CHEMICAL HERITAGE FOUNDATION

HOWARD E. SIMMONS, JR.

Transcript of an Interview
Conducted by

James J. Bohning

at

DuPont Experimental Station, Wilmington, Delaware

on

27 April 1993

(With Subsequent Additions and Corrections)

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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Howard Simmons

(Date) 5/4/93

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Howard E. Simmons, Jr., interview by James J. Bohning at DuPont Experimental Station, Wilmington, Delaware, 27 April 1993 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0111).

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

HOWARD E. SIMMONS, JR.

1929	Born in Norfolk, Virginia, on 17 June
1997	Died in Greenville, Delaware, on 26 April

Education

1951	B.S., chemistry, Massachusetts Institute of Technology
1954	Ph.D., organic chemistry, Massachusetts Institute of Technology

Professional Experience

	E. I. du Pont de Nemours & Company, Inc.
	Central Research & Development Department
1954-1959	Member of Research Staff
1959-1970	Research Supervisor
1970-1974	Associate Director of Research
1974-1979	Director of Research
1979-1983	Director
1983-1990	Vice President
1990-1991	Vice President and Senior Science Advisor
1991-1997	Consultant
1968	Sloan Visiting Professor, Harvard University
1970-1997	Adjunct Professor, University of Delaware
1978	Kharasch Professor, University of Chicago

Honors

1975	Member, National Academy of Sciences
1975	Member, American Academy of Arts and Sciences
1975	Award, Delaware Section, American Chemical Society
1981	Fellow, American Association for Advancement of Science
1987	D.Sc. degree (honorary), Rennselaer Polytechnic Institute
1990	National Science Board, National Academy of Sciences
1991	Chandler Medal, Columbia University
1992	National Medal of Science
1993	D.Sc. degree (honorary), University of Delaware

1994	Priestley Medal, American Chemical Society
1994	Lavoisier Medal, E. I. du Pont de Nemours & Company, Inc.

ABSTRACT

Howard E. Simmons, Jr., born 17 June 1929, begins the interview describing his family history. The men on his father's side were merchant marines; his maternal grandfather was an entomologist from Germany and descendent of noted entomologist Jacob Hübner. Simmons, an only child growing up in Norfolk, Virginia, pursued early chemistry interests in a home laboratory and graduated high school near the top of his class. Drawn to MIT because of its post-WWII reputation, he studied chemistry and conducted research under Jack D. Roberts. Earning a B.S. in 1951, he continued at MIT with Roberts and Arthur C. Cope, completing Ph.D. research on benzyne, trans-cycloöctene oxide, and cyclobutenes obtained from adducts of acetylene. Here Simmons' describes coursework, professors, research, colleagues, and MIT's lab atmosphere. In 1953, Simmons met Theodore L. Cairns, science director in DuPont's Chemical Department, who invited him for a DuPont visit which led in 1954 to Simmons becoming a member of research staff in the Central Research Department [CRD]. He began research on polyacetylenes but quickly moved to fluoroketones. His early studies on structure and mechanisms led to the Simmons-Smith reaction, the first general synthesis of cyclopropanes, and a related patent. Here he discusses this research, relevant colleagues, and thiacyanocarbons studies, before moving on to work with Harvard University's Robert B. Woodward and proteges, including Tadamichi Fukunaga, and research on spiroconjugation. Simmons mentions collaborations in quantum chemistry and topology with Rudolph Pariser and Richard E. Merrifield, and details Cairns' program of interaction between DuPont and European universities. He describes trends in turnover from CRD into industrial departments and in company support for publications and basic research. Also discussed are his CRD promotions from Research Supervisor in 1959, to Associate Director of Research in 1970, Director of Research in 1974, Director in 1979, and Vice President in 1983. In the late 1960s, Simmons began collaborations with Chung Ho Park to synthesize macrobiotic amines, large rings containing hydrocarbon cavities. He describes this and related research on crown ethers, and their relationships to work by Nobel Laureates Charles Pedersen and Jean-Marie Lehn. He next summarizes additional publications; collaborations with scientists at Eidgenössische Technische Hochschule [ETH] and with Joseph Bunnett, George Hammond, and Jack Leonard; and associations with the University of Chicago and the University of Delaware. Finally, he discusses work as Director under Irving Shapiro and Richard Heckert, and the growth of CRD under Ed Jefferson; CRD accomplishments in molecular biology and superconductivity, including a DNA-sequence reading machine; and Senior Science Advisor and retirement work with DuPont and other organizations, including the University of Delaware Research Foundation, the National Academy of Sciences, and the National Science Foundation. He closes with a description of his sons' DuPont careers and comments on scientific misconduct.

INTERVIEWER

James J. Bohning is Professor of Chemistry Emeritus at Wilkes University, where he was a faculty member from 1959 to 1990. He served there as chemistry department chair from 1970 to 1986 and environmental science department chair from 1987 to 1990. He was chair of the American Chemical Society's Division of the History of Chemistry in 1986, received the

Division's outstanding paper award in 1989, and presented more than twenty-five papers before the Division at national meetings of the Society. He has been on the advisory committee of the Society's National Historic Chemical Landmarks committee since its inception in 1992. He developed the oral history program of the Chemical Heritage Foundation beginning in 1985, and was the Foundation's Director of Oral History from 1990 to 1995. He currently writes for the American Chemical Society News Service.

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INTERVIEWER: James J. Bohning

LOCATION: DuPont Experimental Station
Wilmington, Delaware

DATE: 27 April 1993

BOHNING: Dr. Simmons, I know you were born on the 17th of June in 1929, in Norfolk, Virginia. Could you tell me something about your parents and your family background?

SIMMONS: My dad was a sea captain, as were my two uncles. My paternal grandfather and others in the family had gone to sea, and my father's greatest fear was that I would do the same thing. [laughter] My mother's parents were from Germany; they were Bavarians. My maternal grandfather was an entomologist. If you go back two or three grandfathers, you come to Jacob Hübner, who was the first man to catalog North American butterflies.

And, as a matter of fact, German chemists in later years have found me much more famous for being the great-great-great-grandson of Jacob Hübner than they did for my chemistry. That's literally true. [laughter] These were fellows who were working on butterfly pheromones in Germany, and they were astounded that a relative of Hubner's was in chemistry. [laughter]

BOHNING: Why didn't your father want you to go to sea like he did?

SIMMONS: My dad had me at a late age, as his father had had him. In fact, my grandfather was a man of twenty-one when the Civil War started, and there are not many of us who can say that. [laughter] But he was at sea during the great years, and the Merchant Marine had changed a great deal by the earlier part of this century. It was no longer the pleasure that it used to be, particularly because of labor problems, in his opinion. My dad was too old for the First World War. Of course, he was at sea, so it didn't make any difference.

BOHNING: Do you have any brothers and sisters?

SIMMONS: I'm the only one.

BOHNING: Did you grow up in Norfolk?

SIMMONS: Yes.

BOHNING: Was all your early schooling there?

SIMMONS: Yes, all of my schooling was in Norfolk.

BOHNING: You were born just before the Depression started.

SIMMONS: Right. I was still small during the height of the Depression, but fortunately for us, I never saw much of it. We were strictly a middle-class family, but one that was financially secure, in terms of my father's work. That is not something that I have any bad recollection of; that is, the Depression years.

My main thoughts of Norfolk were during the 1940s and the war, when I was in junior high school and high school. Norfolk, of course, was a great military base, and I was steeped in that at that time.

BOHNING: What was it like growing up in Norfolk? What was your early schooling like?

SIMMONS: I don't think there was much really distinguishing about my schooling, until I got to high school. As a matter of fact, I didn't really care very much about school during the grammar school period. I almost had an inverse relationship with being interested in school as a function of time. By the time I got to junior high school and could take Latin and a few other interesting things, I started to take more of an interest in school. I liked high school much better than I did the earlier years.

I was blessed with having very good teachers. There was a math teacher who had taught mathematics at Princeton but happened to be gay and was thrown out of Princeton. But the Virginia school system saw fit to hire him. He was absolutely a super guy, along with being a concert pianist and a few other things. I had a Ph.D. organic chemist to teach me chemistry.

BOHNING: What was his name?

SIMMONS: Jackson. If you ask me his first name, I probably couldn't tell you. He had a Ph.D. from the University of Virginia in organic chemistry. He ended up as a newspaper editor, which was what his real love was. [laughter] After he had taught school for many years, he got out of that and became a newspaper editor in Smithfield, Virginia.

At any rate, I thought I was very lucky in high school and had a very stimulating group of teachers. A particular love of mine was languages, especially French. I won the Virginia first prize and gold medal in a French competition for high school students. After winning for three years, they gave me a full scholarship to the University of Virginia in French, which I turned down and broke my father's heart. [laughter] I made him pay for me to go to MIT, where I could study chemistry. [laughter]

BOHNING: Let me ask you just a little bit more about your high school chemistry experience. Did you do a lot of laboratory experiments?

SIMMONS: Actually, it seems to me I was always interested in chemistry for reasons that I don't really know. Much earlier, my father had a carpenter convert one small room of our house to a laboratory, when I was around twelve. This was not terribly well-equipped, and I don't remember doing anything original or really interesting in it. But it played a great role in exciting me, even though I was just following directions and doing experiments and learning.

So my early experience was all learning, but I found it very, very exciting. As I say, I had an honest-to-goodness little laboratory setup, from twelve on. By the time I took high school chemistry, I thought I knew all that stuff. [laughter]

BOHNING: Did you ask for this laboratory, or was it your father's idea?

SIMMONS: No. My father only complied; it was my idea. The science was on my mother's side of the family, the German side, and I think she was very anxious for me to do things like that. If I wanted it, she would support it very strongly.

BOHNING: Did you have any interest in that family part of studying entomology?

SIMMONS: No. I knew my grandfather. He was in his nineties when I was a boy of ten or twelve, and I only knew him briefly. He was an accomplished man. I still have some of his notebooks. He kept everything in Latin and spoke seven languages; a very strange fellow. He was still out catching butterflies when he was ninety. [laughter]

BOHNING: Why did you turn down the language scholarship at Virginia to go into chemistry?

SIMMONS: It had been in my mind all along that I was going to do something in physical science. I dearly loved French and French literature and languages in general, but it was an avocation, and I've kept that up over a long period of time. It had gotten fixed in my mind very early on that I wanted to be a chemist. I truly can't tell you why, except that it sounded like an exciting thing to do. [laughter] I enjoyed reading about it. I think there was an early recognition, sort of astounding to a child, that the whole world is made of chemicals; it's rationalizable, and presumably we can get control of all these things, if we know enough about them.

BOHNING: Was there any specific reading that you had done at that point?

SIMMONS: Probably, but I can't remember.

BOHNING: Why MIT? How did you pick it?

SIMMONS: For no good reason, other than the fact that it received such great publicity during the war. MIT, to many young people in the 1940s, was like a Mecca. This was where radar came from, this was where a lot came from. It was the great place in science to many of us who didn't know anything beyond that they were talking about good places in science. [laughter] I have to confess, it wasn't for any relatives going there or anything else. It was just simply that I knew of it as, or thought of it as the Mecca of science. Rightly or wrongly, it was the reputation that came out of the war. So it had fixed itself in my mind somewhere, when I was sixteen or seventeen, that that's what I wanted to do.

BOHNING: Did you have any friends who were like-minded?

SIMMONS: No, most of my friends in Virginia in those days—and I imagine a lot of states were not too different—went to William and Mary, the University of Virginia, particularly VPI and VMI. It was mostly local Virginia schools. A handful of us went out of state, but I think in those days, it was partly the cost and partly people just didn't think about it. I don't know; I just took off and went there. [laughter]

BOHNING: Had you traveled any outside of the Virginia area before that?

SIMMONS: Nothing significant. Just within a few states; that's all.

BOHNING: What was your reaction when you got to Boston?

SIMMONS: I liked Boston very much. I think it took a little bit of time to grow on me, because I was pretty green when I first went there. But it was a thrilling thing to me, as I noticed it was to many freshmen, guys who later became good friends of mine. All of us found it thrilling in those days to be going to this Mecca.

After you were there for a while, you might have changed your mind. [laughter] This was right after the war, and MIT was very tough on the undergraduates; they had to try to cull out some of the large number of veterans that they were obliged to admit. It was not unusual at all to have the class average of physics exams, chemistry exams, calculus exams, that sort of thing, of about thirty. That was very common in that period. They were very tough on people.

After the first year, I lived in a social fraternity. I was a Deke—Delta Kappa Epsilon. They had a fraternity house that was on the campus, right on Memorial Drive. I thoroughly enjoyed that, and I thoroughly enjoyed the undergraduate program in chemistry there. That was where I met Jack [John D.] Roberts, who was a young chemistry professor. I did a bachelor's thesis with him, which actually was published (1). We actually got a paper out of it.

BOHNING: Yes, that was an early paper in the small-ring compound series.

SIMMONS: That's right.

BOHNING: Let me just back up for a moment. I assume you must have had a high ranking in your high school class in Virginia?

SIMMONS: Yes. It was a big high school, and out of several hundred I think I was third, or something like that.

BOHNING: But when you got to MIT most of the other people were similarly inclined, so the competition must have been a little different?

SIMMONS: Yes. Except that it was very compressed, MIT was sort of like starting school all over again. The longer I stayed there, my enthusiasm really grew rapidly with time. I really enjoyed those latter years, but particularly my junior and senior years, and did very well. I'd say in my freshman year I was only a modest student, and by the time I graduated, I was first in the chemistry class.

BOHNING: How many chemistry majors were there in that group?

SIMMONS: Thirty-odd, or something like that.

BOHNING: I'm assuming you started there around 1947. Maybe we could just go through the curriculum, as you experienced it at that time. Whom did you have for your first chemistry course?

SIMMONS: A man named [Edmund Lee] Gamble, who was a Virginian, of all things. [laughter] He was a true Virginia gentleman; he dressed and he spoke like somebody from the western part of the state, from the aristocracy, and he literally was. He was a marvelous chemistry professor, at least from the teaching standpoint, for undergraduates. He worked hard at this. You had all the time that you could have wanted, any time you needed to see him. He was a very inspirational guy. Certainly not a great chemist, but really a great teacher. This is just what places like MIT needed, fellows like him to grab you in that first year.

One of the things that I truly enjoyed as an undergraduate there, and I don't know how it is now, was that it was very rare that I met a faculty member who didn't have a lot of time for students. If you dropped in, they would put their feet up on the desk and talk to you. Some of them were great names, so that was a real thrill for us. That started right out in my freshman year with Professor Gamble.

BOHNING: Do you remember the text that you used?

SIMMONS: No, I don't.

BOHNING: I was just curious as to what they were using then.

SIMMONS: We had texts all through MIT, but we depended very heavily on lectures; people took really copious notes. It was sort of stupid, because you spent your whole time writing, rather than thinking. I remember the original math book and the physics book, but I don't remember chemistry. [laughter]

BOHNING: In the sophomore year, did you have organic or quantitative analysis?

SIMMONS: Interestingly, between the freshman and sophomore year in those days, MIT required their chemistry majors to spend a summer at MIT and take qualitative analysis. I can't remember his name now, but he was one of the famous old inorganic guys. He worked us pretty hard during that summer, but I think most of us looked at it as a special kind of vacation. [laughter] It was a lot of time you had to put in to it, but it was fun. Qualitative analysis was basically a lot of fun. I think those of us who were seriously interested in chemistry got a kick out of it. If I remember correctly, quantitative analysis was a sophomore course, but we took the qual in the summer.

In those days too, MIT had it arranged that the chemistry course had more elective time than many other departments. We had a lot of opportunity to take other things than chemistry, things you were interested in. For people who were going on in chemistry, like I planned to do, by the time I had graduated, I had taken all of the required courses for a Ph.D., with maybe one exception. I had decided, somewhere in my junior year, that I wanted to stay there. It was after meeting Jack Roberts and working with him. Then I was totally sold when I was a senior doing an experimental thesis for him, that I really wanted to stay and work for him in grad school.

Art [Arthur C.] Cope, who was chairman then, was pretty adamant about not having people stay, but he allowed a couple of us to stay, Ken Kopple and me. And this worked out great for me, because I started right away doing a Ph.D. thesis with very little class work to do, and practically nothing in chemistry because I'd taken all of these as undergraduate electives. I did have to take a minor; I did a minor in both physics and math. So most course work was in the minor, not in chemistry.

BOHNING: Whom did you have your first organic course with?

SIMMONS: I believe it was with Jack.

BOHNING: That's what I was wondering. That's where you first met him?

SIMMONS: Yes. Over the years, I had courses with Jack, with Gardner Swain, John Sheehan, Cope, and [George] Büchi.

BOHNING: That's a stellar lineup.

SIMMONS: Yes, it was great; they were great. Art Cope had just finished reorganizing things there and was bringing in a bunch of young blood, and there was a tremendous chasm between the older guys on the faculty and the younger ones.

BOHNING: Were you aware of that as a student?

SIMMONS: Yes, everybody was. In fact, a lot of interactions ended up in shouting matches, or people not speaking to each other and things like that. So the students knew about a lot of them.

BOHNING: What was it about Jack Roberts that attracted you to him?

SIMMONS: At the most trivial level, I really liked the way he drew structures. [laughter] He was the one who excited my interest in physical organic chemistry. I'd always liked physics, more of the quantitative physical mathematical side of it. It was exciting, because he was actually in the center of one of the major controversies, the non-classical carbonium ion, and made a lot of major contributions to it.

Almost everything was exciting about Jack to me. He was a very kind, considerate guy, who took a lot of time with his students. He was very impressive, because he was an outstanding glassblower, and he could do almost anything in the laboratory. He could put all of his students to shame in terms of laboratory technique. It was not just because he was older than we were or had more experiences, but because he was really good! [laughter]

I was very impressed with the laboratory and actually doing things with your hands, and I think a lot of that came from Jack. He certainly stimulated a lot of us, both from the theoretical end and from the laboratory end.

BOHNING: That was really the time when organic chemistry was changing. Jack Hine's book (2) wasn't out yet, and [Louis P.] Hammett's book was the only physical organic book available (3).

SIMMONS: Hine came out right around that time.

BOHNING: I think it was around 1955.

SIMMONS: No, I thought it was before that.

BOHNING: Before that?

SIMMONS: Yes. It might have been when I was in grad school that it came out.

BOHNING: So you really saw a change in organic chemistry?

SIMMONS: Yes, and what was very exciting was that each time you picked up a journal, each time you picked up a JACS, there was one of the classic reactions, the Knoevenagel reaction or something like this, that someone was working out the mechanism on. The idea of being able to draw and rationalize in a quasi-mathematical way—the bookkeeping of electrons and that sort of thing and the flow of electrons—had a great appeal to me.

There was no question that in the 1950s, chemistry was really breaking open, and I think all of us recognized it at the time. All you had to do was look at journals ten years older, and it almost looked like you were looking at something in another world. So this stuff was really new. New approaches to mechanism, new approaches to synthesis—all of that seemed to be hinted at in World War II. When the war was over, suddenly we took off in great new directions.

BOHNING: Was there any interaction with the Harvard chemistry department?

SIMMONS: I didn't have any as an undergraduate. There was modest contact in graduate school, where I would go to some of the joint seminars that Swain and [Paul] Bartlett had together.

BOHNING: I'm thinking of Louis Fieser in the more classical organic chemistry.

SIMMONS: Yes. But I did not have any contact with Louis really until I was teaching there, much later.

BOHNING: You indicated earlier that you had done an undergraduate thesis with Roberts. Did you ask to do that? Was it standard to do that?

SIMMONS: It was required. MIT required you to do an undergraduate thesis. In educational talks, I've often included that point. I'm a strong advocate of that. If you want to be a chemistry major, I think even if you're not going to go beyond that, it's a really good thing to learn what the laboratory is about. There are plenty of things to work on as undergraduates that are not taxing but very valuable from an educational standpoint.

[END OF TAPE, SIDE 1]

BOHNING: Did Roberts assign you to this problem?

SIMMONS: I think it was more or less an assignment. He talked to me in terms of, "Boy, here's something exciting for us to look at." And of course, if he said it was exciting to look at, I thought it was exciting to look at too. [laughter] I don't think he said, "This is what you're going to do." But it was certainly his problem and it bothered him. He was familiar with this very old [N. A.] Demjanow work in the rearrangement of cyclobutanols, and he wanted to learn more about that. This silver salt reaction without solvent was sort of bizarre, because you got more or less a non-classical carbonium ion distribution, but with no solvent. [laughter]

It was one of the stranger reactions I've ever performed, because you grind iodine and silver cyclobutanecarboxylate together gently to mix them, put them in a vial and heat them up. There's a big burst of purple light, [laughter] and it's all over in a flash, literally a flash. And here are all of these three conversion products of cyclobutyl carbonium ion, as esters of cyclobutanecarboxylic acid.

BOHNING: Did this give you a chance to interact with Roberts' group as a whole, especially the graduate students?

SIMMONS: Yes. He assigned a half or a third of a lab bench of a graduate student to an undergraduate. As a matter of fact, the guy who's bench I worked at was Rudy [Rudolph A.] Carboni, who also came to Central Research here at DuPont. He and his family subsequently became great friends of ours. He was just starting in graduate school. He had spent some years in the service and was six or seven years older than I was, but we became close friends through that beginning. So that was great, because you had some older graduate students to look to for help.

BOHNING: When you entered MIT in 1947, had you already planned going on through the Ph.D.?

SIMMONS: I think so.

BOHNING: Had you given any thought beyond that point?

SIMMONS: No. I never had a clear thought that I was going to teach, because I really didn't know that much about it until I got into graduate school and saw what life was really like. I think when I was working with Jack, I came more and more to that thought. Actually, after two years in graduate school, Jack left and moved to Caltech. Jumping ahead a bit, on the thesis that I'd done for him, he said I was finished, and in essence, Cope said, "No, you're not." So I stayed and worked for Cope for a year. Jack had arranged a postdoc for me with [Linus] Pauling at Caltech. My wife and I were thinking about doing this, but I was in the Army Reserve and was under active duty orders to appear at Fort McClellan, Alabama. To be frank with you, that certainly colored my view of this.

In the meantime, Ted [Theodore L.] Cairns, who at that time was a science director in DuPont's old Chemical Department, had met me as a graduate student when he was poking around at MIT. He had invited me down here on a visit. It was not a formal interview but an earlier one, a year early. He really knew how to recruit people. [laughter] He made it very clear to me that there was a job here. I was terribly impressed when I saw the Experimental Station and found out that people in industry were doing honest-to-goodness chemistry here.

Having that option available and having a lot of flak from the Army, I told Jack that I was going to put that postdoc aside for a while, and maybe I'd think about it later. In those days, we talked crazy things like, "Well, we'll go to work for a couple of years and then maybe go back to academe." [laughter] At any rate, I ended up coming to industry.

The thing that moved me here was Ted Cairns, more than anything else. There have been a lot of internally important chemists, some of external importance too, in the old Chemical Department at DuPont. But Ted Cairns was the guy who made this into a modern chemistry laboratory. He brought in fields that were traditionally not here, like physical organic chemistry. This place was entirely synthetic and dominated by Illinois people, as Ted was himself. But he recognized the value of bringing in some MIT [laughter] physical organic chemists and things like that.

He did a great deal, in terms of bringing in very qualified people and making sure they got promoted. There was a time here when the science directors were Bill [William D.] Phillips, Earl Mutterties, me, and Ralph Hardy, and three of us were Academy members before we finished. These people were all handpicked people by Ted. Ted's influence here was really incredible, when you compare this with the old days. It's not that what was going on before was bad; it was just Ted trying to move us into the twentieth century a little faster. So he's the reason I came here.

BOHNING: Let me go back to some MIT questions. You had indicated that Cope's policy was not to let people go on, and he made the exception in your case. Did that take much persuasion on your part or Roberts' part?

SIMMONS: I think Jack must have been the one who talked him into this. Ken Kopple worked for Sheehan, and Sheehan got Kopple to stay on. I believe there was a third person that year, but I can't remember. In general, in the average year it was zero; there were very few people who stayed on. I think Art recognized that his principle was a good one, but that if there were promising students who really wanted to stay, it was maybe sort of dumb not to keep them.

BOHNING: How would you describe Cope and what were your interactions with him?

SIMMONS: Well, I knew Art as a student and as a young scientist here, then as a supervisor. It took me several years to get around to calling him "Art." He was not the kind to call "Art" very fast. [laughter] I always liked him. He always presented to me a very rational, gentle face. I always had the impression that he genuinely liked me and confided and talked to me frankly about things here or elsewhere. I had only a very good experience with him. I think most of the students who worked for him, who were tolerably good students, had the same view. He was not a hard taskmaster, although he was demanding; he expected a fair amount out of his students.

At any rate, when I stayed on for my third year, I worked on the rearrangements of trans-cycloöctene oxide and the transannular hydride shifts that occur with that (4). I really enjoyed that; after a year, he let me go.

He was a good friend of Ted Cairns, and Ted Cairns actually came to my Ph.D. orals. I don't think Dr. Cope probably should have let him. [laughter]. But Cairns actually came to my orals and listened.

BOHNING: So your thesis work was considered more the work with Cope than with Roberts?

SIMMONS: No. The thesis was with Roberts. I started the benzyne problem for Jack. Actually, after several months, Jack went to Europe for several months, so we were not together a lot of this time, except that I wrote to him every couple of weeks and sent him lab reports. While he was gone, I made the first C-14 labeled chlorobenzene, made benzyne from it, ran through the sequence and demonstrated the equivalence of the ortho-carbons. I also made the deutero-halobenzenes, chloro- and fluoro- and bromobenzenes, ortho and para, and looked at the proton exchanges under the reaction conditions. About a year or so ago, Jack said, "You know, when I look back in those notebooks, that was all really great stuff. It was all right." [laughter]

I had a really great start in being allowed to do things on my own. He considered that what I had done was enough, in terms of original work and new techniques and that sort of thing, and it was done in two years. That was not totally unheard of in those days. There were a lot of good people who might have gotten out in two or two-and-a-half years.

He wanted me to come with him, and I didn't because of the Army problems. Everything was quiet with the military when MIT was dealing with them. I didn't want to stir all this up again by trying to move schools in the course of this.

I did the first part of my thesis on some small ring chemistry for Jack too, besides the benzyne part. Benzyne was the major part of my thesis (5); the secondary part of it was the trans-cycloöctene oxide (4); and the third part of it was on cyclobutenes obtained from adducts of phenyl acetylene (6).

BOHNING: Your benzyne paper was the very first paper I gave a seminar on when I was a graduate student. That was 1957. I have two questions relating to that. First, how did you get into this concept in the first place?

SIMMONS: This had been bothering Jack for a long time. In reading the old literature—and I can't remember the references now—he had been unable to rationalize in his mind or on paper, mechanistically, how some of these very strange rearrangements occurred under very mild conditions. They were unprecedented electronically, unless you did something as crazy as to say, "Well, a simple elimination reaction occurs, and then addition occurs back to this very short-lived, highly energetic intermediate." He had speculated that this might be an intermediate and told me about it, and asked if I wanted to work on it and see if I could demonstrate it.

BOHNING: The other part of my question relates to my experience in giving that first seminar. It was my first year as a graduate student, and I remember the reaction afterwards, because a number of the people didn't believe it. I was wondering what your experience was in that regard.

SIMMONS: The answer to that question is, "Then you explain [laughter] the equivalency of the ortho positions."

BOHNING: Did you get much skepticism from that paper?

SIMMONS: Oh, yes, but there are others who believed it from the beginning, like Georg Wittig. Wittig, of course, did his own work in this area. The Germans quite accepted this, and there were some pretty good people in Germany in that period. In particular, I think Wittig was a believer; Saul Winstein was a believer. That's what it really took, if you knew folks like that.

BOHNING: In those last few years at MIT, what was it like working in the laboratory? Who were some of the people who were with you?

SIMMONS: It was very nice. For one year, Andy [Andrew] Steitwieser and I were lab mates. We had some large labs where maybe five or six graduate students would have half a bench. We also had some very small labs that were just two-man labs, and I was lucky enough to be in one of those with Andy. In those days, Andy and I were at each other's throats more than anything else. He was an obnoxious young bastard, I thought. [laughter] But very bright. We didn't get along too well until I was out of school and here. Andy and I started seeing each other professionally for one thing or the other and I got him here consulting, and we ultimately became good friends over virtually all of our careers.

But it started out with a lot of antagonism. I was a graduate student and he was a postdoc, and he didn't have much faith in anyone who didn't think pretty much like him, in those days. [laughter] He mellowed a great deal over a period of time, and I came to greatly admire him. We were good friends over virtually all of our careers, but it didn't start that way. It was good because he was very bright, and it was nice sharing a lab with people like that.

BOHNING: Anyone else?

SIMMONS: Yes, there were some pretty potent folks around at that time. There was a man named Bob [Robert H.] Mazur who was probably the most brilliant student Roberts ever had. I think Jack would still claim that might be true. He had this fellow Mazur at the very early part of his career. He was a physical organic guy, and it was very strange for him to go to a drug-house, but he did and adapted to that and did a lot of very important things later on. I can't recall what they were now, but he did very, very well in his professional career in the pharmaceutical industry.

Everyone who knew Mazur thought that he was truly exceptional. He was the sort of person who would go to the cyclotron and get some hot chlorine that was made that afternoon and rush back, make hydrochloric acid, make Lucas agent, make a halide and hydrolyze it, and measure the rates of radioactive isotopes that only had a few hours' half-life. [laughter] He did those things and did them beautifully. He was a very deep thinker and a powerful thinker. I think Bob Mazur was a guy who really influenced me a good deal.

As I said, Rudy Carboni, who was a very good organic chemist himself, became a personal friend and also came down here to DuPont. E. J. [Elias] Corey was just getting out of there when I was starting; we overlapped very briefly. I was starting graduate school and E. J.

was finishing. I was a first-year student, but he was a last-year student. I knew him, but that's just about all I could say; I really didn't get to know him well until later years.

BOHNING: As we said earlier, that must have been a very exciting time, with the group you were in and the events that were happening in chemistry.

SIMMONS: It was the events of chemistry. Almost every named reaction or known reaction was up for grabs, in terms of speculating or working on it mechanistically. It didn't have to be done with the depth of a Jerry [Jerome A.] Berson with all the infinite nitpicking details. [laughter] It could be done in the way of a Hine. I guess my interest was somewhere sort of in between those two, like Jack's approach.

BOHNING: Did you meet Hammett at all during this time?

SIMMONS: No, I never met him; I heard him speak but never met him. Still, if you look back at that period and look at his book (3), there's enough information there that any chemical company in the United States could begin to look at processes they were running and probably save themselves millions of dollars by understanding a little better about the mechanisms of what they were doing. He was an honest-to-God father in physical organic chemistry. He was a physical chemist, but he was, nevertheless, the real father of physical organic chemistry, as far as I was concerned. He and Paul Bartlett.

BOHNING: As a graduate student, you were certainly reading the current literature, but did you read any of the old literature at the time, like Roberts was doing?

SIMMONS: Not so much as a graduate student, but when I came to DuPont, this became a real hobby. I pored over the old stuff, because I found that there was just a gold mine of things that you could speculate on and that might have a modern interpretation that would make them very exciting. Later on, the cyclopropanation reaction I worked on was derived exactly that way. I came across these old references of [G.] Emschwiller and read about his getting ethylene under very mild conditions from methylene iodide and this bizarre zinc-copper couple. There were ways of interpreting that as either a low-energy methylene or a carbenoid, which was unknown. As a matter of fact, that turned out to be the first carbenoid. I spent a lot of time at night in the library, with two or three other friends who were interested. We'd come over, pore over the old *Berichte* and that sort of thing.

BOHNING: Did the MIT undergraduate curriculum have any course in chemical literature or history of chemistry? They were somewhat common in those days.

SIMMONS: If it did, I don't have any recollection of it at all. I know I didn't take it.

Those were also great undergraduate days. I had economics with Paul Samuelson. Paul and I are both on the finance committee of the Academy. It's incredible that that would be so. [laughter] I had de Santillana in philosophy. The Institute was a grand place after the war. You never thought very much of it as a school for the humanities, but they were making a real effort to have quality education in the humanities there.

BOHNING: How much humanities did you take?

SIMMONS: Only what was necessary. [laughter] I'm afraid that most of what I've done in the way of fine literature and that sort of thing, which is appreciable since I've been a voracious reader, has been afterwards. But in terms of formal education, along those lines, not a hell of a lot.

[END OF TAPE, SIDE 2]

BOHNING: I guess that brings us to your arrival here at DuPont. I think your title was just "research chemist"?

SIMMONS: I was a "member of the research staff." All of the departments at DuPont had titles, and this has been an ongoing battle for all the years that I've been here. There was one other department that didn't use titles. Our view was that there was a great deal of prestige of just being a member of the research staff of a really good department. Other departments used an old system, senior and junior assistants, all these sorts of titles. This was to show promotion, and some people liked this. I found that the top fellows in DuPont, when they came to Central Research and lost the fellow title, which was <u>the</u> big title, came to me six months later and said they couldn't care less. The freedom to do good science was more important to them. So we never had any titles.

In those days, we had a very skimpy infrastructure. We had members of the research staff; research supervisors; associate directors, who were sort of like a research manager and had two or three supervisors under them; an assistant director of research; and a director of research. We dropped having assistant directors fairly early on. Pretty soon, we just had a director of research, which was pretty high in DuPont levels; the associate director; and supervisors. So it was really only four levels.

BOHNING: If I may ask, what was your first salary?

SIMMONS: When I signed up, we had agreed on a salary of five hundred and fifty dollars a month. Before I came to work, I got a letter saying I had a salary increase to six hundred dollars. This wasn't me, this was just the system at work. The latter 1950s and the early 1960s were that period where things were really changing rapidly. Particularly in the 1950s, when we sometimes went through two and three salary increases a year. They were small, but the point was we were rushing ahead, trying to reflect the growth in the American economy and the competition for hiring and all of these things. I can remember in some years, we would have two and three increases a year, which was a great period to live through. [laughter] I enjoyed it.

BOHNING: You mentioned that Cairns was the one responsible for your being here. Had you considered any other place?

SIMMONS: I only interviewed one other place, and that was at Rohm and Haas, because at the time, there was a pretty good guy running research there and a few good things going on there. I did interview them, but they didn't make me an offer. I interviewed at DuPont and they did make me an offer, and I took it.

BOHNING: You said that Cairns sort of had you come down for a visit a year before you finished.

SIMMONS: That's right.

BOHNING: I assume that you came back for another interview.

SIMMONS: I came back for a regular interview.

BOHNING: Whom did you talk to, and what was it like?

SIMMONS: In my first trip down here I just visited Central Research, CRD, in a very informal way. I talked to Dave [David C.] England, who did so much in fluorine chemistry. Of course, Rudy Carboni had just come before me, so I talked to Rudy. There was Ed [Edward L.] Jenner, who was a mechanistically inclined guy and very bright. I also talked to a chemist, Dick Heckert, who later became chairman. Several of the staff here were doing things that they could talk about.

At that time, there were a relatively small number of people who were known outside of the industry. The great days of publishing and getting involved with chemistry on a wider scope with the university people in meetings and all of that occurred after I came here, so a lot of the people who were here when I interviewed were not so well known outside. A few of them were, like England. I guess I was very impressed with the esprit of the place and the kinds of things that they were doing. They were doing honest-to-goodness work that was not so far removed from academic work, and it sounded pretty exciting.

The second time I came here was a regular DuPont interview, where I spent one day with CRD, one day with the Explosives Department, and one day with Orchem, Organic Chemicals Department. I think three departments made me an offer, but I ended up taking this one.

I met Herm [Herman E.] Schroeder, who became the director of research at elastomers. He was a supervisor at Orchem at the time, because that was before the Elastomers Department existed. I was very impressed with him, and we've talked about it in later years. He was one of the people I <u>really</u> remembered. [laughter] There were a lot of people at Explosives, too, who made a very good impression on me.

I was not so much interested in an industrial career as I was interested in a chemistry career. This is why I came to CRD, because it literally promised that kind of a career from its own history. It's easy to say these things, but it's very true. I never thought about a management career. Those sort of things never interested me in the least.

Whether you believe it or not, I can tell you that virtually every promotion I've had had a fair element of surprise in it, because I didn't covet these things. My first response to any promotion has been an almost sinking feeling, because I really didn't want the responsibility, compared to the things that I was doing.

One of the top recruiters at DuPont, who was a Ph.D. chemist himself, was Jack Reynard. Many hundreds of people who came to DuPont in that period were involved with him. In terms of salary, I remember he told me that by the time I retired, in the normal course of events, if I'd stayed a scientist and went up and did well, but didn't do anything special in terms of promotions to management or anything, that I could expect to double my salary by the time I retired. That's what was happening in America at the time. I think by 1959 I doubled my salary. [laughter]

BOHNING: What was your first assignment when you arrived here, although I'm not sure that's the right word. What were you first asked to do?

SIMMONS: We were divided up into research groups that were more or less discipline-oriented. That is, this group would be mostly organic chemists, this one inorganic chemists, this one biochemists, this one physicists, or what have you. In those days, all of the groups used to have brainstorming meetings where they would try to come up with interesting problems. These would get talked about and then the person who had brought it up would be given the job of casting this into one or two paragraphs in a more formal form as a type of research proposal. These were kept in a big bound book that all of us could look at. Very often, the exploratory projects were chosen out of that.

When I first came here, I worked on a problem that Ted wanted worked on. I can't reproduce the rationality of it, but the idea was to make perfluoropolyacetylene. That is, the simplest example would be a triple-bond CF_2 as a repeating unit. There were some calculations suggesting some interesting or unusual properties of this. Ted wanted that looked at, and as the new guy on the block, I was assigned that.

My first boss was Don [Donald D.] Coffman. Coffman was the first guy to really spin nylon. Julian Hill would shoot me if he heard me say that, but the fact is that's probably true. Don was rewarded handsomely by the company, but died thinking that he had been denied the role of honor that he should have had. I think most of us felt that was right. He was the first guy who used a syringe and actually demonstrated the spinneret principle, I believe.

He was a very fine classical organic chemist, one of the sweetest, nicest people I've ever known in my life, outside of here; inside, he was a martinet that we all hated. [laughter] No one who ever worked with Don loved him. One of the more unusual things that I've encountered among lots of people was his ability to raise your hackles when you worked for him, but that same evening he might have you to his house for dinner and you just could not find a warmer, nicer person. He really was a warm, nice person, but he just did not know how to handle people under a work setting; that's what it boiled down to.

He was very interested in this perfluoropolyacetylene that I was working on, but then I started to branch out. I got disinterested in polyacetylenes [laughter] and was working on other things. For instance, I made the first ketals of a perfluoroketone. These are really extraordinary materials. You can't hydrolyze them with concentrated hydrochloric acid or with metals. There are products now that involve that kind of ketal linkage with a fluoroketone.

BOHNING: Paper number ten on your list, with [Douglas W.] Wiley, is the first one that was in that series (7). Was there much of a problem with publishing something like that? I believe DuPont led the industry in the number of publications for a long time.

SIMMONS: Again, the man who pushed this prior to my coming and certainly brought it to a head after I was here was Ted Cairns. Ted was very adamant about this. The gripes came primarily from the industrial departments. Even as late as the 1970s and 1980s, there were high people in industrial departments who believed that <u>nothing</u> should be published. You are giving succor to the enemy if you publish anything. The only time you published is when it was in concert with some patent purpose, that you were going to strengthen the value of the patent.

But we finally worked out a scheme that we had the departments review our publication proposals in a fairly short order. I think the secret there was to deluge them with so many applications that they had to succumb. [laughter] We did eventually come down to a point where things that were of just scientific value but there was no clear company interest in could go through pretty quickly. If there wasn't, there were some clear rules about what to do next and to get this to the point where we could publish it. Occasionally, you might have to give up for a while. But when it was working its best, the bulk of the things out of here were being published, and only the truly industrially significant things were being held up. But there were many people in DuPont who wanted to hold up the whole damn works.

BOHNING: I was going to ask how you responded, when you started here, to what might be called the industrial mentality. In going to meetings and talking to other people in other places, did you have to be aware of, let's say, a good scientific discussion as opposed to, "I can't say this because it might affect the company?"

SIMMONS: Yes. I think that if you're a reasonably serious scientist, that's mainly manifested in the early years. As soon as you've been around for a few years, you've got enough things that you can talk about that aren't going to get you into difficulty. Most people who work in a setting like a corporate laboratory here will retire with a huge number of unpublished things; it's a tragedy. I've got drawers full of stuff that with a little bit of work—some with just writing, some with a little bit of cleaning up here or there—would make an interesting paper. As I say, once you're here for five or six years, by that time, when you're at an ACS meeting or maybe going to give a talk at a school or something, you usually do have enough things to talk about. I think you're right, though, that there's an awkwardness at the beginning.

BOHNING: You mentioned that you tired of Cairns' polyacetylene early. How did you move into the fluoroketone area?

SIMMONS: Well, I just <u>did</u> it. In those days, we were always encouraged to spend ten percent of our time here in what we wanted to do. There was a point where I was spending much more than ten percent, because I just didn't see anything entrancing about perfluoropolyacetylene. Coffman wasn't very happy with me.

Dave [David M.] McQueen was a director of research who just stopped in the lab one day as he was wont to do with the young scientists and relatively new people. He asked me what I was doing. I told him and showed him what I was doing with this fluoroketone ketal. He said, "That's not your program, is it?" I said, "No. It will probably get me in some trouble." He said, "For God's sake, don't worry about any of that; you work on whatever you want to work on. These look great to me." [laughter] That put an end to perfluoroacetylenes. [laughter] I never went back to fool with <u>them</u> again.

BOHNING: You started here in 1954, and your first paper out of DuPont came in 1958. That was with Smith on the cyclopropane synthesis (8). Again, that was a pretty unique kind of paper. How did you get into that area?

SIMMONS: It was just an exploration on the side, as I said, reading the old literature. If you remember, at that time, [William von Eggers] Doering and Hine and others were doing so much in carbene chemistry. It was a very exciting thing to think about. I thought that there was a remote chance that this intermediate in Emschwiller's work might be a low-energy methylene, just like the low-energy dichlorocarbenes were, since it was not born from a diazo compound or irradiation. Of course, that didn't turn out to be so, but it turned out to even be more exciting, because it was a long-lived carbenoid which reacted with even unactivated double bonds.

So I found this new intermediate, and the first time I ever ran the reaction was with cyclohexene. This is usually just a faintly exothermic reaction, if it works well. So after running it several hours, I had no idea whether anything had happened. But I filtered out the solids and distilled the product. Sure enough, the boiling point, after just a little bit of cyclohexene, went up [laughter] to where norcarane was supposed to boil. But there was only very little norcarane from Doering's work with diazomethane and cyclohexene up at the Hickrell Foundation.

So Doug Wiley called up Larry Knox, who was a black chemist who ran the Hickrell Foundation for Doering. Larry read him the infrared bands out of the infrared spectrum of norcarane, and sure enough, that's what it was. Here I was sitting with a bottle of norcarane, probably more than had ever existed in the world. That was pretty exciting. It was like a pig at a trough, just find anything with a double bond and try it. [laughter] I did that for a while, from the patent standpoint, trying to get the scope of the reaction, and I did a few experiments to look at the mechanism.

Then Ron Smith came along. Ron was a Caltech undergraduate and MIT graduate school. He was assigned to work with me on this. So we worked, looking mainly at synthetic questions. It was later, in the early 1960s, that I worked on the mechanism of this with Doc [Elwood P.] Blanchard, another MIT guy. Doc was a postdoc of mine for four years here. We had an arrangement in those days—it was such a glorious period—where a guy would be hired by DuPont. If he was willing, and someone like myself as a research supervisor was willing, he could put himself under me as an internal postdoctoral student and still have the same salary and get the same raises.

Doc stayed as a postdoc of mine for almost four years. We did a lot of mechanistic work on cyclopropanation (9) and I think Doc did the first GC kinetics. Doc had built a really nice system for doing that. Then he came up with on his own—which I had nothing to do with—the first real synthesis of a bicyclobutane. Again, we sent Ken [Kenneth B.] Wiberg at Yale a pound bottle. [laughter] This was with a methyl and a cyano at the bridgehead.

Doc did very well on his own. He went from a postdoc to a supervisor here. After a year or so, he took off like a bird, went to our old film department and shot up to become vice chairman of DuPont. It was very nice all along the way to be able to boast of my hiring skills and my training skills. Doc and I have remained close friends over the years. Those were grand days.

BOHNING: One of the things that I also wanted to ask you about was that you were starting out at a time when instrumentation was changing rapidly. In your early papers, it was melting points, IR, UV. I think it was the cyclopropane paper where you first used GC. Then you used nuclear magnetic resonance with the fluoroketones. Was Roberts consulting here at that time?

SIMMONS: Jack was a consultant at the old Orchem Department, not at CRD then. Before I came to DuPont, and when the first ferrocenes were scraped out of a stack in England and almost immediately studied and derivatized at Orchem, Jack brought some of the Orchem products back and was doing dipole moment measurements on these chlorinated ferrocenes, and he wouldn't tell any of the others what it was. He had us doing these measurements for him on the side at MIT, but he couldn't tell us what they suspected they were or what they do.

I think his consulting goes back to 1950 or something like that. I think he's been consulting at least forty years. I guess his fortieth anniversary was in 1990. He has also been consulting with CRD for many years now.

BOHNING: That's approaching Carl Marvel's record.

SIMMONS: Yes, the fiftieth. Oh my God, incredible! [laughter]

Another MIT guy here, Bill [William D.] Phillips, was a spectroscopist. Bill got into NMR very early. Varian and DuPont got together and Varian's first machine was here at the experimental station. We had a real leg up on that. We were fortunate in having Bill Phillips, who ultimately switched fields and became a molecular biologist and went on to an incredible career doing all sorts of things. Bill and I have been close personal friends for many years.

He was the guy who did so much for getting NMR used in chemistry as an analytical tool. Jack Roberts' dedication in his first little book on NMR (10) is to Bill; he dedicates it to Bill Phillips for teaching him all of this. Phillips was really on his own. Things like rotating methyl groups; it was just totally unknown that you could do these things. Bill wasn't the only one, but he was one of the major people. He had all of these marvelous fluorinated compounds here, plus the DuPont support and a machine. So we got into NMR fairly promptly.

[END OF TAPE, SIDE 3]

SIMMONS: I was greatly in awe of analytical tools and learned enough to be able to use them. I don't ever think that anyone would claim I was one of the great experimentalists, but I had some great experimentalists working for me over the years.

BOHNING: We've talked about the early cyclopropane work which you came back to later on with Blanchard, and we've talked about the fluoroketones. In 1961 you had a paper on benzyne that came out of here (11).

SIMMONS: Which one was that?

BOHNING: It was titled "Cycloaddition Reaction of Benzyne."

SIMMONS: That wasn't anything very important.

BOHNING: Then you had some other paper with Jack Roberts after you were here, on the small ring compounds. One was with Marjorie Caserio (12). I'm assuming that was just cleaning up things left over from MIT.

SIMMONS: Yes.

BOHNING: The next thing looks like the thiacyanocarbons (13).

SIMMONS: Yes. I became a research supervisor in 1959, and I ran the group that I had very much like an academic group; that is, I met with these guys at least every other day. But it wasn't to bug them; we met to do chemistry together. I never heard anyone complain about this. It wasn't a matter of the boss breathing down your neck. It was truly the kind of the thing that I think most of the guys enjoyed.

Again, in reading the literature, I came across Bähr's salt. [G.] Bähr was an East German inorganic chemist who had made this cis-dithiodicyanoethylene. I started fooling around with that, and this opened a whole new area. As a matter of fact, the first isothiocyanate ever made was from Bähr's salt. I got together with Paul Lauterbur, who did the first C-13 NMR spectrum. The first structure proof using C-13 was done with one of the compounds out of these thiacyanocarbons. I made it with the labels in it and sent it to him, and he did the NMR.

Many of those dithiins and tetracyanothiophenes looked like they might be very exciting polymer intermediates. They were cheap, because the carbon came from carbon disulfide or cyanide, and both of those were DuPont products at the time. What was so nice about this stuff is that you could take sodium cyanide and carbon disulfide and chlorine, and if you did it right, in the same pot you could get tetracyanodithiin. It is a polycyanated six-membered ring. That was exciting, if we could find something to do with these new heterocycles.

The people in my group spent a fair amount of time exploring the chemistry of a lot of these heterocycles and exploring potential uses. But we never really came up with much. There were a lot of possibilities for the dye and pigment intermediates. A lot of things went on for quite a distance, but never really scored any hits. Some pigment possibilities went on for a long time, but never quite rung any great bells. But it was lot of fun doing the chemistry.

BOHNING: It was a lot of good, basic chemistry.

SIMMONS: Yes, right.

BOHNING: I noticed Cairns' name was on the papers. Is there any significance to that?

SIMMONS: I have to tell you that there is none. That was as a courtesy author. Ted didn't contribute anything.

BOHNING: The reason I asked that is that I know that it varies. Some research directors want their names on most papers and others don't.

SIMMONS: When I came here, there was a more than not prevailing view that supervisors ought to have their names on papers. Don Coffman was an example of this. He really believed this. He honestly believed that ethically this was the right thing to do. There were many of us younger people who felt that that was <u>not</u> the right thing to do. [laughter] That was a real issue here in the late 1950s and early 1960s. I'd say that by the middle 1960s this had been pretty much resolved and the only names that got on papers were those that ought to be on there, unless the author himself wanted to add someone's name as a courtesy or something like that.

But there were some heated discussions around here with people who didn't get their names on papers, who felt they should have been there. The consensus would have been, "No, they shouldn't have." For some reason, they felt they made some contribution or they were a supervisor and this guy had been working for him, and therefore, by definition, should be on there. [laughter]

BOHNING: As a very trivial point, I noticed that in those papers you mercifully did away with Roman numerals and used Arabic numbers in identifying structures.

SIMMONS: Yes, right. Right.

BOHNING: Following [Louis F.] Fieser's example. I guess that was just about the time that started to change.

SIMMONS: There are a couple of things that I might tell you, at least from our own standpoint, since this is not fully public property.

BOHNING: You have complete control over access to the tapes and transcript.

SIMMONS: If you look at the first cyclopropanation communication (8a), you will find a very interesting footnote in there, where we referred to Doering and LaFlamme, where we were unable to understand their results. I didn't know Doering at the time. Doug Wiley is the guy who called Larry Knox to find out about this. Doug just died last year, as a matter of fact, of cancer.

Wiley told him up there, where the norcarane came from. A graduate student, Paul LaFlamme, tried to repeat our work and told Doering that he had heard that methylene iodide in zinc gave methylene. He said that when he treated cyclohexene with it, he got all the insertion products; he claimed he got the identical insertion products that he got from diazomethane. They wrote that up as a back part of a paper on carbene (14). They just wanted to get it in print before our stuff was; I saw it and was able to put in a footnote.

Since in later years Bill Doering and I became good friends, I never discussed the episode with him. I think the problem was this student. I think LaFlamme was thrown out not too long after this, so it might well have been that that's the case.

As a young guy in the early 1960s, I went to Yale to give a talk on this reaction. Harry Wassermann and Ken Wiberg were all very friendly and excited. Doering didn't make an appearance until the very end of the day, and he said, "Well, I did want to meet you." Actually, I didn't allude to any of this. But this was sort of a thing hanging over us for some years.

If you look at my footnote, it gives a reference to this LaFlamme paper on this. As you read them, you'll see that what LaFlamme did was to claim that this reagent behaved just like diazomethane. Of course, it does not. There's no <u>possible</u> way he could have gotten the results he claimed, so they had to be totally dry-labeled.

BOHNING: One thing that doesn't show up in your publication list are patents.

SIMMONS: I probably have fifteen or twenty patents or something like that. Thirteen, actually.

BOHNING: Well, for example, with the thiacyanocarbons, there were a lot of patents.

SIMMONS: Yes, there were patents with the ketals; there were patents with the cyclopropanation reaction. I have a list of those somewhere (15).

BOHNING: If you find it, I would appreciate a copy of it.

When you say that you had a patent on the cyclopropanation reaction (16), how do you publish a paper with all of that beautiful chemistry out there and get a patent at the same time?

SIMMONS: That was one we sort of screwed up on. The initial view was that this was just some exciting new chemistry. Later on, while we still had not barred ourselves from patenting, we decided that there were some real possibilities, maybe in the pharmaceutical industry and that sort of thing. So we did go after a process patent here. This was done so that our initial publication didn't act as a bar to a patent.

BOHNING: I was wondering whether one would say, "Well, I won't publish for a while, until I see whether or not there's anything worthwhile patenting."

SIMMONS: Usually, that's the case, but for some reason, this particular piece of work inspired everybody around here as exciting chemistry but not much in terms of patent possibilities. I have a feeling that if we had looked at it entirely differently—and I'm maybe even guilty of this [laughter]—we might have built an interesting series of patents around carbenoids more broadly. But we didn't really know enough about what was going on until five years after that, when we knew something about the mechanism.

BOHNING: You said earlier, you became a research supervisor in 1959, a position you held for about ten or eleven years. What size group did you manage? Were you involved in recruiting for the group?

SIMMONS: One of the things that DuPont did was to have the recruiting centralized by people like this John Reynard. CRD was unusual in as much as we did a fair amount of our own recruiting or worked closely with the employee relations department. About the time I was becoming a supervisor, we moved that function more and more down to research supervisors and chemists in the labs and let them do much of the work, but with a science director, or what we then called an associate director. They would be assigned these people as hosts to take a look at them. But the intense work of that was done by the research supervisor and the chemists. So all of us did a lot of recruiting in those days.

The average group sizes varied. Probably the smallest were six or eight, and the largest were fifteen or sixteen or so. My groups were usually around a dozen.

That period in the early 1960s was one period where I was very fortunate in getting a lot of good Orientals, like [Tadamichi] Fukunaga, who had worked for [Robert B.] Woodward. I had someone working on triquinocene, and so had Woodward. We both conceived that this bowl-shaped molecule might dimerize to give dodecahedrane.

During a visit to Harvard one time I happened to mention this to Woodward, and then he disclosed they were working on it and introduced me to Fukunaga. Tada was finishing up, and he then came to work with us. Bob and I worked jointly with DuPont funds mainly, on triquinocene and the dodecahedrane. Tada had made triquinocene up there in very small amounts. When he got here, we used our facilities to be able to get hold of larger quantities. Then we worked on trying to get it to dimerize.

That was probably the beginning of my getting together with Woodward in more detail. We agreed to do the work together. In 1968, I went up there as a visiting professor. During that half-year that I spent at Harvard, Bob and I became good friends and remained that over the years. He always provided me with very good, outstanding students, and I ended up with a lot of Orientals. At one time, they called me the "yellow peril" here because I had a predilection to having Orientals. I had a group at one time that was all Oriental. It wasn't entirely by design; it just sort of happened that way. It half happened and half was planned. [laughter]

Chung Ho Park, another MIT guy who was a Cope student, did the longest experimental thesis in MIT's organic history. He came and worked for me, and we did the work on "<u>in</u>-<u>out</u>" isomerism and macrobicyclic amines. That was another area where much of what we've done lies in manuscripts that are in drawers and have not been submitted; it's really terrible. Strangely, much of that is still submittable. We have to do something about that.

I had a very close relationship with the Orientals, who were very good co-workers. Bob [Robert Shing-Hei] Liu became a professor at Hawaii, after he left here. Bob and I worked together quite a bit. Chung Ho Park is Korean; Tada is Japanese.

BOHNING: What factors influenced the size of the group? You said it fluctuated.

SIMMONS: In the early days, the groups were entirely exploratory. In those days, we had gotten to the point of trying to make supervisors of those people who could hopefully do some important independent research on their own. It was somewhat a question of how many good ideas they had to work on, but that might be dictated by how many people we were hiring at the time. If we were hiring a fair amount of people, this would tend to make the groups bigger, even if people didn't want them to be large. Later, when we began to do more directed work, it depended on wherever the outside pressures were coming from.

BOHNING: This period in the 1960s was a very productive period, in terms of paper output. There were a lot of things happening in a number of different fields at the same time.

SIMMONS: Yes, we had several things, like spiro conjugation. Tada and I sent a paper in on it. The editor wrote me back saying that a referee said that he was working on this too, and would I be willing to let him submit a paper. It turned out to be Roald Hoffman. [laughter] Being the nice guy I am, I said "Okay." We waited a week while he finished writing his up and we put in back-to-back papers (17). We became good friends after that. There was no problem about this. We overlapped several times, where we did things independently.

BOHNING: I wanted to ask you about that, because starting around 1964, you started moving into what I'll call really theoretical papers as opposed to bench-type chemistry.

SIMMONS: One of the people here at DuPont was Rudolph Pariser, who worked for Herm Schroeder at Elastomers. I purposely went up and got to know Rudy, because I was very impressed with what he had done with Bob [Robert G.] Parr, and doubly so when I found out he wasn't Parr's student, but was probably the leader of that twosome. Rudy became my mentor.

I had a long interest in quantum chemistry, starting back in graduate school. Jack was just getting interested in it, and so was Andy. All of us were doing back-of-the-envelope sort of things, and this had a fascination which just continued to grow. Once I was here and had more time to be a dilettante and fool a little bit with all these things, I sought out Rudy Pariser. Over the years, Rudy and I became close friends. He ended up as a science director. I brought him in here in later years, when I was a director of research. He was a science director for polymers. He spent time to teach me and encourage me.

Just about that time in the 1960s, the first quantum chemistry Gordon Conferences were starting. Largely through Rudy, I had the chance to go to them, because I had published very little at that point. I had the chance to go and rub elbows with [Robert] Mulliken and all of these folks who were there. Those were just incredible meetings!

I really liked that. Rudy always owned Thunderbirds, and we would drive up to those meetings together. Often, Jan Hoytink, another fine theoretician from Holland, would come over, and we'd all three meet here in Wilmington and drive up to the Gordon Conference together. I had some great times with those guys.

But my interest grew and I did some things. Then as years passed, I got more and more interested in topology. I eventually worked with Dick [Richard E.] Merrifield and put out this book (18) trying to show the way of applying some topology to organic structure. That primarily started when I was science director. I worked on it for two or three years—not the book, but the area—when I was director of research here. Then Merrifield was looking for something to do. I showed him my notes, and he got fired up with this and joined me. He is an outstanding mathematician, in terms of real proofs; I mean, the hardcore stuff. He could do what I couldn't do. We had just a very fruitful relationship, which still continues. We retired together, and he and I are still working together on getting some papers done.

BOHNING: I was just struck by your going from good synthetic chemistry to the first paper in 1964 on "An Empirical Model for Non-Bonded Hydrogen-Hydrogen Repulsion Energies in Hydrocarbons" (19); then there was the Pariser-Parr Theory (20); and later on, of course, getting into the topology. I have to admit, when I looked at two of those first topology papers, I didn't have a clue as to what was going on in the mathematics.

SIMMONS: There is no reason you should. That's the problem with it; it's not very complex, but it's alien to chemists who have not had the sufficient modern algebra.

[END OF TAPE, SIDE 4]

BOHNING: Part of that is like learning a foreign language again. You really have to know the language.

SIMMONS: One thing we didn't talk about that Ted Cairns fostered was that by the late 1950s, there was still not much contact with European universities. Ted started a program here of sending staff members over to visit European universities and give lectures. The first time I went was in 1960; that's before the Berlin Wall, and East Berlin was still lying in ruins. The Germans treated all Americans as though we were Christ coming back from the dead; they loved us all. As a matter of fact, most Europeans did.

In 1960 I made a grand tour and met [Albert] Eschenmoser. He was a young guy who was my host at ETH [Eidgenössische Technische Hochschule]. I met [Vladimir] Prelog and met Wittig and everybody. I went to France and Switzerland, Germany, England, and the

Netherlands. I spent a month or so over there in a car with my wife, just going around and giving talks. Part of that was to invite them back over here.

This meant a great deal, because the young Germans, in those days, had no money to come to visit. So we would pick two or three young, promising academics and have them come here to give a lecture, and then pay for two or three weeks of travel anywhere they wanted to go in the United States, to give talks at the universities. Those are very popular programs, obviously.

I started going every two or three years to Germany and made close friends with a lot of these people, like Georg Wittig. These were all folks I eventually knew pretty well, who would have you to their homes for dinner and vice versa. A guy at the University of Münich, Rolf Huisgen was another person that I got to know very well, and Rolf and I still correspond. He's sort of fun because he always has a lot of gossip about what's going on in the world of chemistry.

Ivar Ugi, Privatdozent at Münich, had practically been thrown out by Huisgen when I first met him. He went off to become the head of corporate research at Baeyer Leverkusen. He then came to the United States and was a professor at USC and then went back. He's the professor at the Technical University in Münich now. He became one of our close personal friends, and our families, wives, and all get together and visit. He comes and stays with us when he's over here.

But one of the nice things of this DuPont association: it gave the time, the encouragement, and money for many young people here, like myself, to go over with an entrée to visit people like Wittig and Huisgen and Prelog. If you could bring science with you, you were very quickly very popular, because not only was it the science, but you represented an entrée to America. So those were just outstanding, thrilling days.

I can remember going to the ETH one time and Prelog, who had just separated from his wife, grabbed my wife and whisked her away with him for the morning, telling an elderly American couple who were visiting him—I can't remember his name; he was a crystallographer—that my wife was his new wife. [laughter] He kept up this craziness for the whole morning, until we all met for lunch at one of the guilds.

I got to know people well enough for closeness to develop. If I was at an American university, I would have never had the opportunities I had that came through DuPont.

BOHNING: In many respects, you had a virtual academic setting without students, as it were.

SIMMONS: That's right. Huisgen always called us the industrial university, and he always would refer to CRD this way in lectures that he gave. He was another one who followed up our work. I worked on the mechanism of cycloaddition reactions (21), some of which is not quite right, and that was because we didn't have the tools to do the NMR resolution, but some of it is correct. We collaborated with Huisgen on something he got interested in later, and then he followed up our stuff and showed that some of it was right, some of it was wrong. [laughter]

BOHNING: Did you employ any of these people you met on your European trips as consultants later on?

SIMMONS: I don't think we ever employed any of them as consultants. I think the theory behind all of this was good will and sending us good potential students. Over the years, many of these people provided a lot of students who came over here to work for DuPont. If there was a company goal, it was probably more recruiting than anything else. In any specific case, we probably would have used some of these people as ad hoc consultants or as on a special contract or something like that. But there was no program at all to have these people in anything other than what I've described. The program was to develop close, personal friendships. The main idea was scientifically driven, and if there was any company side to it, it was probably the recruiting more than anything else.

BOHNING: You've mentioned that you had people like Blanchard, for example, who was essentially your postdoc and then took off in the company. In terms of the longevity of people coming into your group, did people tend to stay, did they move to other parts of the company, or did they leave the company?

SIMMONS: CRD was a reservoir of technical talent. When I first came here and the first decade I was here, a large number of research directors around DuPont were CRD people. Herm Schroeder started there. Maury [Maurice L.] Ernsberger and Jerry [Gerald] Whitman both became director of research at Orchem. I can think of a lot of them who started here and went as a technical person to another department, then went into the businesses, because a lot of the guys had the real desire not to do science but to get into the mainstream and do things like that.

There was a fairly high turnover. In the class that I came here with, there were sixteen hired. By the way, eight were men and eight women.

BOHNING: That's interesting. Was that done purposely?

SIMMONS: I don't think so. It wasn't because we were out just trying to hire women. One of them was a Ph.D. from MIT, Laura Kaiser; as a little girl she had been in a concentration camp.

Within eight or ten years, I was the only person left of the original group. The theory at that time was that we would be an entrance port. The really serious scientists, the best scientists, the ones who were most productive, would probably earn a slot to stay here; the others would probably move through to other places in the company. Strange as it may seem, this caused less problems than you might think, because competition was fairly high with the quality of folks here. Those folks who were asked, or it was suggested that they might want to go on to an industrial department, usually agreed. Not always, but for the most part, people do see themselves more accurately than we sometimes suspect they do. [laughter]

A fair number of people, no matter what they say coming in here, actually end up wanting to get into the business. They were lured on by management, and of course, in management outside of here, many, many paths are open to you, compared to the very small number in research. So the turnover was fairly high. But even some of the best people ended up ultimately leaving, because somebody really wanted them. If somebody in the industrial department had a position that they really wanted this person and they felt they could make a real contribution, and if you know you're really wanted, then people tend to be more amenable to be drawn away. Chung Ho Park was an example. He's with DuPont Merck now, and he's done extremely well there.

BOHNING: Do you think that some of that movement out of the research end is partially because you really have to have a concerted effort to keep yourself on top of the science as it's changing over the years, as opposed to being in a business? Not that it's not changing, but keeping up with the science over a long period of time is very demanding.

SIMMONS: Yes, that is right. I certainly think that's part of it. There is no question that there are people who start out from graduate school believing they want to be a bench chemist. I can think of a fellow whom I was involved in hiring from Columbia. He was just finishing a postdoc. I can't remember where he went before that. He was a really exuberant guy and a very knowledgeable, very bright guy. After he'd been here for two or three years, he told me one day that what he <u>really</u> discovered he wanted to do was employee relations work. [laughter] I said, "Fred, that's fine. I'll be glad to help you do this." He wasn't doing a bad job here by any means; he just decided that's what he wanted to do. So I gave up trying to second-guess them. He was a [Ronald] Breslow postdoc.

I've seen other guys who left here to go to Wall Street and do something. Mostly all the ones who stay in the company transfer more or less laterally to become a research scientist in one of these departments, where they can focus on something. More often than not they're pretty good people who can end up having a very good career where they go.

Some clearly know after two or three years here that what they want to do is be director of research; the probability is low here, but they might be director of research over there. They will leave here maybe with a promotion to a research supervisor in another department. If everything works okay, they go on up. That's what happened in the past. A lot of people ended up as top management in these departments, often as the vice president of the department, too.

So for a long time we were the hiring pool. We became less and less important to the company that way by the middle 1970s. We still provide selective people of real importance to the company. But in the broad concept that we were going to be a hiring port, I think that this probably died away by the early or middle 1970s.

BOHNING: How did the company react when you started publishing papers in the *Journal of Chemical Physics*?

SIMMONS: No matter what your status is here in terms of rank or that sort of thing, there is an unwritten scientific rank. Certainly the people in the department above me wouldn't have blinked an eye at that. They would have thought about this as just working across boundaries or something like that.

Next door, we have all the bound publications of Central Research, going back to [Wallace] Carothers' day, right back to volume one. If you look at these publications in the heydays of the 1960s, 1970s, and 1980s, you'll find everything under the sun in there, every kind of thing.

BOHNING: So top management in the company would also have to be supportive to an extent, at least, in having this activity going on.

SIMMONS: Yes. In the heyday, way back, they took the view that science is great, and technology is the basis of modern American society. To make money, we want to do this through technology as a base. We will put in so much money a year as an investment in this, and we don't want to try to guess what it is you ought to be doing. What we want to do is to have good people running things and leave it in their hands. This is our high-risk money, and what we hope is that in several years, those guys will come up with something that will do us some good.

That view persisted for a long time. Then it wavered and changed as a function of the outside world, and the company's fortunes, and how the businesses were doing. There were those in the industrial departments who felt that the whole thing was a boondoggle, and that if we were going to earn our keep here, we ought to work on what they wanted us to do. We've gone through periods of having as much as twenty or twenty-five percent of our programs funded by the departments. That doesn't mean they tell us what to do, but it's a joint agreement of what it is you're going to work on. Then we'd go back down to zero. Then we may go up to five or ten percent, then go back down to two percent. So it oscillated all around.

Nowadays, the concept of a free approach to industrial science, "Do what you want to do, and something will come out of it," is pretty much dead all over the country, and it's pretty much dead here, too. Much of what we're doing are things that probably wouldn't get done in the industrial departments but are much closer to <u>their</u> goals and needs. Often we had people here who could do things they just couldn't do, like mechanistic people.

So the future of corporate research is a mystery. DuPont's heritage is so strong that the chairmen of DuPont have been loathe to do anything drastic so far. But I don't know about next year or the year after that.

BOHNING: I was going to ask how you viewed what was happening with your counterparts in places like Dow and Monsanto and so on, over the time period that you've seen changes. I guess DuPont was really the leader in this kind of basic research.

SIMMONS: Well, there was a difference in this sense. Back in the early part of the century, all research was centralized here. Very early on, Eastern Lab was built in New Jersey and the Experimental Station was built in Delaware. The concept of a centralized operation started, and that was the beginning of things. From that industrial department, research grew. For a long time, CRD supplied all of it. Then pieces would break off, and you'd go over to the Explosives Department or the Fibers Department or what have you, and they'd act as a nucleus for new ventures.

Most American industries are just the opposite. They have a plant doing this, a plant doing that, and a laboratory there. As a later afterthought, usually after World War II, people said, "Hey, we ought to have a central research department." I remember when the director of research at 3M came out to see me one time, and I told him in detail how we were structured and what our philosophies were. He went back and they duplicated our structure and even called it the Central Research Department. They had an exact duplicate of what we had. [laughter]

I'd say that the basic difference is that at DuPont everything grew out of the Central Research Department. These other companies had a central research department much later, as an afterthought. So when things got tight, it was easier to do away with it at a Shell or a Monsanto. Even AT&T doesn't do as much as they used to.

I don't know if there's anything more to say about that, except that DuPont is still keeping it alive, and I hope they do. But if things got really bad, I suppose we would have trouble too.

BOHNING: How does this affect recruiting?

SIMMONS: I haven't kept up with it closely, but I think the maximum we've been doing in the last few years is just replenishing holes that really need to be filled. There's been a slowdown in recruitment. There is no desire to drive out so many people. But this more and more is becoming budget-determined. Previously, all management at DuPont Corporate Research, Engineering, and Central Research have been determined by the executive committee. They would just say, "This is set aside for you, and now we'll work on budgets."

We had a budget, but this came by waving the hand at the very top. This is not so now. CRD competes with other staff departments for budget. This makes me very thankful; it's a process I didn't have to go through. They waited until after they got rid of me, and then they could go and do this. They knew I wouldn't do anything like that. [laughter] At any rate, poor Al [Alexander] MacLachlan and others are having to struggle with that now. He was just coming in when I left. It's in the whole company, and it's moved much more rapidly.

So Corporate Research does not have the supporters that were built into the system. The chairman, the executive committee just don't exist anymore who can provide that sort of umbrella. I don't know where that's going to all go.

BOHNING: You've already commented about the future, and you're not clear now what's going to take place.

SIMMONS: I can tell you that if business turns up, all will be forgotten. We will get tons of money and go back to business as usual. The company hasn't been doing that well, but then again, they're doing better than their competitors. [laughter] So it depends on how you look at it. There's a lot of gloom and doom around here these days that had to do with this, and it's now three years of cost-cutting and reduced production of size.

But the fact is, going back five or six years, DuPont is too damn <u>big</u>, too many people in the company. As an example, around 1985, our vice president of corporate plans presented us with a figure one day that compared to IBM, DuPont has per capita eight times as many employee relations people as IBM. Eight times <u>per</u> <u>capita</u>! This is because of this historic structure in all of these parallel, independent departments. Each has its own finance division; each has its own employee relations division. On top of all of that, there is a <u>corporate</u> employee relations division. [laughter] So if you have eight departments, you probably have eight times as much as IBM does or something like that. [laughter]

Interestingly, all of us who have run Corporate Research over the years have done a very good job about not wasting money. We could lead a much more extravagant life here, and no one would say anything about it, especially in good times. None of the heads have ever thought that was a very good idea. I think all of us were right. I don't mean that we are niggardly, but for example, people in the departments, if things are going well, they immediately find reasons to take their wives and all out and have a celebration in the Bahamas or something like that, and call it a business meeting. We never spent money for that sort of thing.

[END OF TAPE, SIDE 5]

BOHNING: How did the research people feel when there were some major changes occurring in the company—such as the Conoco deal, for example—that looked liked there were going to be some real dramatic differences in the company's outlook?

SIMMONS: I don't think there was much concern when it happened, as compared to much later. We bought Conoco in the early 1980s, and because of Conoco, the Bronfmans ended up owning a quarter of the company. That probably worried many of us in management more than it did the research community as a whole. The reason was because [Edward G.] Jefferson has just gotten in the saddle and everybody knew that research was going to be king for some years. There was not a lot of worry. We were going through expanding the station.

In that period, Central Research doubled in size. One of my jobs was to build a molecular biology component here, which ultimately became part of DuPont Merck. This was a truly outstanding group. The atmosphere in those days was very positive for research and no one worried very much about these crazy business things that were happening on the side. When economic times got harder, then people suddenly started to wonder what influence the Bronfmans did have here. Was this a good idea becoming an oil company?

Conoco is a good example. It's trite to say that oil doesn't mix with everything. [laughter] And it really doesn't. Their whole idea of life is based on very simply drilling holes in the ground. It's like a bingo game or a dice throw; you either strike it or you don't. Their exploratory money, their big research money goes into drilling holes. It's not back doing some research in the lab. They don't spend that much on that.

So they can't understand why we would work on a product that might take six years to develop; that's inconceivable to the oil company people. We tend to look at them as dirty-fingernail guys whom we admire because of their ability to make money, but they're not that exciting to be around intellectually.

My own opinion was that we should have kept DuPont and Conoco apart—run them well, but have the connection up at the top. Instead of that, these latter chairmen, like [Edgar S.] Woolard, [Jr.]—and [Richard E.] Heckert contributed to this too, unfortunately—their idea was to bring these two things together, and to bring management from one side over to the other. Every time we've done this, it's been a disaster! These guys don't understand. How can you take a guy who's on an oil man and ask him to make decisions on high-tech products? It just doesn't work.

Your question was, "What did the research people think about this?" I think because times were so rich for research, no one thought about it then. It's only been six or eight years later that they started to worry about it.

BOHNING: Let me return to your research. There were a number of good chemistry papers coming out in the 1960s. There was a paper on cyanonitrene (22).

SIMMONS: Yes, that's the nitrogen analog of CO_2, CN_2. And I did not discover it; Frank Marsh did. My contributions to that were largely theoretical. People working on it weren't very strong from the physical side, and so my contributions were more on the physical, mechanistic, and theoretical sort of stuff. But the chemistry and the conceiving of doing it, and what have you, all belong to Frank Marsh. I was a legitimate helper, but not the driving force there.

BOHNING: This is the same time that papers with Blanchard started coming out on aminobicycloalkanes (23). Then there was one on dicyanoethylene, four papers many years after the original paper (20).

SIMMONS: That's with Cairns. That's the one I was saying involved Huisgen. This had to do with the mechanism of 2+2 cycloaddition. It wasn't called that then.

BOHNING: Then there were the spiroconjugation papers. The first one was on aromatic azapentalenes, "New Aromatic Systems" (24).

SIMMONS: My contributions there were more theoretical. My old friend Rudy Carboni was the one who conceived of those. These are multi-rings, where everything is the nitrogen inside holding it together. For a while, the tetranitro derivative, the dibenzotetraazapentalene, was a high-temperature explosive used in oil wells, because you didn't get predetonation. You could get it down in the damn thing, where it got hot as hell; this wouldn't melt or decompose and you could set it off when you wanted to.

For reasons that I can't remember now, it never made any great money for the company, but it was an interesting topic. A lot of explosives material came out of there. Los Alamos was interested in a lot of these and fooled around with them. They may have even used them, for all I know, [laughter] although they never told me. I was later on their advisory council out there, in the weapons division. If they ever did use them, nobody told me about it. I probably would have known it.

BOHNING: In 1967 you had a chapter, "Theoretical Aspects of the Cyclobutadiene Problem," in Cava's book (25). I was struck by a sentence here, and I'm quoting, "The failure of chemical intuition with respect to an apparently simple molecule brought the chemist face to face with a new kind of chemical behavior." That's in the opening paragraph in that chapter on the cyclobutadiene problem.

SIMMONS: This was with Toli [Apostolos G.] Anastassiou. Toli went off to be a professor at Syracuse. By the way, we had a large number of folks who worked with me and others, spent two, three, or five years here and went off to the university; some to fame, some to at least a happy career, anyway.

Yes, I remember that, now that you mentioned it. I can't dredge up precisely what prompted that. I wrote that, and I'll have to think about it. Obviously, it wasn't that deep, was it? [laughter]

BOHNING: You went on to say that cyclobutadiene proved to be a testing ground for theories as well as experiments, and that it proved to be rich in both areas.

SIMMONS: Yes. Actually, cyclobutadiene was one of Roberts' earliest loves. He was very interested in that. Of course, that got Andy and me and others interested in it too. That chapter is probably a fair amount out of date, because a lot happened not too much later, with regard to generating cyclobutadienes of various types.

BOHNING: In 1968 you spent the year at Harvard. That was when you collaborated with Woodward.

SIMMONS: Yes. I taught Bartlett's course in advanced physical organic for him—or my version of it. I saw a lot of Woodward. We had dinner one night a week together, just the two of us. He had reestablished the Woodward seminars, which were very popular, and I went to those. The Bartlett and Swain seminars were still in operation. That was a fairly exciting time. That also was the period where [Martin Luther] King and [John F.] Kennedy were killed. That was an exciting time to be at Harvard. Harvard Square was lit up after that.

BOHNING: I happened to be living in Boston in 1963 when Kennedy was assassinated, so I can understand that.

SIMMONS: I had been down to the shore, when Martin Luther King was killed. I didn't get back until really late. I can't remember where I was or who I was with. It's probably not worth repeating. I got home at maybe two or three o'clock in the morning. At six, the phone rang, and it was Woodward to tell me that King was shot. Bob and I talked about politics a lot, and I'll never forget that. He woke me up, and I could barely think straight because I had been in bed about two hours.

BOHNING: What were his political inclinations?

SIMMONS: I think he was fairly conservative. I think he was fair. I don't think he was biased, in the sense of holding bizarre social views or things like that. But I think he was certainly a political conservative. He <u>really</u> didn't go for the things that were going on in the 1960s with students. Then again, neither did I. I was a lot younger than he was, twenty years younger than he was. I thought it was not my cup of tea.

BOHNING: That year that you were there would have still been in the midst of all that. Cambridge was certainly a center of student protest.

SIMMONS: Oh, yes! Yes, during that time, first King was killed, then [Robert F.] Kennedy was killed.

BOHNING: What you had was really like a sabbatical leave. Did the company have that policy?

SIMMONS: Yes. Paul called and asked me if would I come up and be a visiting professor there for a year, and that they would pay so much. We had a program here that would allow you to do that, with management's concurrence. That was just so somebody that was at the bottom of the staff didn't wangle a deal to go to Podunk U. with a buddy of his, and where we might not want to be bothered with it. I think practically all of the requests of the people who were asked to teach, or to be a visiting professor, were acceded to.

The company was very generous. They made up the difference. Harvard paid me, and then the company paid the difference to bring this up to my normal income, and they paid all the expenses, including room and board.

BOHNING: Did you take your family with you?

SIMMONS: No, I came home every couple of weekends or something like that.

BOHNING: Were you at this time doing any experimental work with your hands, or were you mostly doing paper chemistry?

SIMMONS: As an associate director—or science directors, we call them now—and as director of research, I kept a group. I was probably the only one in history here who actually kept a research group, with a group leader overseeing this who reported just to me. I did that up until 1979, when I became department head or a vice president.

At that period, I was doing nothing with my hands. I have to confess, I dropped out somewhere in the 1960s of doing anything on my own. I had a little lab, and I use to continue to putter around. After that, I spent a fair amount of time in research, but it was all theoretical things. I did a lot of computing. For a long time, I was hiring a half-time mathematical assistant, a programmer, from the engineering department. These guys or girls would get excited with what we were doing.

In those days, for our purposes, we couldn't run it when we wanted to. We might have to run it after midnight, but we might want to be there to see it. It was not uncommon for me and whoever it was to go down to Louviers, down in Newark, at the big computer center, and be there at one o'clock in the morning to run batches of stuff. I did a lot with my hands, but it was with pencil and paper or computer; it was not in the laboratory any more.

BOHNING: I was really struck by how you moved into that area.

SIMMONS: Well, mathematics has been the driving force. Like many people who don't have any real talent there, they do have a love for it and a desire. This is why the collaboration with Dick Merrifield was so good. It was because I provided a lot of intuition and he provided a lot of rigor. We just had a great time working together in this.

BOHNING: What's his background?

SIMMONS: He's an MIT spectroscopist. I didn't go out and seek Dick. Dick was a supervisor here himself at one time. He was the father of exciton theory in organic crystals. That Merrifield. Dick had gone back to the bench, and the last six, eight, ten years here, he chose just to work with me. We both did theory together, which was a nice life for him. He had the boss' protection. [laughter] I kept telling him, "Richard, you'll get normal raises and that sort of thing, but you can't get the kind of treatment that you really ought to be getting if you were doing something outside." He said, "Absolutely not." He said he was enjoying life too much to worry about whether he was making an extra buck here or there.

BOHNING: The work with Park on the macrobicyclic amines was also at this time in the 1960s.

SIMMONS: Yes, that's the case where we literally do have important work that is unpublished. Once a year or twice a year, Chung Ho and I look at each other and say, "God, we have <u>got</u> to do this." Nothing much ever happens.

BOHNING: I think there was at least three or four papers on that topic (26).

SIMMONS: Yes.

BOHNING: In 1970, you became associate director of research.

SIMMONS: Yes. They're our science directors today.

BOHNING: You had said earlier that these promotions came as a surprise to you.

SIMMONS: Literally, inasmuch as I hadn't been thinking about them, or contemplating them, or wishing for them. It was always a bittersweet feeling. It's impossible not to have a good feeling about it, but I <u>always</u> had a sinking feeling—literally, a sinking feeling—to say I'm getting this much further away from science, and that's not where I really want to be.

During the middle 1970s, after the oil crisis, and during [Irving S.] Shapiro's reign here at the company, research wasn't one of the top things. All the company was doing was looking to shoring up existing processes and not spending much money on new products. Irenée du Pont used to come out. When I was up here, running this job as director of research—now it's called vice president of R&D—Irenée du Pont used to come in here. I'd gotten to know Irenée from one thing or the other, and he would come out. He'd be waiting for me at eight o'clock in the morning, and the gist of this was to say, "Hang in there. Things are going to get better; don't worry about the way things are going."

When Cairns got ready to retire in 1979 or so, Dick Heckert, who was then on the executive committee and a past graduate from here—this was just at the time Jefferson was going up—came out to see me. He said that the executive committee wanted me to replace Cairns. When Cairns left, I was to jump over the assistant department head—at that time, we had an assistant department head. His name was Monroe Sadler, who used to be the director or the vice president of the whole development department, which was a business group. He told me quite frankly, "Monroe is not going to get the job, but we want you to do it." Also he said, "Some of the people on the committee think it would be a good idea if you went out for two or three years to an industrial department." I said, "Well, I'll think about it." He looked really funny, but he said, "Well, let's talk about it a week from now."

So I went downtown to see him a week after that. I told him, "Dick, I've thought it over. I really like being director of research here. It's a really super job. It's still close to the science. Thank you, but no thanks! I'll stay here." He looked at me, and then he burst out laughing. He said this was the first time to his knowledge in DuPont that anyone had turned down a vice presidency for a department head. [laughter] I said, "Dick, I love you guys, but there's got to be a first time for everyone." [laughter] This occurred about a year or a year-and-a-half before Ted retired, something like that.

When Ted retired, Jefferson called me downtown and told me that I was vice president of the department. [laughter] So I never went out to an industrial department. I reported directly to Jefferson in those years.

BOHNING: You also did a little bit of crown polyether chemistry.

SIMMONS: Yes, it was interesting, because I didn't even know Charlie Pedersen.

BOHNING: I was going to ask if that came out of his work.

SIMMONS: I didn't even know Charlie [Charles J.] Pedersen, but in talking to Jack about this work Chung Ho and I were doing, he asked me, "Do you know Charlie Pedersen?" I said, "Who's he?" He said, "He's in elastomers. He is making these really incredible ethers, and he's going to publish on them soon." I said, "No."

So I went up to meet Charlie. I was really impressed with his work. There's a side tale to this. After we had exchanged some chemistry and what have you, I said, "Well, one of the things that we really ought to do is incorporate ether linkages into our microbicyclic amines, because these look like they'd be incredible complexing agents." I'll never forget what Charlie said. He said, "I'd be honored if you want to do it." [laughter] "I'm just going to work on the monocycles. I've got plenty to do with that. If you want to put ethers in your bicycles, that's great."

Chung Ho did that. Just as he was finishing this, we were in contact while I was on a trip to Europe. I went to Strasbourg for the first time. There was a young faculty member there, Jean-Marie Lehn, whom I didn't know and didn't know of. I gave a lecture and my last slide was this cryptate. He came up to me after the lecture and said, "Dr. Simmons? I didn't know whether I should meet your or not, but I think I ought to show you this." He had the galley proofs from *Tetrahedron* for this synthesis. [laughter] He said that he had read the communications from Charlie's stuff and my stuff and he decided to put them together. I said, "Well, that's fine." [laughter] You know, it's one of those things in life that happens.

We also went on to become great friends over the years and visited each other. When he won the Nobel Prize, as others will readily attest, he called me from France twice to make sure I wasn't upset by this. When I got the Chandler Medal a couple years ago from Columbia, Breslow was recounting some of the things that led to Jean-Maries' work and to ours, and he very graciously told everybody I got screwed, that I should have shared this prize. [laughter] We made the barium complex also.

So that's how you get close to fame. [laughter] I did a few things with crown ethers, like wanting to look at reagents, like potassium permanganate oxidations (27), how would they go and stuff like that. Another oriental was Don [Donnie J.] Sam, a Chinese who went on to do great things in DuPont; he's done very well in DuPont.

[END OF TAPE, SIDE 6]

BOHNING: Did you have much more interaction with Pedersen then?

SIMMONS: No. Charlie and I knew each other, but not really closely. Of course, I greatly admired what he did. The only thing I ever wrote up were these permanganate experiments. Donnie Sam was a new employee at the time. I discussed this with him, and he was very interested in giving this sort of thing a try. In all of this stuff, we kept in close touch. I wasn't doing anything with Charlie except to let him know what we were doing. He was just the opposite. He wanted as many people as he could find to work on this. That's how I usually felt too. I never wanted people not to work on my things. I'd rather have people working on what you're doing than not.

Charlie retired not too long after that. He went off to do some crystallography in England after he retired, to look at these crystal structures. He had a visiting professorship. As I said, I didn't even know what he was doing in elastomers. This can happen in DuPont if you're not publishing.

BOHNING: Then you don't have much interaction with the other groups then, as such?

SIMMONS: Oh, we do, but they're just so many groups. Charlie was in a polymer department and probably any connection that our people would have had with Charlie might have been with some polymer work. But at the time, that department was being very generous to him. They recognized that he had some important findings and they encouraged him. He was given the time and wherewithal to bring this stuff to a conclusion.

BOHNING: There was a paper on acetylenedicarbonyl fluoride, in which there was a note that said <u>caution</u> and pointed out that it was toxic and corrosive (28). There was another one where you had a note about something that caused skin lesions (13). That sounds like it came out of personal experience.

SIMMONS: Yes, that was back with the thiacyanocarbons. That was this first isothiocyanate. As I said, it was the first compound on which the C-13 structure was ever done on. The Army was looking at this intently, as an incapacitating agent. If you ever had a bad dose of poison ivy, you would know what it's like. This was like this, but much worse. It was like poison oak or poison sumac. It was really awful. Dilute solutions of it got everybody; you didn't have to be very sensitive. Almost anybody who worked around this, who got it on them, got this very bad reaction.

The Army showed a lot of interest in this for a long time. It might even be in use somewhere, for all I know. It was bad and I was sensitized to the point where for years, if I opened one of my old notebooks, this whole triangle around my mouth just lighted up red. All I had to do was just open up the lab book. So we're really talking about picopicograms.

BOHNING: That's incredible.

SIMMONS: It took over a decade before I felt reasonably safe around it.

BOHNING: In the acetylenedicarbonyl fluoride, you were again dealing with materials which are toxic or corrosive.

SIMMONS: For instance, with acetylenedicarboxylic acid, you might make a bis-anhydride, which would be a new oxide of carbon. We were looking at it for that and as a polymer intermediate. They were able to make it, but nothing extraordinary came of it.

BOHNING: There was a paper in *Helvetica Chimica Acta* on bishomoquinone (29), with [A. S.] Dreiding and others. How did you made that connection?

SIMMONS: I was visiting ETH and got to know those people very well. It was something we collaborated on, but I don't think we played a very large role in that. This was largely Dreiding's work. One of the people I knew best there was Eschenmoser. Both of us have wives named Elizabeth, and we saw a lot of each other over the years. Then he turned out to be a great collaborator of Woodward's and close to Bob. Jack Dunitz was another one there, a crystallographer, a Scotsman. And Duilio Arigoni.

When I bought the house we're living in now, the man I bought it from was a Swiss living in this country, who had Arigoni's children here living with them while they were going to school. [laughter] He was an Italian-Swiss industrialist. We bought our place from him. The world is very small.

BOHNING: I don't have a full title here, but there was a paper on a tetraazatridecane (30). What I was struck by in this one is that in water the rearrangement made all the protons equivalent magnetically.

SIMMONS: Oh, yes. That was with Jack Richman. That paper won the Best Paper of the Year award with the Delaware ACS Section. They never gave it to me for the Simmons-Smith reaction or anything that was really good. [laughter] They gave it to Jack Richman and me. This is this tetramine. As you can see, polycyclicamines were on my mind at any time. That's a very interesting compound, and that has fostered a fair amount of work. Again, I don't think anything world-shattering has happened with others. But it was nice, a very pretty piece of work. It tends to be what a lot of the things that I've done are; there's often more beauty in them than there is value or utility. That was one I thought was very pretty.

BOHNING: But if you had been an academic chemist, that wouldn't have made any difference, and it would have taken that onus of utility away.

SIMMONS: Yes, that's right.

BOHNING: Also at this time, you started at Delaware as an adjunct professor. Do you still continue there?

SIMMONS: I'm still on there, at their request, but I don't do much for them anymore. I used to teach a special topics course for graduate students, usually an evening course. I trained a Ph.D. with one of their faculty down there. The guy would spend a year here in our lab, which is tremendous, because he had a guy like Fukunaga as a lab mate. There are not too many grad students who can do that. Then he'd go back to Delaware for a year and come back to us for a year. One of the departments at DuPont hired him. He did extremely well at DuPont, and just last year or so, transferred into this department here. [laughter] So, I've produced one Ph.D. down there.

BOHNING: I was curious if you had been able to do that. Well, in 1974 you became research director and I guess we've already talked about that, although you had some more theoretical papers. You had an orbital symmetry paper with [Joseph F.] Bunnett (31) and then you had a paper with George Hammond and Jack Leonard (32).

SIMMONS: Yes. This was on the structure of cyclohexane itself. This was some conformational analysis, something that we both did independently. We just happened to find that we had done the same thing independently, so we just put it together. It turned out not to be anything of any great value, but George liked it.

By the way, the person I probably got the most encouragement from early on in that cyclopropanation was Saul Winstein. He was just looking at homoaromaticity, and this reaction gave him the opportunity to synthesize some incredible structures to test out a lot of his theories. I spent a lot of time on the phone; every time something didn't work, he would call me up. This was a great thrill, because I was a pretty young guy. To me, I thought Winstein was another one of these next-to-God people. He was deeply impressed by the reaction, but more impressed than anything because it was a tool to do all kinds of things he wanted to do. He and his students made very heavy use of it and popularized this, which played a big role in helping to get the word around. I remember the day; it was a tremendous tragedy when he died. He was a guy I liked very, very much.

BOHNING: In 1978, you went to the University of Chicago.

SIMMONS: I was the first industrial Kharasch Professor they had there.

BOHNING: Did they ask you, or were you looking for something?

SIMMONS: A Kharasch professorship is a one-year visiting professorship and is awarded as an honor. Most people come and stay a month or two and give some special lectures, which is what I did. I think there have been about fifteen or sixteen Kharasch Professorships. In their terms, it's a high honor to get the Kharasch award. After Kharasch died, his wife established this visiting professorship. It's more in the nature of an award than it is a visiting professorship. I was the first industrial person that they've given it to.

BOHNING: Then your work with Merrifield starts, at least in the publications.

SIMMONS: Yes.

BOHNING: Was it in 1979 when they broke up the development department and part came to CRD?

SIMMONS: No, it had already happened.

BOHNING: Okay. That was in 1974.

SIMMONS: That's right. By that time, we had taken over the Haskell Laboratory of Toxicology and Industrial Medicine. The development department merged with CRD. The head of development was Monroe Sadler, who was here in CRD when I first came. Then he moved out and up to become the head of the Development Department. He and Cairns never got along. They never saw anything eye to eye. I liked both of these people very much. They were different kinds of characters. When they merged the departments, they made Ted the head of the combined department and Monroe the assistant. When Ted got ready to retire, that's when Heckert came to me and said they wanted me to take over, and that's what they did. So Monroe worked for me for a year—uneasily. It wasn't comfortable. He left at an early time; a tremendous loss, in my opinion, to DuPont.

BOHNING: [David] Hounshell and [John K.] Smith state the following (33): "By supporting Simmons, Jefferson calmed the nerves of DuPont scientists, who feared that research was no longer viewed as the mainspring of the company."

SIMMONS: As I've said, everybody knows where I'm coming from, from the science end of it, and I think most of the department feels comfortable if there is somebody like me, or others like me, that have an outlook like that or Cairns or others who have been there. By the late 1980s, things were starting to get rocky. I don't know when David was applying that quote.

BOHNING: That was in 1979, when you became director of Central Research and Development.

SIMMONS: That was still in Shapiro's reign. That was a down period, and research people were concerned about what was going to happen. Some of Shapiro's talks had included statements that didn't endear research people to him. Let me say, however, that I always admired Shapiro very much and got along well with him.

BOHNING: When you were put in that position, did you have any specific agenda you wanted to follow?

SIMMONS: Yes, I wanted to do what we were already doing. [laughter] But then Jefferson got in the saddle a couple of years later. As soon as he got in the saddle, research became king again. It was that first couple of years, when Jefferson was an executive committee member, that I was reporting to him. This was the end of the Shapiro reign. But things were certainly much better in 1980 and 1981 than they were in 1974 and 1975, with the oil crisis on and the uncertainty there.

You know, Jim, in many ways, I had it easy because the tough decisions were the ones that are having to be made now. Much of my reign coincided with Jefferson and Heckert, who were great research supporters. That first couple of years were under Shapiro. Shapiro wasn't a problem; he just wasn't a research guy, and his eyes didn't light up like a Jefferson's would. That's the thing that inspired the troops, when the boss really understands what the hell they're telling him. But those weren't really bad years in the sense that Shapiro was going to do anything drastic about research. It was that the morale was low, because people were just still not sure where in the hell they were going.

BOHNING: As director of research, how many different groups did you have reporting to you? Were you still organized the way you were before?

SIMMONS: Yes. We had organic, inorganic chemistry—really organometallic chemistry—catalysis, a big biology group, and physics. Under Jefferson, Central Research here just about doubled in size and we built this big building up at the end to house molecular biology and biochemistry.

BOHNING: He was the one who was instrumental in moving in that direction, wasn't he?

SIMMONS: Right. Jeff and I went up to Harvard and had dinner with the dean of the Harvard Medical School and with Howard Johnson, who was the outside chairman of the corporation board. This was around 1981. We went to talk about the future of biology and molecular biology. Out of that grew a friendship with Dan [Daniel C.] Tosteson who was the dean of the Medical School. We ultimately supported Philip Leder up there, to the tune of about $1.5 million a year for several years, to establish a department of human genetics.

A lot of us had been prodding Jefferson in a sense, but he was the one who took the bull by the horns and did something about it. Of course, he was in a position to do something about it.

BOHNING: As director of research, what was your mode of operation with the different groups that were reporting to you? Did you meet with them weekly? Did you give them much freedom?

SIMMONS: We had a weekly meeting of the science directors. We had two days a week—sometimes one day a week—a research review downstairs, which was open to everybody, and they rotated through the various science directors. There might be a review on polymer chemistry, or exploratory organic chemistry, or plant science, or whatever it might be. Following that more formal review, I would meet upstairs here in the conference room with the science director and any of the people who talked that day and the other members of the group who didn't talk. We'd have an informal session for the rest of the morning, where they would talk about things that they didn't have time to talk about in the formal session, or maybe it wasn't as complete, and they wanted to tell you about it separately.

So I'd keep up with the people this way. Once a week we had staff meetings with the science directors. This was a combination of science and personnel items and finance items or whatever, but not to exclude scientific angles, too.

BOHNING: You became vice president in 1983.

SIMMONS: Nothing really changed. I was really vice president in 1979, but we were called "director" then in CRD. Central Research had a responsibility, in dollars and people, far exceeding some departments that had vice presidents. We had always been called, since the nineteenth century, "the director." [laughter] So to regularize things, they made the head of Engineering and the head of Central Research vice presidents. Nothing changed at all, except for one thing. We became corporate officers at that time, because you can't use the title vice president such and such at a certain level, unless you are a corporate officer. So in 1983 the only thing that really changed was to literally become a corporate officer, but not salaries or anything like that. They were all the same. So they abolished the old concept of the director.

I think they regularized a couple of other staff departments too at the time, if I remember correctly, but it was more in name than anything else.

BOHNING: During that 1980s period up until 1990, what would you say were the major accomplishments of CR&D?

SIMMONS: I think <u>the</u> major thing in the 1980s was the life sciences. We had an outside board that consisted of Dan Tosteson, Bob [Robert A.] Weinberg, from the Whitehead Institute at MIT; the neurobiologist [Floyd] Bloom. We had maybe ten academics, and these were all top people in all sorts of areas that we were interested in. We worked with them; we met with them three times a year. It was almost impossible to get that much of their time, but they all came to every one of these meetings. They were well paid, but nevertheless, they spent a lot of time on this with us.

They had two jobs. One was to look at and comment on and provide advice on DuPont's Pharmaceutical Department and to Central Research's build up of a modern program in molecular biology and modern techniques. At the end of several years of all of this, at their last meetings, they provided a final written report, saying that the quality of people who we had pulled together in this over the years, like Mark Pearson and so many of them, they would take on their own faculties anytime, and that it was the outstanding molecular biology organization in industry in the world, as far as they were concerned. These were guys like Weinberg who were saying this.

I think we felt that we did a good job on this. Al MacLachlan, who was my assistant through a good deal of this and is now the senior VP—after they restructured again entirely—has my job plus a residue of engineering, but not much different than what I was doing, except having that residue of engineering. Al and I spent an inordinate amount of time on the life science related things, because we were building big new buildings and we were hiring so many people, and we had to put this much time in it.

[END OF TAPE, SIDE 7]

SIMMONS: I'd say that was the major thing we accomplished. During this period, we also made some of the seminal discoveries in the modern superconductivity business and are still going great guns at this. We have a three-way joint venture with Los Alamos, Hewlett-Packard, and DuPont, which is doing great. The science that came out of this was really good. Our solid-state program that invented whole new classes of frequency doublers still had some real commercial promise.

Most of the new chemistry and new processes that have been developed for Freon replacements came out of Central Research. These are programs that are being put in place right now. There were a whole lot of specialized things, like a close to room-temperature, atmospheric-pressure synthesis of hydrogen peroxide from the elements. It always seemed to me that the way to make hydrogen peroxide was to take hydrogen and oxygen, H_2+0_2, giving H_2O_2. We actually did that, and a pilot plant has been built to look at that. That was one that I pushed for years to try and get through, and one Jefferson strongly supported.

BOHNING: Was that primarily a catalyst problem?

SIMMONS: Yes. There were a fair number of things going on then. We're making about fifteen million dollars added right straight to the bottom line, in angioplasty catheters, a new type of polyester that guarantees no burst problems, and that sort of thing—also, an ultra-high molecular weight high-density polyethylene that has been given some additional treatments to toughen it for use in hip replacements for orthopedic devices. The sum total of those two things that CRD is doing itself is adding fifteen million dollars directly to the bottom line.

We came up with the second, but the best—better than Lee [Leroy E.] Hood's concept of reading DNA sequences and the Genesis 2000 sequencer. When we couldn't get enough appropriate help in DuPont's pharmaceuticals, we took the floor of a building here at the station and set up a manufacturing facility and made the first ten machines ourselves. Then that department took it over, and the boob that got the job took the first devices and sold them to Communist China and Japan. I would have given the first ones away to Paul Berg. [laughter] The big thing that you want is to get these top Nobel people to say, "This is the way to go." Well, once it was out of our hands, it was out of our hands. But that was an invention that came entirely out of here. It involved some great new chemistry. So there were a lot of things that have looked pretty good. I probably missed some of the big ones.

But we realized that our earnings picture was not getting any better. Over a long period of time, it was dwindling down and down. This got people's attention more and more towards the late 1980s; the early 1990s basic research took more and more of a shot in the chops. That's as it stands right now, as we started out in the beginning, saying that it's not clear where we are going. I don't have the slightest doubt that if the company has an upturn, research will share in that right away.

BOHNING: In 1990, you had a change in your position, but it's not clear to me exactly what was happening.

SIMMONS: That's right. Normally, there's an unwritten law—and I don't know of cases that it hasn't been applied—that you can stay on the executive committee or as a department head for ten years. And after that, they want you to go out. I'd been department head for about twelve years, something like that, and they were getting ready to make a restructuring change here. What could they have done? Rather than go through me being promoted, and going through this change, and then my staying for a year or something like that, I made a suggestion. I could see that Blanchard was stumbling around, wondering what to do, and I said, "Hey, look. Why don't you just put me on as your science advisor, and I'll do that for a while, because I don't intend to stay very long, anyway."

Nothing at all changed, except that Al took over the CRD. I had been getting sort of the best of both worlds, where I reported to Blanchard for a year or so. Then Doc was going to retire, and he retired a couple months after I did. I always said sixty-two, and I was sixty-two and a half.

The big thing is getting out of the main line. Once I got out of that, I would much rather be doing this. They pay me a retainer, and I can come and go as I want. It's the best of both worlds. [laughter] The big change was leaving as department head, and as I said, I had put in about twelve years.

They were going to make major changes in the way that things were done in the company. They were very nice about it, though. They retained me as a corporate officer during this whole period. I could have stayed on if I wanted to stay on there, but I chose to get out of it. I was not unhappy with any of this. I don't want you to think that I was dumped out; it wasn't that. But it was clearly going to be a new kind of DuPont. I'd been sitting on that committee up there all this time. I didn't agree with a lot of things that the chief was saying. It made me feel less comfortable, knowing that they were going to have a new attitude towards research, and I didn't want to have to cope with that. I was really very pleased with the way it all worked out.

BOHNING: What do you do with your time now that you come and go as you please, as you said?

SIMMONS: I am involved with local things. I was heading a committee overseeing the building of this new chemistry building at Delaware. For the last six years, I was president of the University of Delaware Research Foundation, which is a private philanthropic group that raises money. Actually, we call for proposals, and young faculty make grant proposals to us. We have a committee of scientists who go through these and make the recommendations as to what we're going to support and what we're not. We normally have about three hundred to four hundred thousand dollars a year that we divide up between fifteen or so faculty members. These are usually all new faculty, and this gives them a head start.

That program took a fair amount of time. I'm very active on a school board here, an independent school, which is where I'm going this afternoon. I'm on the board of the Franklin Institute; I'm seeing you folks. I'm up there once or twice a month, it seems like. I'm on the steering committee for the Corporate Council of Math and Science education at the NAS. I've been permanently on the finance committee at the Academy for some years. This is my third year on the National Science Board. There are other things for the NSF. There are some visiting committees, like MIT's, where I've been on it for twenty-some years.

The company likes all these sorts of things and thinks that it should have somebody wired into NSF or the various government agencies or the Academy. DuPont has done a lot with the Franklin Institute over the years. This gives someone to do that. These were all things I was involved with one way or the other beforehand, so I'm just continuing along now.

I've collaborated a lot with Carolyn Thoroughgood, the dean of the University of Delaware's College of Marine Studies. I'm a real booster of the marine studies college down there. The same thing at Maryland. I'm on the advisory council for the University of Maryland's marine biology lab. If you add up all of those things up, they're all labors of love, mostly. They're sort of fun to do. Some of them are even useful. [laughter]

BOHNING: I understand your sons work for DuPont. Did you have anything to do with that?

SIMMONS: In the opposite sense. Both of them went to MIT as undergraduates. One was with Woodward when he died, and was one of Woodward's last graduate students when he got his Ph.D. The other one worked for Wiberg and got his Ph.D. at Yale. The older one, in particular, had worked one summer here in the summer programs that used to hire a lot of the children of employees.

He really liked this station. He had a good experience working for the old Polymer Intermediates Department then. He wanted to come to DuPont, and I warned him again against it. Interestingly, the younger one had also worked two summers here. He swore he was <u>never</u> coming back to Delaware. He was the one who went out and interviewed with a whole pile of companies. He came back and said, "Dad, none of them are like the Experimental Station." [laughter] I said, "Well, it's up to you."

I was with Shapiro the other night, and he was asking me about the kids. He was saying that he absolutely refused to have his son work for DuPont. [laughter] I said, "Well, it's a little different with you, being the chairman." But he said, "I would imagine it's the same with you." I said, "That's true." It's a two-way sword. Some people hate you for the fact that your father was in general management in the company, and others are inclined to react just the opposite. I think that by now they feel pretty good about it.

BOHNING: I'm curious about their both being chemists, because my experience is that very few of the people I've interviewed have children who followed them as chemists.

SIMMONS: Howard started in physics and John started in biology. Howard found the physics too hard, and John found the biology too soft. I'm serious; this is really how it happened. They both ended up by coming to chemistry. [laughter]

Howard is very physically inclined. He spent almost ten years in the Photo Products Department working on optical discs. This is the Woodward guy. He's very interested in high-tech recording of optical data and that sort of thing. The younger one is a polymer chemist and has done extremely well, patent wise, with the polymers department in polyimides and membranes. Once I left the company, they were looking for a guy with optical background, and the older one jumped at the chance to come over here. And so he's over here now, actually in CRD.

BOHNING: Did the company have any policy about children of employees working for the company?

SIMMONS: Oh, yes; we keep them out of the same department. After I retired, then it was different for them. But that all started to go by the board, because at one time we wouldn't allow a husband and wife in the same department. That broke down in time. There were a large number of these family arrangements throughout DuPont, and DuPont sort of likes that. I don't know that it's done any harm. I think DuPont likes it because they think it breeds loyalty.

BOHNING: Do you consider DuPont to have a family atmosphere to it? Dow talks about the Dow "family."

SIMMONS: I think it absolutely did. That was the keynote of DuPont. I think this is what's been lost in the last two or three years. If you talk to the average DuPont employee, downtown or out here or anywhere else, they will claim that the paternalism that so characterize DuPont has disappeared.

BOHNING: And, as you said, it breeds loyalty.

SIMMONS: That's right. Speaking of one thing we didn't cover, probably one of the things I've been prouder of than anything else in the company is recruiting. I have done a lot to bring good people into the company. There are some oddball cases. Ed [Edel] Wassermann was at Bell Labs and became a professor at Rutgers. Then he became head of corporate research at Allied Signal. Ed and I are old friends. I use to visit him at Bell Labs all the time. I'd go there to give talks and vice versa. I had lunch with him one day in Washington at the Watergate, and after a couple of hours he said, "Hey, how about getting me a job? I'm tired of it up there. I'd like to have a small group, and I really want to throw myself back into science," which didn't surprise me at all. So I said, "Absolutely." So Ed came down here, and he is like a science director without portfolio. He's got physicists and biologists, and he's doing one thing or the other.

There are not many black quantum chemists around. I brought Fred Van Catledge, who was on the faculty at Minnesota, here. Over the years, when I was director of research, I saw every candidate who came through here, and the average director of research in the company doesn't even remotely put that kind of time into it. I saw every candidate that came through, even though it might be only ten minutes.

BOHNING: You can tell a lot in those ten minutes, though.

SIMMONS: Oh, yes.

BOHNING: Is there anything else? You just mentioned one thing that I haven't asked that you wanted to include.

SIMMONS: I don't know Jim; I'm just trying to think. I'm sure the answer to that is yes.

All right. While you were asking about other activities, one of the things that I spent a fair amount of time with in the Academy is the committee that worked on *Prudent Practices in the Laboratory* (34). After surveying all of these universities and company manuals, we concluded that the DuPont safety program was so good, why in the hell don't we just write the book from that? [laughter] Which is what happened. That's the biggest best seller the Academy has had, of any of their books. OSHA later revised their recommendations and strongly endorsed the study.

BOHNING: DuPont's always had this corporate image throughout even academe, of being so safety conscious.

SIMMONS: Yes, it truly is. I also spent a lot of time on the Academy program on scientific misconduct. When I get involved in some of those things I may sometimes spend more time than I normally would, and that was true in this case. I probably spent a little more time than I normally would.

BOHNING: We see a lot about scientific misconduct now; there are a number of very famous cases. Is it as much a problem in industry as it is in academe?

SIMMONS: No, that's the interesting thing. The kind of things that you see in academia, plagiarism or scientific falsification of records, none of this happens very often. Occasionally, it does. It behooves no one to falsify data, because if it's of any value at all, it will probably get repeated and then you're doomed. We did have a case a few years ago that I was involved with. There was a guy here who published some other people's data from another department, and that got us very upset.

There's just no question about it. There is very little of that sort of thing that goes on in industry among the scientists. I think they just live in an atmosphere, where if you're in a big company, a decent company, you're going to get booted out on your ass. You're not just going to get into an argument with the provost; you're going to be hauled out on your butt. We don't take lightly to these sort of things.

Now, there's a different kind of thing, where someone falsifies records so that they get a product out, and the product does damage or something like this. But I'm talking about just back at the science lab, with the bench scientist and the bench engineer. The kinds of things that we're talking about in the Academy study, I think the atmosphere in industry doesn't really foster that. The kinds of competition that you experience in the universities are so high that I suspect that has a lot to do with it.

People here have different kind of pressures, but they're not ones that engaging in scientific misconduct is going to get them to the end. Maybe if it did, they would.

BOHNING: I can imagine in industry, getting caught doing something like that would make it difficult to get a job anywhere else. Does that kind of thing go on?

SIMMONS: I don't think so. Unless you have brought this person to court and formally charged them, I think you're in deep trouble if you fire somebody and then somebody else calls and says this guy is looking for a job and you say, "Hey, I got rid of him because he's a thief." I don't think you can do that. Our legal department would shoot you if you got involved with that sort of thing.

Most of the crime I've seen associated with the chemical industry has been just plain <u>crooks</u>! Some guy in middle management has found a way to capitalize on something his department is selling, and he's working with another outside guy, and somehow they're splitting some deal that they've got going. You see that sort of thing. Once in a while, we've had environmental problems, where, unbeknownst to even their middle management, some guy lower down has consciously violated an environmental procedure. We had a case like this that was really embarrassing. There was a case in a plant, where a low-level supervisor thought he was really doing the company a great favor by dumping something where it shouldn't be dumped. Jesus! It's the last thing DuPont wants, you know. [laughter] So the poor guy not only has the federal government after him, but DuPont is not ready to give him anything, except the boot!

BOHNING: If there is anything else you would like to add, I'll be glad to keep going.

SIMMONS: No, we've probably exhausted ourselves.

BOHNING: I'd like to thank you for spending the time this morning. I've enjoyed it very much! We'll send you a transcript, and then maybe you can be prompted into adding some other things along the way.

SIMMONS: I've seen the transcripts of things like this. I tend to talk, probably because I think that way, in a very disjointed fashion. [laughter]

BOHNING: I always edit it before you see it.

SIMMONS: Thank goodness! I hate to put you to that work, but otherwise I'd be trying to figure what the hell I was saying. [laughter]

[END OF TAPE, SIDE 8]

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