about hexafluoropropylene epoxide (19).

TRAYNHAM: Is your concern with environmental issues part of your role as supervisor or manager at DuPont?

ELEUTERIO: Yes. DuPont's very safety conscious, but they're also very environmentally conscious. Frankly, I take pride in the fact that nobody ever got seriously hurt while working in any of my groups. I think one of the things that I appreciated about Savannah River was the

attention to safety by Savannah River people. I got involved, by request, in talking about environmentalism and sustainable development. Basically, the message I have on environmentalism is that it's certainly a noble, worthwhile objective. But we are not going to be able to have sustainable economic development if people don't start doing more prophylactic things—taking preventive measures. This may not be popular in all corners, but I think all of us ought to think about consuming less. There are a lot of environmental issues that become serious because people are frivolous about the consequences and don't take them seriously.

So the message that I would like to pass on is you have to be serious about the environmental consequences, the unforeseen consequences of technology. I believe that technology on balance is a friend. I want to be a friend of science and technology. There's a dark side to it. You have to recognize that there is a dark side to it. But you can't be anti-technology just because of the dark side. I've tried to tell people that over the years. I also feel strongly about the ethics that are involved with technology. I've spoken about that a great deal. This is a little different twist on fluoropolymers that you might be interested in reading (20).

The last bit of technical work that I've been involved in is with some research groups at DuPont and Osaka University, where we've been trying to establish the structure of polyhexafluoropropylene. It's still an active program. Part of the problem is that we can't get a clear-cut answer because the samples are very difficult to purify. We haven't been able to get funded.

TRAYNHAM: But so far the managers haven't decided that it's worth it?

ELEUTERIO: Right. Sooner or later, somebody will do it. But it's the last technically active program that I've been involved in.

TRAYNHAM: Well, I do have one more question that may lead you to make some comments that could be helpful to chemists in the making, so to speak. Your graduate school experiences and your post-doctorate experiences did not include any focus on polymer chemistry. Yet your first job was a polymer assignment and you were very successful with it right off the bat. Could you comment some on DuPont's wisdom in hiring a non-polymer chemist and assigning him to a polymer role and your own response to that assignment?

ELEUTERIO: That's a good question. In fact, some of the people that I was associated with were unhappy that I was going into the polymer area.

TRAYNHAM: You mean the people that you were associated with before DuPont?

ELEUTERIO: Yes, you're absolutely right. At the time, a lot of polymers were considered gunk and gunk that you couldn't work with. I was on the Advisory Board for Ohio State University after I joined DuPont, and it was a few years before the Advisory Board could convince the head of the chemistry department to get an honest-to-goodness polymer chemist on the faculty. But, you know, that prejudice existed in Carothers' time. I looked at polymer chemistry in terms of physical chemistry and physics rather than pot boiling, which is a lot of people's view of polymer chemistry. In fact, it isn't. The purity standards for a lot of monomers are higher than pharmaceutical grade chemicals that we ingest. Very often, the physical properties are completely misleading because of some trace impurity.

So very early on, you learn that experimental techniques, laboratory techniques, are very important. So to answer your question, I really took more of a physical chemistry/physics view of polymers than I did a synthetic chemistry view. But then what I found is that synthetic chemistry, over the years, got short shrift. People will spend a lot of time characterizing polymers and not really knowing whether they're pure or not. I've seen some bad data because people didn't have the sense that I think you and I would have, you know, how good is this stuff? I guess I'd like to think I have two heritages: one is a romantic heritage from my parents and the other is a pretty hard-nosed heritage from a bunch of New England, Yankee, school teachers. I guess I've never completely thought the thing through, but now that you've asked me the question—I was always interested in breaking new ground. That's fun.

You know, the greatest satisfaction was having done something that nobody had done before, to the best of your knowledge. So whether you were doing it with a polymer or monomer is not really important except you want to make sure it's right and you haven't fooled yourself. So one of the things I remember Harold Hart telling me—he had asked me to make a pound of cyclobutane dicarboxylic acid, a chemical that was not easy to make. Again, Harold put me through my paces in trying to teach me things beyond being a chemist. So I made the material and I delivered it to him and he was happy. He said to me, "You know, remember one thing—you never have to rationalize results." That stuck with me. I read [Paul J.] Flory's book and I read a lot of papers when I came to DuPont (21). I spent a lot of time catching up.

What I found interesting is, after all those years, when I went to Singapore, I was told that I would have all engineers in my class. So my first half dozen lectures were all written for engineers and had a lot of technical jargon. When I showed up, my class was forty-nine people—half of them were M.B.A.'s and half of them were engineers. I thought, "What do I do now?" So I literally got less than four hours sleep every night for the first eight-week session that I taught because I was trying to read—in fact, I was trying to do a Harvard M.B.A. in about six weeks. I worked my tail off and I kept shaking my head. My wife wasn't with me that first semester. I thought, "How did I get myself into this?" But I survived. It turned out to be interesting. But it is true that sometimes I dive into water and look for the sharks after I get in the water. But I never had any limbs bitten off. It's very exciting to find something new—very exciting. Nobody can pay you for that. It's even more exciting when you help somebody else find something. I still get a vicarious thrill when one of my colleagues, or one of my students find something new and have a better understanding of a phenomenon. That's great. I enjoy it. I've always enjoyed it with my kids. I enjoy it with my grandkids. I don't know if I've answered your question.

TRAYNHAM: Some recruitment officer or manager at DuPont had the astuteness to recognize your potential for enjoying the breaking of new ground and then to pick you for polymer chemistry even though you had not had a background in that area.

ELEUTERIO: Well, I think you make a good point. I think that recruiting people is extremely important, not so much for knowledge, but willingness to learn and eagerness to learn. In fact, you have touched on a very interesting point. When Merckling discovered linear polyethylene, everybody was excited, particularly after it was confirmed. I remember, actually, one of the guys who helped recruit me had a Ph.D. and a polymer background from Princeton [University]. I won't name him. But his reaction to what he'd heard was, "That can't be. That's a bunch of bullshit. We know there are only two kinds of polymerization. You couldn't have found another kind of polymerization." Free radical and carbonium-ions catalysts. We were talking about anionic catalysts. He was kidding the supervisor, but not completely. He was important because he had influence with the upper management.

So part of the difficulty that we found out later in the polypropylene patent interference is that some of this negativism had been put into writing. The lawyers were trying to establish that DuPont didn't recognize the significance of the invention. Unfortunately, they found some reports that, in fact, backed the suspicions up. These were written by polymer experts. I leave that to you to judge whether not having a polymer background might not be an advantage. In fact, now that I think about it, one of the rules of thumb for innovative companies is that it will not be your competitors, but somebody completely out of your field, who comes up with a disruptive technology. Andy [Andrew] Grove at Intel made sure—he doesn't have a separate R&D division at Intel. He makes sure that every supervisor or manager is responsible to following up what's going on in other companies that are not competitive to Intel, but might be working in technology areas that could impact Intel. Grove is very shrewd.

In fact, this is documented, that a lot of the real advances will come outside your field. You know Bryce Crawford. I had Bryce Crawford's contract for a long time at DuPont and we became good friends. Incidentally, I also had Jack Roberts' contract at DuPont for twenty years. Bryce, in one of his awards, said that you always ought to try to encourage people to read outside their field. I did try to read in physics—polymer physics. I was lucky to have a couple of colleagues who were willing to teach me some polymer physics at DuPont, which is part of the answer to your question. I did my homework—made sure that they weren't talking into a vacuum. They were very good. They were state-of-the-art people. DuPont had some really terrific people at that period of time. But, you know, I had a good grounding in thermo, in kinetics, and in the things that you have to be familiar with. So I wasn't trying to learn physical chemistry as well as polymer chemistry. I already knew the physical chemistry but I didn't know the polymer chemistry.

I was on the Advisory Board to the University of Massachusetts Polymer Center and I've been invited to other polymer centers. One thing I've noticed is that students tend to get too structured and get too expert in an area and then become reluctant to work outside their area. This

example wasn't a polymer chemist, but I remember a very bright guy that we hired from Caltech [California Institute of Technology]. He would not work in any area other than gas photochemistry. We had a terrific liquid photochemistry problem, which he wouldn't touch with a ten-foot pole because he said he could only do gas photochemistry. Eventually he became one of these patent agents. He had tremendous potential. Really, he worked with some very fine people. Other people—if you couldn't do it in an NMR-tube they wouldn't do it. Now, I really can't blame the system. All I say is that if the students are that narrow and not willing to branch out, then probably the education system is doing them an injustice unless they're so good that they're going to get to the top of their field somehow. But if they're looking at industry, they're probably not that good.

TRAYNHAM: You've given us a very interesting summary of your career at DuPont, which included both your bench chemist experience and your supervisory experience. You've told us something about your family. Both you and your wife are successful scientists. You have six children. Did any of them decide to become scientists?

ELEUTERIO: That's a good question. My son teaches advanced placement math and psychology at a local high school. My oldest daughter teaches in Chicago, and she was a folklorist. My number three child, number two daughter, is a lawyer. My middle son is a manager in a real estate insurance company. My youngest daughter, who was a valedictorian in high school and a very, very good student in college—I thought she was going to medical school but she had one stint, winter term, at a local hospital here—the E. I. du Pont Institute—and she decided she wanted to be a physical therapist. She could have done science. She could have done medicine. But she runs a hundred-person organization in Fairfax, Virginia. Some of her patients are young kids whose parents are in the military complex near there. They drink beer and put their heads through windshields on Saturday night and she tries to keep them alive. I couldn't do that. But she does it. My youngest son is a lieutenant commander in the Navy. He runs a weather forecasting station. He was one of these out-of-sight kids in the IQ and SAT tests. I thought he was going to become a scientist, and he may yet. But he's interested in oceanography. He probably would rather be a curator at the Monterey Maritime Museum. But I know that they have all their marbles and they're healthy, and I feel privileged.

TRAYNHAM: Apparently they all had the degrees of freedom to make their choices.

ELEUTERIO: Yes. My wife and I like drama, plays, and music. My oldest daughter was an English major. Then she wanted to go in the Peace Corps. The assignment she got, having absolutely no training, was in Chicago, of all places, at a halfway house to try to help people that had been institutionalized for, in some cases, violent crimes, but all had mental problems—trying to integrate them into the community. She survived it—not without her problems. Then she got a master's degree in folklore. She has been teaching at the Chicago Art Institute and in the high

schools. They have an interesting program, where she teaches them English, history, and how to accept other people's cultural differences and that sort of thing.

TRAYNHAM: Well, as I indicated to you early on, this is the first in a series of oral history interviews that have been requested by the DuPont Company to record the DuPont story in terms of polymer chemistry. DuPont identified you as one of the ones that they wanted to have interviewed. Do you think of anything else that should be added to make the story complete at this point?

ELEUTERIO: Well, as I mentioned to you earlier, I thought the group that I was hired into—DuPont's polychemicals department—was a great experience. The people were super. I feel lucky that I had the opportunity to be part of that group. You know, the old cliché that if you really like what you're doing, it's not work—there's an element of truth in that. I was privileged to work with some really, really first-class people—absolutely first class. What I hope this series does is try to attract some of the younger first-class people and give them the same opportunity that I've had.

TRAYNHAM: Well, I think that this recording may very well help realize that ambition. I want to thank you for being so generous with your time and your story for the Chemical Heritage Foundation archives. Thank you.

[END OF TAPE, SIDE 4]

[END OF INTERVIEW]

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INDEX

A

Adams, Roger, 11
Aiken, South Carolina, 23-24
Aluminum, 7, 29
Aluminum chloride, 7
American Chemical Society [ACS], 14, 17, 28
American Institute of Chemical Engineers [AIChE], 25
Anderson, Arthur W., 8-9, 15
Australia, 18
Azores, the, 1

B

Baltimore, Maryland, 7
Baxter, Warren, 8
Biochemistry, 20
Breakthroughs!, 22-23 35
Brown, Herbert C., 4
Butene, 9

C

Cady, --, 2
California Institute of Technology [Caltech], 33
Carbon, 14
Carnegie Mellon University, 21
Carothers Award, the, 16
Carothers, Wallace H., 12-13, 31
Center for Process Engineering, the [CPEC], 27
Chemical Heritage Foundation, the [CHF], 34
Chemtech, 35
Chicago Art Institute, the, 34
Chicago, University of, 22
 School of Psychology, 22
Coimbra University, 2
Columbia University, 22
Columbus, Ohio, 5-6
Conant, James Bryant, 2
Cornell University, 6
Council of Chemical Research [CCR], 25
Cram, Donald J., 4, 26
Crawford, Bryce, 32
Crick, Francis Harry Compton, 29
Csikszentmihalyi, Mihaly, 22

Cyclobutane dicarboxylic acid, 31
Cyclohexene, 9
Cyclopentene, 9-10, 28

D

Dowagiac, Michigan, 5
DuPont, E. I. de Nemours & Co., Inc., 6-8, 10-12, 14, 17-18, 21, 23-26, 28-34, 36
 Atomic Energy Division, 23-24
 Carney's Point laboratory, 15
 Central Research Department, 7
 E. I. du Pont Institute, the, 33
 Polychemicals department, 16, 34
 Research & Development, 18, 25, 36

E

East Lansing, Michigan, 5
Eleuterio, Herbert S.
 children, 33
 parents, 1-2, 31, 33
 Eleuterio, Donatilia, 1
 Eleuterio, Francisco, 1
 wife, 4-5, 19
Environmental issues, 24, 29-30
Epoxides, 15
Ethylene, 7, 9

F

Fairfax, Virginia, 33
Fisher, Hans, 7
Flory, Paul J., 31, 36
Fluorine, 6, 13-14
Fluorocarbons, 14
Fluoropolymer research, 28
Frame, Sutherland, 3

G

Gassman, Paul, 25
Gilman, Henry, 29
Goetzels, --, 22
Goodwin, Bill, 3
Graustein, --, 3
Greenewalt, Crawford, 13-14, 24
Gresham, Frank, 7, 10-12, 14-15
Grove, Andrew, 32
Grubbs, Robert H., 10-11, 26, 35

Guadeloupe River, the, 24
Guide to Creative Expression, A, 18

H

Hammond, George, 4-6
Hart, Harold, 3-6, 13, 31
Harvard Business Review, 22
Harvard University, 3, 31, 35
Heckert, Richard E., 25
Henne, Al, 6
Hercules, Inc., 8
Herman, William, 28
Hexafluoropropylene, 14-16, 28-29
 hexafluoropropylene epoxide, 16
 hexafluoropropylene oxide [HFPO], 15-16
Hounshell, David A., 29, 36
Hydrogen, 13-15

I

Illinois, University of, 12
Institute of Molecular and Cell Biology, 19-20
Intel Corporation, 32
Iowa State University, 4-5, 29

J

Journal of the American Chemical Society, 11, 35

K

Kalrez elastomers, 16
Kennedy, Pres. John F., 3
King, Judd, 25
Korean War, the, 4, 6
Kress, Jackie, 26-27, 35

L

Lavoisier Library, the, 7
Lavoisier Medal, the, 25
Lederberg, Joshua, 22, 35
Lehn, Jean-Marie, 26
Linear polyethylene, 7-8, 32

M

Marvel, Carl ("Speed"), 12
Marvell, --, 2
Massachusetts Institute of Technology [MIT], 2, 10
Massachusetts, University of
 Advisory Board for the Polymer Center of, 33
McGrew, Frank, 11, 16
Merckling, Nicholas, 7, 32
Meschke, Robert, 15
Metathesis, 9-11, 26, 28-29
Michigan State University, 3, 5
Michigan, University of, 10
Miller, Joseph A., 25
Montecatini, 8, 10, 28
Monterey Maritime Museum, the, 33
Moynihan, Daniel Patrick, 3
Munich, Germany, 28

N

Nafion membranes, 16
National Science Foundation [NSF], 24
Natta, Giulio, 7-8, 10-11
Nature, 22, 35
New Bedford, Massachusetts, 1, 3
New England, 2, 6, 20, 31
New York Times, 24
Newman, Melvin S., 4-6, 13
Nobel Prize, the, 8, 26
Nonaka, Ijurio, 22-23, 35
Norbornene, 7
North American Aviation, 6
Nuclear magnetic resonance [NMR], 26, 33
Nylon, 12, 25

O

Ohio State University, 4, 6, 31
 Advisory Board for, 31, 33
Olefin catalysis, 7, 9-10, 22
Olefin metathesis, 28, 29
Orange, New Jersey, 6
Organic chemistry, 2-7, 28
Osaka University, 30

P

Patrick, Ruth, 23-24
Pauling, Linus C., 3, 11
Payne, Donald, 10
Pederson, Charles J., 26
Pennsylvania State University, 3
Perfluoropropylene, 15
Petit, --, 10
Phenol
 alkylation of, 3
Phillips Petroleum Company, 8
Physical chemistry, 3-4, 10, 22, 31-32
Plunkett, Roy J., 15
Polyethylene, 7, 8
Polyhexafluoropropylene, 13-14, 30
Polymer chemistry, 30-32, 34
Polypentenamers, 10
Polypropylene, 8-10, 13, 22, 32
Polytetrafluoroethylene, 13
Polytetrafluoropropylene, 14
Princeton University, 32
Proceedings of the National Academy of Sciences [PNAS], 22, 35
Process Analysis and Optimization Enterprise[PAO], 27
Propylene, 8-10
Pryor, Richard, 24
Purdue University, 4

Q

Quantum chemistry, 4
Quantum mechanics, 4

R

Ring-opening polymerization, 11, 28
Roberts, John D., 10, 28, 32
Robinson, I. Maxwell, 7, 15
Rogers, Max T., 3-7
ROMP reaction, 10
Ruth Patrick Science Education Center, 23

S

Savannah River, the, 24-25, 30
Schechter, Harold, 6
Schrock, Richard R., 10-11
Schroederor, Herman E., 26
Science, 11, 26, 35-36

Seagram Corporation, 24-25
Shakhashiri, Bassam Z., 24
Simon, Herbert A., 21-22
Simon, Joe, 3, 35
Singapore, 16-20, 21, 25-27, 31
 National University of, 18, 25
South Bend, Indiana, 5
South Carolina, University of (Aiken), 23-24
Stamatoff, Gelu, 8
Standard Oil of Indiana, 7-9
Statistical mechanics, 4
Staudinger, Hermann, 12
Stine, Charles, 12-13
Streitwieser, Andrew, 10, 17
Sustainable development, 30
Swain, Gardner, 10
Swarthmore College, 6

T

Teflon, 15-16
Tetrafluoroethylene, 15
Texas, University of, 10
TFE/HFP (FEP)^R copolymers, 14
Titanium tetrachloride, 7
Tufts University, 3
 Fletcher School, 3

U

Union Carbide Corporation, 6
United States of America, 1, 10, 12, 17-18, 25, 29, 35
 Air Force, 6, 14
 Navy, 33
 Patent Office, 8
 Peace Corps, 33

W

Washington, District of Columbia, 8, 23
West Chester University, 19
Westinghouse Electric Company, 24
Wilmington, Delaware, 1, 23-24
Wilson, Christopher L., 6
Wisconsin, University of, 24
Woolard, Edgar S., 18
World War II, 4, 23
Wright-Patterson Air Force Base, 14

Y

Yale University, 3

Yelton, Everett, 25

Z

Ziegler catalysis, 22

Ziegler polymerization, 7

Ziegler, Karl, 7

Zuckerman, Harriet, 22, 26, 35