THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

WILLIAM JOHN BAILEY

Transcript of an Interview
Conducted by

James J. Bohning

at

The University of Maryland

on

3 June 1986

William J. Bailey JH 3/15/96

E BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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(Date) July 26, 1990

(Revised 24 February 1988)

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

WILLIAM J. BAILEY

1921 Born in East Grand Forks, Minnesota on 11 August
1989 Died in Honolulu, Hawaii on 17 December

Education

1943 B.Chem., University of Minnesota
1946 Ph.D., University of Illinois

Professional Employment

1946-1947 Arthur D. Little Postdoctoral Fellow, Massachusetts Institute of Technology

Wayne State University
1947-1949 Assistant Professor of Chemistry
1949-1951 Associate Professor of Chemistry

1951-1989 Research Professor, University of Maryland, College Park

Honors

1955 Fatty Acid Producers Research Award
1968 Service Award, Washington Section, American Chemical Society
1970 Welch Foundation Lecturer
1971 Research Award, Gulf Oil Foundation
1975 Honor Scroll, District of Columbia Chapter, American Institute of Chemists
1975 President, American Chemical Society
1976 Outstanding Achievement Award, University of Minnesota
1976 Rauscher Memorial Lecturer, Rensselaer Polytechnic Institute
1977 Polymer Chemistry Award, American Chemical Society
1979 Scientific Achievement Award, University of Maryland Chapter, Sigma Xi
1983 Gossett Award Lecturer, North Carolina State University
1984 Mobay Award Lecturer, College of Charleston
1984 Hillebrand Prize, Chemical Society of Washington
1986 Applied Polymer Science Award, American Chemical Society
1988 Henry Hill Award, American Chemical Society

ABSTRACT

The late Bill Bailey starts the interview by describing his upbringing in rural Minnesota, where his family operated a small lumber business. An outstanding high school teacher sparked Bailey's interest in science, which became focused on chemistry during his undergraduate studies at the University of Minnesota, where Lee Irving Smith was a major influence. Indeed, Smith was largely responsible for Bailey's move to Illinois for graduate work with Speed Marvel and research on polymer synthesis. After a year at MIT as postdoctoral assistant to Cope, William Bailey started his teaching career at Wayne University; here he started his noted combination of organic and polymer chemistry. Five years after going to Detroit, Bailey accepted a research professorship at the University of Maryland where he spent the rest of his career. The interview concludes with an account of Bailey's long involvement with the American Chemical Society, including his presidency in 1975, and his thoughts on the current image of chemistry.

INTERVIEWER

James J. Bohning, Assistant Director for Oral History at the Beckman Center, holds the B.S., M.S., and Ph.D. degrees in chemistry. He was a member of the chemistry faculty at Wilkes University from 1959 until 1990, where he served as chair of the Chemistry Department for sixteen years, and chair of the Earth and Environmental Sciences Department for three years. He was Chair of the Division of the History of Chemistry of the American Chemical Society in 1987, and has been associated with the development and management of the Center's oral history program since 1985.

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INTERVIEWER: James J. Bohning

LOCATION: University of Maryland

DATE: 3 June 1986

BOHNING: Professor Bailey, you were born on 11 August 1921 in East Grand Forks, Minnesota. Can you tell me something about your parents?

BAILEY: My father was born in Inkster, North Dakota. My grandfather was in the retail lumber business and my father continued in the retail lumber business all of his life. So I grew up in the lumber business and my brother continued managing the family business in Walker, Minnesota. My father was managing a lumber yard in East Grand Forks when I was born. My mother was also from North Dakota, born in Park River. They were married quite young. My mother finished only two years of high school and didn't graduate from high school. My father started at the University of North Dakota and then was drafted in World War I. When he came back he didn't go back to college, so he had about one year of college.

BOHNING: Did he have any particular interests at that time when he was at college?

BAILEY: No. He was going to take general courses, probably with an emphasis on business. Both my parents were born in 1898. My father was named Admiral Ross Bailey. He was born on the fourth of July and that was not long after Dewey had captured Manila. His parents decided they would have to name him something patriotic. They didn't like George and they didn't like Dewey so they named him Admiral. All of his life he had a lot of fun with the name Admiral. [laughter]

BOHNING: I can imagine. And your mother's name?

BAILEY: Erva Violet Stewart.

BOHNING: You moved to Walker, Minnesota when you were quite young. What was the occasion of the move?

BAILEY: We moved from East Grand Forks to a small town called Fisher, Minnesota, where my father managed another lumber company. My uncle Norman, my dad's brother, was a salesman for the Weyerhaeuser Lumber Company. They were always talking about buying a lumber yard of their own, and when they had a chance to buy the one in Walker, the two brothers entered into partnership as the Bailey Lumber Company.

BOHNING: You started your schooling in Walker?

BAILEY: I actually started in Fisher but we moved to Walker midway through my first year. Walker was a very small town. At the time we moved there were six hundred people. Now it's up to about one thousand.

BOHNING: That's still quite small.

BAILEY: Well, it was the county seat and was the biggest town for about forty miles.

BOHNING: Is that in the lake country?

BAILEY: That's right. Right in the heart of the lake country; practically the only industry there is the tourist business.

BOHNING: How was it as a kid growing up in that environment?

BAILEY: It was delightful. Lots of outdoor activities. Camping, fishing, hunting, and all kinds of sports activities.

BOHNING: Do you have any siblings?

BAILEY: One brother. Robert is about a year and a half older than I am.

BOHNING: Were there any teachers there you remember having any particular influence on you?

BAILEY: I remember a few of them that were really good but it wasn't until high school that I really could identify someone who had a profound influence on me. There was one high school teacher that spent a lot of time with me and other students that

showed some interest. This was Fred Olness. He was really a college teacher. He had taught at the college level but during the Depression a lot of people lost their jobs and he came to Walker working on a WPA project. Then he got the teaching job at the school. He had a tremendous interest in science of all types: chemistry, physics, math, and also languages. He spent hours after school teaching advanced courses to students. He really encouraged the kids. As a small high school we didn't have many additional facilities. John Elliott and I took solid geometry after school with him and passed the state examination to get credit. John is a metallurgy professor at MIT now. A lot of people came out doing well because of Fred. I took additional math courses and we had a lot of discussions about science. He even taught German to us after school one year, things like that.

So I kept up my contact with him. After he left Walker he was teaching at the community college in Alpena, Michigan, until he retired. We kept in good contact and when my kids were younger they were really surprised on his visits to see the relationship I still had with one of my high school teachers. I think that made a tremendous impression on my children.

BOHNING: How long would these sessions be after school?

BAILEY: They would be several hours.

BOHNING: How many of you were there at these sessions?

BAILEY: From two to maybe half a dozen at some of these sessions.

BOHNING: What about chemistry?

BAILEY: He didn't teach chemistry. The coach taught chemistry and did a very good job. He was the basketball coach and also the football and track coach. We didn't have very many teachers. I think he was an excellent chemistry teacher.

BOHNING: What was his name?

BAILEY: Milton Gulsvig.

BOHNING: What kind of laboratory facilities did you have?

BAILEY: It was very crude. Only a few old things to work with; a few beakers and test tubes and some chemicals, so we did very crude experiments. During the Depression the school didn't have enough money to do anything of real consequence.

BOHNING: Was that your first exposure to chemistry?

BAILEY: Probably my first experience was when I got a Chemcraft chemistry set.

BOHNING: When was that?

BAILEY: That must have been when I was ten or twelve years old.

BOHNING: Did you work with the chemistry set?

BAILEY: I went through all of the experiments that were in the set. It wasn't the largest but it was a pretty good size.

BOHNING: Did you ever strike out on your own from the prescribed experiments?

BAILEY: A few things; obviously I was interested in explosives and things like that. We made gunpowder and so on.

BOHNING: You also participated in athletics when you were in high school. Could you say something about your experiences with that?

BAILEY: This was a small school. You had to get most of the boys out in order to form a team. My brother was a very good athlete. I was not quite as big as he was, nor as fast. But I certainly got in good physical shape and I had good endurance. I played all of the sports. We played six-man football which was popular in those days but you hardly hear anything about it now; it was quite common then. It's much faster than the eleven-man game as every block is an open field block and every tackle is an open field tackle. I played end on six-man football and made all-conference. I also played guard on the basketball team. We had a pretty good team. Then I went out for track. I ran the 440, threw the shot and the discus. I was good enough in the discus to get to the state tournament, as I won our region. But I didn't do too well in the state competition.

BOHNING: Did you have to do much traveling in terms of competing with other schools?

BAILEY: Yes. In the conference, to schools maybe fifty or sixty miles away. That was the most we ever traveled, usually by school bus. Then in the summer, I played American Legion baseball. I also played golf.

BOHNING: You said the chemistry teacher was also coach in a number of these sports. Did you develop any relationship with him?

BAILEY: Yes. It was a nice personal relationship with the coach.

BOHNING: Had you discussed any career options by this time?

BAILEY: Probably not that early. I wasn't really sure that I knew what field to go into. I knew I wanted to be a scientist or mathematician but I hadn't really excluded other fields. Although I had an interest in chemistry, it wasn't until I went to the University that I quickly narrowed my field. When I first went down there I could not decide whether I wanted to be a chemist or a physicist. We had a battery of stiff entrance exams. The test results showed that I had a better aptitude for chemistry than for some of the other sciences. I was surprised that it came out that way. I took a general course starting out in the liberal arts college. Then I took beginning physics, beginning chemistry and a beginning math course. I did well, liked chemistry so I picked that for my major. Later on, I actually shifted from liberal arts to the Institute of Technology. Finally I ended up with their Bachelor of Chemistry, which is a much more intense degree than a Bachelor of Arts.

BOHNING: Let's go back to high school for a moment. This was the Depression. Did that have any effect on your father's business?

BAILEY: It certainly didn't help. The community there was very depressed even in normal times. As soon as they started getting federal aid, they were probably as well off there as normally. Because of the tourist business, for instance, a lot of the out-of-work farmers decided that it wasn't worthwhile planting crops. They would rather go fishing. The tourist business was only moderate in those days. People didn't have much money. My dad did a lot of bartering. People would cut a load of wood and trade it for some tar paper or cement. My father sold to the school system; they were burning wood during that time. There was a lot of bartering at that time. People worked long hours;

during the Depression the average work week was sixty hours. Everybody in the lumber business worked ten hours a day, six days a week.

BOHNING: Did you work in the business as well?

BAILEY: Sure. I worked in the afternoons, on Saturdays and all summer. My first job was building fish boxes in the lumber yard. I remember my first job was for ten cents an hour, but ten cents was a lot of money in those days.

BOHNING: At what point did you make the decision to go to college? Were your parents involved in that decision?

BAILEY: There was really never any question about us going to college. We grew up with the idea that we were going to college. It was just a question of where and not if. It was just taken for granted.

BOHNING: Did your father discuss that with you as you were growing up?

BAILEY: Yes. He certainly wanted us to have an education. He was a good businessman. He really could figure things well. As a lumberman he could calculate how to build a house and how strong it had to be. He had picked that up along the way. He was very good with numbers. All the way through, he encouraged us to pick up whatever we needed. I was manager of the school newspaper. I went out for the declamation. I was an orator and I actually won a few of the oratorial contests in the area. I played clarinet in the band.

BOHNING: You must have had an incredibly busy day.

BAILEY: That's right. Well, we just did everything. There wasn't a lot of homework in those days, you tried to do all of your work in school. I didn't take my books home very often. There was something to do all of the time.

BOHNING: Was any one of those activities a favorite over the others?

BAILEY: Probably not. I enjoyed all of the activities. I certainly got a lot out of the oratorial contests. I enjoyed my

music; the social side of the band was always good. I learned a little business on the side. All of those activities were pretty good.

BOHNING: How did you decide on the University of Minnesota?

BAILEY: I think that came about because of our financial state. Obviously the state school was within our grasp, although I got some scholarships to private schools since I was in the top of my class. The tuition at the University of Minnesota was low.

BOHNING: Do you remember what it was?

BAILEY: Twenty dollars a quarter or sixty dollars a year. Room and board was the biggest expense, one hundred and thirty dollars a quarter or three hundred and ninety dollars a year. For that two of us shared a three-room suite. I roomed with my brother in Pioneer Hall at the University of Minnesota. The total cost was not much in today's dollars but it was quite a bit of money when I went to the University in the late 1930s. I think there was no question that I was going to stay within the state and the University had so much prestige. There was a school close to Walker, Bemidji State Teacher's College. Now it's a full-fledged university, but at that time it was designated as a teacher's college, and didn't have the opportunities that the University had.

BOHNING: You mentioned some private schools for which you had scholarships. Did you apply to other institutions or was that by virtue of being high up in the class?

BAILEY: Because I was second in the class. I was valedictorian.

BOHNING: Do you remember which schools offered you scholarships?

BAILEY: More private schools, but I forget just which ones they were; mostly tuition grants rather than full scholarships.

BOHNING: You mentioned that you roomed with your brother. Had he gone on ahead of you to the University?

BAILEY: Yes. He was a year ahead of me. He wanted to be an engineer at that time. He was also an athlete and went out for football and track. He was good enough to be the third-string quarterback on the national championship team. However, that

took too much time away from his studies and he didn't do so well. My dad made him quit football, so he concentrated on track and field. He was very good in the weights and won the Big Ten Conference in the shot. He didn't finish his degree because he went into the service in his senior year. He never went back to finish up his degree.

[END OF TAPE, SIDE 1]

BOHNING: Did you pursue athletic interests at the University?

BAILEY: I was an all-conference end so when I went to theUniversity I thought I would try out. I went over and talked to the freshman football coach, Dallas Ward, who went onto great heights as a coach later on, not in Minnesota but in some of the other schools. He took a look at me and asked what I played in high school. I said, "I played end." He said, "You're too small, kid. Come back when you grow up." I said, "You don't even know if I can play football or not." He said, "Well, you can try out if you want to but it won't do you any good."

At the first practice of the freshman team two hundred and ten guys turned out. I got shuffled down to the eighteenth team. It wasn't the bottom but all we saw was dummy scrimmage. I never got into a full scrimmage. For six weeks all we did was go through the motions and exercises. I decided I had more important things to do so I dropped out. National championship teams were being introduced so that they didn't need so many players. That was when everybody played both ways. They could have used me a few years later on but I wasn't there. [laughter] So I never went out for any more of the varsity sports and I only played intramural sports.

BOHNING: Not even track?

BAILEY: I just decided that I was out of my league. So I studied.

BOHNING: Professional salaries weren't what they are today either.

BAILEY: That's right. [laughter]

BOHNING: You started in the liberal arts college and took general courses in chemistry and physics, as well as mathematics and English. Do you remember who you had as your chemistry instructor?

BAILEY: [M. Cannon] Sneed. He was well known for his book (1). He taught all the freshmen. My physics instructor was [Alfred O. C.] Nier of atomic energy fame. Of course, at the time I didn't know how famous he was.

BOHNING: It was after your first year that you changed over to the Institute?

BAILEY: It was probably not until the end of my second year that I shifted. I could have been a chemistry major in the liberal arts college.

BOHNING: I'm not clear on the difference between the liberal arts college and the Institute of Technology at that time.

BAILEY: The Institute of Technology is the college of engineering. The school of chemistry at Minnesota is in the engineering college. You can get a chemistry degree from either the liberal arts end or the engineering end. One is a B.S. with a major in chemistry and the other is a Bachelor of Chemistry. The professors there told me that it would be more desirable to have the super-degree which was not only ACS accredited but included a lot of additional courses of an engineering background. So with the Bachelor of Chemistry I took engineering drawing, descriptive geometry, statics and dynamics, industrial chemistry, and some mechanical engineering. I took more typically engineering courses, in pattern practice and one on the blast furnace.

BOHNING: Any chemical engineering?

BAILEY: Industrial chemistry was really chemical engineering.

BOHNING: What chemistry did you have your sophomore year?

BAILEY: We were on the quarter system so we took two terms of freshman chemistry and then we took a qualitative analysis course covering the cations. In the first quarter of the sophomore year we took another qual course which emphasized anions. The other two quarters of our sophomore year we took quantitative analysis along with organic chemistry. We took three quarters of organic. Then we took physical chemistry our junior year. In the senior year we had a whole bunch of advanced chemistry courses, including a course in advanced organic chemistry from Lee Irving Smith. Also I took organic qual, advanced organic quant, and a specialty synthesis course from [C. Frederick] Koelsch.

BOHNING: Did you have a choice?

BAILEY: These were all advanced courses. You had a little bit of a selection but you were supposed to take advanced courses in each of the main areas. I picked organic as the one for extra electives.

BOHNING: Who was your first organic instructor?

BAILEY: Walter M. Lauer.

BOHNING: Is that when you developed your organic interests?

BAILEY: I guess so. I think I decided to go into organic because I watched the kind of research that was going on. The physical chemists spent a year building a machine, then a year taking data, and a year calculating results. I wanted to see results in a short time. With organic chemistry you could run a reaction, work it up, and see whether you got the expected product or not quite quickly. That appealed to me even more than some of the other areas.

BOHNING: Who did you have for physical chemistry?

BAILEY: Ralph MacDougall.

BOHNING: You said you were also taking a lot of math at this time. Did that high school math you did after school help?

BAILEY: Well, that certainly helped me. I never got credit for the trigonometry that I took in high school. When I got to the University I was signed up for trigonometry and I came out the highest in the class. So I was well prepared to handle the math, even coming from a small high school. There was no problem, because of this extra help. Certainly other people who had come from a small high school had a hard time adjusting. My brother did not participate in these sections in high school and he had much more difficulty with the University math courses than I had.

BOHNING: Did you do any research as an undergraduate?

BAILEY: Not really. The nearest would be the advanced prep course that Koelsch ran. We had to do some preps that were small research problems but nothing that you could really call research.

BOHNING: Were there any instructors that had any influence on you?

BAILEY: Lee Irving Smith obviously took me under his wing. It was wartime when I went down there and many of us thought we should get into the ROTC. It wasn't required but a lot of my friends decided that, since we didn't know how long we were going to be there, we should get some military background. I took the first two years of basic ROTC and I was about to go into the advanced corps which would have committed me. Lee Irving Smith called me into his office and said, "I understand you're thinking about going into the advanced corps." I told him, "Yes." He said, "I want you to get out of there right away." [laughter] He wouldn't take no for an answer. He was adamant about it. There wasn't anything that I could say or do that would change his mind. He wanted me out of there. That affected my whole career because those who went into the advanced corps were called to active duty at the end of their junior year, whereas I went through and finished my senior year. This was 1943 and we weren't as intensely involved in things at that time. The Draft Boards of a lot of my classmates said, "Well, you can't go to graduate school, but if you get an essential job in industry, we won't draft you." My Draft Board said, "We're going to draft you no matter what you do." [laughter]

I talked to a few people and Lee Irving Smith said, "You're going to Illinois." I said, "I am?" And he said, "Yes. That's where I want you to go." I didn't have to look around at all. He made a call and said, "We're sending Bailey down to Illinois for you." I said, "That's good. I'm going to be drafted so it'll be good to go down and get started so I know what I'm going to come back to." I went down there under the assumption that I was going to get drafted. I was sure I would be drafted. When I got to Illinois, I started to work for [Carl S.] Marvel. We went on the rubber program, so he got me a deferment. My Draft Board would put me in 1A every time and the War Production Board would put me back into the deferment classification. So I was deferred all the way through. The Draft Board said, "Well, we didn't understand what you were doing. If you're doing something essential then the National Deferment Board will take care of you and we'll automatically put you in." And we did that all the way through. I never did get drafted. Most of my friends that went into essential industry were all drafted eventually. I just didn't get caught in the net and was able to work on the rubber program during the war. At the end of the war, I got my degree but was frozen there. For the same reason they couldn't draft me, I couldn't leave. So I stayed there on the rubber program for some time after I got my Ph.D.

BOHNING: Were there any student colleagues at Minnesota that you remember in chemistry?

BAILEY: Well, there were quite a few in my class that I kept in contact with. Bob [Robert B.] Fox, who is on the Board of Directors of ACS right now, was in my class. When he showed up down here at the University of Maryland, he wanted to go on with his studies. I was a professor here at Maryland and he got his Ph.D. here from me. So we were classmates together and then I was his professor here. Aksel Bothner-By, who was the head of the Carnegie-Mellon chemistry department, was in our class. Chris Rondestvedt, who just recently retired from Du Pont, was in our class. Those were the top students in our class. One of our lab instructors for organic became the professor at Northwestern who won the prize in physical organic.

BOHNING: Do you know his name? Was it just this year?

BAILEY: Yes. It was this year. [Frederick G.] Bordwell was his name.

BOHNING: When you got to Illinois, what was your connection with Marvel? Did you talk to other people about research?

BAILEY: Actually the contact at Illinois was [Reynold C.] Fuson. He and Lee Irving were very good friends. I almost worked for Fuson but at the time he really wasn't doing anything at all connected with the war effort. His research was more academic. I decided that I should at least be doing something connected with the war effort. Marvel's involvement was very high and the two of them decided on Marvel.

BOHNING: What about Roger Adams?

BAILEY: Roger was not spending much time there at all and was in Washington most of the time. When the time came to pick a research director, he wasn't around to even talk to.

BOHNING: How was your Ph.D. problem determined? Did you select that or did Marvel assign you to something?

BAILEY: Well, it was a little of both. He gave me a couple of small problems and said, "Let's look around and see what you want to concentrate on." I did a couple of things. One of the problems they were worrying about was sodium-initiated polymerization. I became interested in that and then everything that I touched there worked. So I just fell into that.

BOHNING: Had you had any exposure to polymers prior to working with Marvel?

BAILEY: Not really. A little in some of the advanced courses, but very little. It was something that just came along. Probably Marvel's personality attracted me more than working on polymers per se.

BOHNING: What was there about him that you found attractive?

BAILEY: He was a very easy guy to talk to; very down-to-earth, very matter-of-fact. More like the people I was used to in a small Minnesota town. Some of the other professors were a little less approachable than Marvel was. He was very easy to talk to and very enthusiastic about everything one did. That impressed me.

BOHNING: How many people were in his group when you were there?

BAILEY: It was quite a good-sized group. There were maybe twenty-five working on the rubber program. He had some 'straw bosses' that helped him run the program. One was Norman Rabjohn, who went to Missouri and another was Bob [Robert L.] Frank, who then left after a few years and went up to Morton Thiokol. Another one was George Inskeep who went down to the tobacco company [Philip Morris]. They helped to run the thing. Some of the other people who were in the group were Charlie Overberger, and Paul Smith, who's the chairman of the [ACS] board now.

BOHNING: What was your reaction to [Charles C.] Price? Did you know him?

BAILEY: I did. I took advanced organic chemistry from him. Half of one semester was devoted to polymer chemistry. Between Marvel and Price, we got a lot of polymer chemistry during the first year. So I got to know Charlie in that respect. We had a lot of intramural sports and Charlie was quite an athlete at that time. I played softball with him. You probably heard about the famous softball game.

BOHNING: Tell us about that.

BAILEY: There were a whole bunch of groups there. We had a chemistry softball league. We had a rubber team, an NDRC team, and an anti-malarial team. The big rivalry was between the anti-malarials and the rubber team. One summer, for the championship, Charlie was playing in the outfield. He was pretty good at that.

Catching, throwing the ball up, taking off his glove, catching the ball and throwing it in. He could do that just about as fast as any two-handed guy. We were playing and the score was such that we were about three runs behind. In the last of the ninth, we rallied and scored two runs and there were two outs and the bases were loaded. We were obviously starting to hit the pitcher, so they called in a substitute pitcher. Charlie was called in from the outfield to do the relief pitching. Wally [Wallace] Runge was up to bat. Charlie threw one ball and Wally hit the ball hard. Bill Parham was playing shortstop. He jumped up as high as he could and just tipped the ball. When it ricocheted off his glove everybody was running. Charlie Overberger was on second. He went all the way round, scored and was sitting down having a beer while the ball was up in the sky. However, the shortfielder caught it and threw it in. Of course, it was a triple play. So the anti-malarial team won it. 8-9 was the score. We were sure we had the game there. The last of the ninth, bases loaded, and he threw just that one pitch. [laughter]

BOHNING: Let me come back to your first problem. You mentioned George Inskeep and his name appears on the first publication that you had with Marvel (2). Were you looking for materials with specific properties or were you doing a systematic exploration of the polymerization reaction?

BAILEY: At first we just wanted to make some sodium-polymerized materials to see what they would look like. There had been descriptions in the literature around the time of World War I. Karl Ziegler had studied it in the 1920s and had determined the mechanism.

[END OF TAPE, SIDE 2]

BAILEY: A number of people were interested in the structure of sodium rubber and how it differed from the ordinary polymer. We then figured out how to make it in a four-ounce bottle and subsequently evaluate it. I was able to show that we could make block polymers and copolymers; we could make styrene butadiene copolymers. All of those were new. Michael Szwarc came along some time after and claimed that they were living polymers, but we were actually doing all of that much earlier.

BOHNING: I may be talking to him in September.

BAILEY: He's certainly famous for coining the name. [laughter] My work was really the first to copolymerize butadiene with styrene and the first to make block polymers out of butadiene and styrene. Things went well on that project. In 1944 Phillips

Petroleum wanted to scale up the process and actually make quite a bit of polymer. They were having trouble getting started so Marvel sent me down to Bartlesville [Oklahoma] to help set up their first pilot plant. I spent several weeks down there. Actually, I have just come back from a trip to Bartlesville; Phillips sponsors the ACS award in Applied Polymer Chemistry and they invited me down there to give a talk. I was able to say that I had been there before anyone else in the room. [laughter] I could go back to 1944.

BOHNING: Did they go into production then?

BAILEY: Yes. They don't make any now but they were selling it for some time.

BOHNING: What was it like to do research at Illinois then? Marvel must have been busy at the time. He was probably not as busy as Adams but certainly, at that point, he was very busy off campus.

BAILEY: He went to meetings all the time but he came back. For instance, when I was there he was President of ACS but then they didn't hold any national meetings. [laughter] So he never got to participate in that. He was actually President a little longer because he was President-Elect when [Thomas] Midgley was President but died in the middle of his term. So Marvel was President for a year and a half, but during that time there were no national meetings. He still had a few board meetings to go to as President but not all the things that a President has to do these days. It was a smaller society then. He certainly got involved with a large number of these things. Of course he was quite involved with the rubber program of the War Production Board. Then he got involved with the NDRC and some of the anti-malarials, managing some of the national research organizations of these things. He wasn't involved quite as much as Adams, who was away almost full-time during the war. It was a rare occasion when Roger came back.

BOHNING: How was Marvel as a research director?

BAILEY: He was certainly very relaxed with almost everything that he did. He would come in, find out what you were doing, and talk to you about it, but he really didn't try and push anything. He was very excited when something developed. He was enthusiastic but he let you go through at your own pace. We had to write reports on what we were doing because once a month there had to be a report coming out of the rubber program, a short progress report that would be incorporated into the overall report. Every month a pretty thick booklet left Illinois about the rubber program.

BOHNING: What were the facilities like that you worked in?

BAILEY: Primitive. We were in an old building, the Noyes lab. Essentially we had no hoods. There was a little box with windows that went up and down and had no fan on it. So we put a Bunsen burner in this chimney and the heat from the burner would produce the draft. We cleaned everything by cooking it in sulfuric acid with a little added nitric acid. That was the way to remove polymers from your dishes. We were living on borrowed time, so safety features were not quite what they are today.

I remember one set of polymerizations. I was going to do some polymerizations and we didn't have any thermostat baths. There was an old aquarium with two glass sides. Marvel said, "Why don't you use that?", so I set it up as a constant temperature bath. I had some bottles in there rotating end-over-end. One night, one of the bottles blew. It was a sodium polymerization of butadiene and the reaction was fairly far along. It blew with such force that it took the sides out of the aquarium. Water covered the floor and the rest of the bottles got hot. Every once in a while one would go and would shoot this napalm-like sodium around. Wherever it hit water, there would be a flame with the butadiene burning and the rubber floating around. The night watchman heard the explosion and came up. He would stick his head in there and there would be a big blast and this stuff would splatter all over with a fire here and there. He called Marvel at about three o'clock in the morning. We all slept up on the fourth floor of the dormitories and nobody could hear the telephones. He tried to wake me but he couldn't do it so he spent the rest of the night there. The night watchman quit after that. [laughter] Marvel finally got ahold of George Inskeep. George came over at about 6:00 in the morning to see to it. They couldn't wake anybody at our place until later on in the morning. By that time it was all calmed down. I took quite a bit of kidding. Again, it was only because everything was makeshift. We had no fancy equipment or expensive facilities. All of that came after the war.

BOHNING: Did you have any kind of instrumentation?

BAILEY: Very little. Most of the instruments that you think of as instruments weren't even invented yet. We had some pH meters around but you really didn't need them for organic work.

BOHNING: Any Beckman DU's?

BAILEY: We had a Beckman around. But that was about all. No infrared and NMR and such things that we are used to today. Even to determine the microstructure of the rubber, they were running ozonolysis and analyzing the products. [Izaac M.] Kolthoff was also in the rubber program. He invented this peracid titration. You get a break in the curve and then you extrapolate back. Practically everything was determined by wet chemistry and very little by instrumentation.

BOHNING: Did you do any testing or were you primarily involved with the synthesis?

BAILEY: Mostly just synthesis and polymerization.

BOHNING: Who would do the testing? Would that be done at Illinois?

BAILEY: We sent samples to a rubber company. They would determine the compression set, hysteresis, the heat rise, the Mooney viscosity, and all of that. We wanted to make enough to build a tire because no matter how much data you had, nobody would believe it unless you had a tire. So I made fifty pounds of rubber, all in four-ounce bottles. [laughter] They made a tire out of it but the treads separated from the carcass in the middle of the test, so we never really found out how good it was.

BOHNING: How long did it take you to make that quantity?

BAILEY: It took a couple of months.

BOHNING: I'm not clear what year you got your degree.

BAILEY: February of 1946.

BOHNING: You were allowed to use the work you were doing for the War Production Board?

BAILEY: That's right.

BOHNING: Had you thought about what was going to happen beyond Illinois?

BAILEY: When it looked like the war was going to finish and we were going to get out, I started looking around for some fellowships. I did interview for a couple of teaching jobs but it was quite obvious that without a postdoc you really couldn't get a reasonable job. There were quite a few people at Illinois who could all leave at the same time and who were looking for teaching jobs. There were forty-five people in organic chemistry who overlapped me while I was at Illinois, either ahead of me or behind me, all of whom went into teaching. Just a tremendous number. Not all of them stayed in teaching but as you go around the country, almost everywhere you turn there is somebody from Illinois. Guys like Bill Parham, Gardner Stacey, Le Roy Klemm, Virgil Boekelheide, Minor Coon, Lester Reed, Pete Kovacic; not all at big universities but they went into teaching of some sort.

BOHNING: What about going into industry? Was that an alternative?

BAILEY: Not really at that time. I think the Illinois atmosphere has always been to train people that would go into industry, but I thought I wanted to go into teaching. Certainly, there were an awful lot of other people there that were thinking along the same lines. I applied for a number of postdoctorate fellowships. I got an offer from [Louis] Fieser at Harvard. Marvel said to me, "You don't want to go to Harvard. You're a Midwestern boy; they'll just chew you up and spit you out. I think you should work for Cope. You're more of an easygoing guy and don't know how to do everything. I think with your personality you would fit more with Cope than some of the other people." So he arranged for me to go to MIT and Cope got me the Arthur D. Little postdoctoral fellowship there.

BOHNING: Didn't Overberger go to Cope? Was that at the same time?

BAILEY: That's right. And Pete Kovacic also. The three of us went there. Charlie and I worked in the same lab at Illinois and then in the same lab at MIT. Cope was just a prince of a guy to work for.

BOHNING: Did you have supervision of other graduate students?

BAILEY: No. I was on my own. Cope was also pretty busy but when he was in town he made sure that he talked to everyone that was in his group. He would come around and sit down and chat with you for quite an extended time. That was a good time to be at MIT because he had just moved up there from Columbia and had brought in Jack Roberts, John Sheehan, and Gardner Swain. They all had small groups, just getting started. We had joint research seminars together. It was terrific interacting with

those four high-powered research guys just to see how they thought. When Cope was there, we all had lunch together as a group before we had the research seminar. We got a lot closer supervision with Cope than I did with Marvel but that was just a happenstance of his moving to MIT. As he developed a bigger group and the department got larger, a lot of the students complained that they never saw him. But it wasn't like that when I had my contact with him. It was a very productive year in the sense that it was the first time that I didn't have any courses.I didn't have any exams to take or any thesis to write so I could concentrate on doing research and sitting in on the advanced courses that Sheehan and Roberts were offering. The things that I would have worked on when I left Illinois were not the things that I was going to work on when I finished my postdoc. It was a real productive year.

BOHNING: You worked on cycloöctatetraene. In fact, during that particular period, Cope has a number of papers on that particular molecule. What was so important about this chemistry at that time?

BAILEY: Historically that had been a controversy for years. Willstätter had synthesized cycloöctatetraene way back by classical methods. During the 1930s people became interested in the Hückel rule and whether cycloöctatetraene would be aromatic or not. All those types of compounds became of interest. People tried to synthesize it and apparently had failed. There were a bunch of papers pointing out the similarity in physical properties of cycloöctatetraene and styrene. Quite a few papers implied that Willstätter didn't have cycloöctatetraene but had made styrene. Cope decided that he would like to investigate that. When [Walter] Reppe synthesized cycloöctatetraene from acetylene, Cope thought that was something that he really wanted to investigate. So Charlie Overberger duplicated Willstätter's synthesis. I synthesized it from chloroprene (3). That was a new synthesis, which was then adaptable to making derivatives of cyclo-octatetraene. Cope synthesized a number of other derivatives and cited some of the chemistry based on some of the work that we did and the modification of some of the Reppe chemistry.

BOHNING: You were still single at this point. Was this your first time in Boston?

BAILEY: Yes.

BOHNING: What was that like compared to the Midwestern life of Illinois and Minnesota?

BAILEY: In one sense I could never quite get used to the Bostonian atmosphere. It was the only place that I've ever stopped someone in the street and asked them how to get to somewhere, and they would turn around and walk away from you. They didn't have much trust with strangers and things of that ilk. Although the culture was fabulous around there. Tickets for plays would be sent to MIT where we could buy them at half-price. There were concerts for the Boston Symphony that we got for a fraction of their face value. I participated in a lot of things that I never could back in Minneapolis or Urbana. That was quite interesting.

BOHNING: Was the situation in academe changing during that year? Were you looking beyond MIT now?

BAILEY: It was probably a good time when I finished my postdoc. Many schools had lots of veterans entering and most schools were looking for additional faculty. Some of the more senior people who had been around Illinois for longer than I after their degrees got jobs right out of Illinois and didn't take a postdoc. For instance, somebody like Ben [Benjamin F.] Aycock went to Wisconsin directly out of Illinois without a postdoc. When I finished my postdoc I had a choice of a few places. One of the jobs that I considered was an offer of an instructorship at Columbia. That would have been a tough road.

However, there was an attractive offer from Wayne University. Neil Gordon was the chairman of the department and Herb [Herbert C.] Brown was there. I thought that if Herb Brown is there and is doing well, I should be able to do well also. The job at Columbia only paid $3600 for an academic year and the job at Wayne paid $4700. Detroit was obviously a cheaper place to live than New York, so that was also attractive. I reported to Wayne and it wasn't long after I got there that Brown left for Purdue and left me as the only person in organic chemistry interested in research. That was really a baptism of fire since I was teaching all of the advanced courses. I gave eighteen lectures a week and picked up ten graduate students that first year. So I got a flying start.

BOHNING: Did Cope help you at all in looking for an academic position?

BAILEY: He would pass on leads to me and would talk to people. He didn't quite do what Lee Irving [Smith] did: "You're going to Illinois." Or what Marvel did: "You're going to MIT."

[END OF TAPE, SIDE 3]

BOHNING: What was the situation like when you first arrived at Wayne State; did they have much of a research program going at that time or was it just building up?

BAILEY: They certainly had an organic group. It was essentially Herb Brown's group, so it really wasn't very much. Very primitive facilities and not many of them. At MIT, of course, we had excellent facilities, quite an improvement over Illinois in those days. Wayne was probably worse than Illinois, but I soon inherited Herb Brown's labs. He had gone in there and really installed those labs himself. He essentially did all the plumbing and things like that himself. He really worked at it. We had very little space for anything so life was difficult until we got the new building. We got that just about the time I left, so for years we had labs right in the rest of the building with all these non-chemists all around us. Obviously they didn't appreciate all the odors and things. It was an old high school building. Wayne took over an old abandoned central high school down in a slum area of Detroit and had built it up after the war. They were just coming into their own but it was a municipal university, so their facilities were about as primitive as you could get. But enthusiasm was very high and everybody had very high hopes for the university, had big plans for it, and certainly chemistry was moving ahead quite fast.

BOHNING: What kind of person was Neil Gordon?

BAILEY: He was just very unusual. It's a coincidence that he was also department chairman here at Maryland.

BOHNING: Really?

BAILEY: Yes, for a number of years. He actually started the Journal of Chemical Education here before he moved on to Johns Hopkins. While he was at Johns Hopkins he started the Gibson Island Conference, which is in Chesapeake Bay, and that was forerunner of the Gordon Research Conferences. While he was there [Johns Hopkins] he instituted the Johns Hopkins Fellowship plan, which was very unusual in that, in practically every state in the Union, he organized a committee to raise money to send a graduate student to Hopkins with all fees paid. That was so successful that the President of the University wanted him to do that in other fields. He wouldn't do it, the president got mad at him, so he left Johns Hopkins and went out to Central College in Missouri.

While he was at Maryland he became intrigued with Hooker and had the chance to pick up Hooker's library. Sam [Samuel C.] Hooker was one of these industrial chemists that did extensive research on quinones and amassed his own private library (4). He did all this work on his own on the side. After he retired he

continued and never published anything until [Louis F.] Fieser ran into him. Fieser was working on quinones and found out that Hooker had done much of the same stuff, only much more. Fieser wrote up part of Hooker's work (5). When Gordon found out about this library where there were these old rare editions (Proceedings of the Royal Society back to volume one, things like that) he was always trying to raise money for a building. He took the library out with him to Central College in Missouri but they didn't have any facilities to keep it up so when he had the opportunity to go to Wayne, he said, "I'll come if you promise to take care of my library." A lot of the industry around there pledged the money for the department to maintain the library. One of the guys who had promised him money at that time to entice him to come was [Sebastian S.] Kresge. So he came and moved his library and had the Hooker Library there and I think every day he would call up Kresge and say, "Where's that money you promised me?" [laughter] Finally, Kresge gave him $100,000. They changed the name to the Kresge-Hooker Library and it's a marvelous library. They were able to raise enough extra money to build a separate building. He [Gordon] was a delightful old man at the time when I knew him.

BOHNING: Was he chair the four years you were there?

BAILEY: He committed suicide the third year I was there. Then a guy by the name of Russell Bright took over as chairman. We were recruiting quite a few people. We recruited George [H.] Coleman, who had been at Iowa and then ended up as editor of the Journal of Organic Chemistry, to be director of the Hooker Library. He ended up, right after I left, as department chairman because he was a very sound guy. I recruited Cal [Calvin L.] Stevens to come in the year after Herb Brown left and Cal is still there. There are quite a few people that came in. [Carl] Djerassi took my place after I left. He didn't stay so long. Wayne has been famous for the guys who have been there and left. You can name quite a few people: [Michael] Cava was there for a while, [Norman L.] Allinger was there, quite an influx of people. It was a good stepping stone for a lot of people.

BOHNING: That's quite a list. There were two things I wanted to look at when you got to Wayne. First, the courses you were teaching. You said you gave eighteen lectures.

BAILEY: I taught all the graduate courses in organic and some of the undergraduate courses.

BOHNING: How long did that situation last?

BAILEY: Only about one year, but it was kind of funny. They had a scheme that Herb Brown had put in. He said, "Let's assume the normal teaching load is eighteen hours. Then for every paper you publish, you can subtract two hours from your teaching volume. So if you turn out nine papers a year, you don't have to teach." That sounded good to everybody. Of course, Herb worked it out so he didn't have to teach at all. So some of the other people had to teach quite a bit. I was teaching eighteen hours and everybody thought that was fine. Well, I hadn't published any papers yet.

BOHNING: Did you have any students that first year? Brown left the year after that?

BAILEY: He left the first year I was there.

BOHNING: So you had some made-to-order students.

BAILEY: That's right. I got all the students that would have come to him. I had ten students working for me by the end of my first year.

BOHNING: In that first year, by 1948, you had made the first all-cis diene polymer with [Harold R.] Golden (6). How did you select those target molecules? How did you determine what you were going to do in your research?

BAILEY: I knew that I wanted to pick out some problems that were significant, yet fairly easy to solve. I just tried to find some molecules that were of extreme interest that I thought I could succeed with. The dimethylene cyclohexane was one that I thought was a very interesting diene that was going to give us an all-cis diene polymer. It wasn't too complicated so we certainly went after that right away. We were able to make it and polymerize it and get some very interesting results.

BOHNING: What kind of a student was Harold Golden?

BAILEY: He was an older student. In fact, he was older than I was. He had worked at U.S. Rubber in Detroit for quite a few years during the war and so he had maybe ten years of industrial experience before he decided to return to school. He was more mature. Of course, a lot of the students that I had then had been in the war and had come back. Quite a few of them were older than I was. They were ready to go to work with not much

fooling around. They were married, had kids and responsibilities. They were very serious, hard-working students and were easy to motivate.

BOHNING: After that first paper on polymer synthesis, much of your subsequent work was in developing the cyclic dienes specifically as monomers, but there didn't seem to be much in terms of actual polymerization. Were you doing both?

BAILEY: Well, of course, we did polymerize a lot of the dienes. It is kind of funny, you'll see some compound that you worked on, for instance the thiophene 1,1-dioxide that Earl Cummins and I worked with back then (7). That was one of these compounds again that people had wondered about, whether it was going to be aromatic or not. Nobody had made it. So we made it and showed it was highly reactive. Now a whole bunch of papers are coming out on it. We just had a seminar where one of the guys has a new synthesis where you use the thiophene dioxide as an intermediate, so it's kind of strange that you'll see these things recycling. [laughter]

BOHNING: Again, what I wanted to ask is how you selected the nature of these target molecules?

BAILEY: Thiophene dioxide was a diene reported in the literature with both a theoretical and practical interest. I thought from the chemistry we had done for cycloöctatetraene we could do the same kind of chemistry and come out with the thing. It turned out that it was a more sensitive molecule than cycloöctatetraene. We used the same kind of chemistry and it was an important intermediate, both in the theoretical and synthetic aspect in the way it went. We developed a bunch of synthetic techniques. For some of our monomers we had to study some ester pyrolysis. You'll notice an overlap there, synthesizing unusual dienes, studying ester pyrolysis. We continued that for quite some time.

BOHNING: Did you develop any consulting relationships when you were at Wayne?

BAILEY: Just at the tail end of my time at Wayne. I had a consultantship with Union Carbide. They were looking for young guys who knew something about polymers and there weren't a large number around that could go around to all the companies. Of course, Carbide thought I was a good bet so they offered me this consultancy. I stayed with them maybe six or seven years.

BOHNING: What kind of support was there for your students? Did you have to start getting your own support for your students?

BAILEY: Some of my first support was from the Research Corporation. They have this starter grant when you're just starting out and I got one of those. The Office of Naval Research had a program that was pre-National Science Foundation, there wasn't any NSF at that time. ONR was interested in some polymer research and some basic research. I got a grant of $20,000 and that was enough to support ten students. Wayne had practically no money for salaries, but did have a pretty good system where you could check out things from the stockroom and as long as you weren't outrageous, the department paid for it. We had to buy the big pieces of equipment, but things like glassware and chemicals didn't have to be charged to outside funds because the department would pay. That's not the way it is any more anywhere! [laughter] This $20,000 paid for the overhead, my salary, the equipment, and everything else on this basis for ten graduate students. That was the bulk of my money at that time. Of course when I moved to Maryland, I transferred that contract with me. It was a real lifesaver in that respect. Now I can support one graduate student on $20,000. [laughter]

BOHNING: Did the university have teaching positions for graduate students?

BAILEY: Oh yes, just like any other teaching assistants. They would teach for the remission of fees and so forth.

BOHNING: What was your teaching like the second year? Were the ground rules still in existence?

BAILEY: They were modified but my load had dropped quite a bit. I think the next year it dropped down to twelve hours or something like that. The teaching loads were high at Wayne.

BOHNING: Even twelve is high.

BAILEY: By today's standards, that would be excessive, but on their standards then, it wasn't so bad.

BOHNING: Was anyone else there doing any polymer work?

BAILEY: Wilfried Heller, who was a physical chemist, was interested in both colloids and polymers.

BOHNING: What about testing some of the things that you made.Did you send them out for testing?

BAILEY: We got somebody else to do testing. It is something that takes specialized equipment and expertise. Even today I don't do much testing; occasionally I get a student to go over and use an Instron or something like that. But much more sophisticated than that, you have to get somebody who knows what the results mean.

BOHNING: Why did you decide to leave Wayne?

BAILEY: Well, I was offered a full professorship here, a research professorship. There were quite a few things. As a research professor, I wasn't going to teach very much, so that was attractive. Of course, the area was much more attractive than Wayne. Wayne was in a slum area and there wasn't really a decent place to live within an hour's drive. The idea of being able to live within walking distance of the campus appealed to me as a small town boy. This is, of course, a very active research area. Wayne made a counter-offer to keep me. They offered me a full professorship if I'd stay and offered me quite an increase in salary. Actually, I took a cut in salary over what I could have gotten if I had stayed at Wayne, but the attractiveness of quite a few things out here made me jump at the chance. Maryland was not in the top echelon of universities, but at least it was an improvement over Wayne at that time.

BOHNING: Were there other possibilities? Had you thought about going into industry at any time?

BAILEY: Not really. I never really considered that. Most academic people don't. My only industrial experience was when I spent a summer at Mallinckrodt in St. Louis in the summer of 1942 between my junior and senior years. That was a delightful experience. I worked for August Homeyer for quite a bit of the summer and he gave me some advanced syntheses to run through. He was just a delightful guy to work for and I still correspond with him and see him once in a while. And then my consulting experiences kept me in contact with industry. Because of that contact some of my work is more practical than that of some of my colleagues, because I do keep up with industrial problems and outlooks. When I teach my courses I include a lot of current industrial practice and things of that type.

BOHNING: Do you think that in today's chemistry curricula the students get insufficient exposure to industrial chemistry?

BAILEY: You are right. They don't. Most of them are going to end up in industry and most of the professors don't know any industrial chemistry, so they really can't teach it. The rest of them are inclined to produce graduate students as clones of themselves and so they train them to be college professors rather

than to be industrial chemists. It's hard for the graduates to look for a job because they are looking for the same kind of chemistry that they did in graduate work, because that is what they feel comfortable with and know they could do a good job, but they don't know if they can do a good job in some other field. So they have a hard time even talking to industrial people about what they would have to do on the job.

BOHNING: What about your own students? How have they been distributed?

BAILEY: Well, most of them go into industry. I have a number of students that are in academic work, but the bulk of them are certainly in industry, because that's where the bulk of the chemists are.

[END OF TAPE, SIDE 4]

BOHNING: When you left Wayne to come here did you bring your graduate students with you?

BAILEY: I left a few back there, the ones who were pretty far along, and those with family ties who felt that they couldn't move. I brought ten graduate students with me to Maryland and left four or five back there and I commuted back and forth till they finished up. The few that would have worked for me or were just starting then shifted to work for somebody else.

BOHNING: Did you give any polymer courses there?

BAILEY: I taught an advanced course in the organic chemistry of polymers.

BOHNING: That was probably one of the first ones. I don't imagine there were too many courses like that in existence.

BAILEY: Well, not so many, although I mentioned one. When I was at Illinois half of one semester of advanced organic was on polymers. Not so long as the one I taught at Wayne, but it was a nice short course in polymers. That was our first introduction, so I took that material and used it as the nucleus of my course, and there were some books that were coming out at that time that could serve as a basis.

BOHNING: There are some other questions that I will come back to later in terms of polymer education. For the time being, let's go back to your early years here at Maryland. In 1953 you synthesized the first all-trans diene polymer (8). Now had that been of interest since you made the all-cis version in 1948?

BAILEY: Obviously we wanted to complete matters; there certainly was an interest at that time. We got so we could either make all-cis or all-trans in various ways but when the Ziegler catalyst came out we were put out of business. It was easier to make all-cis or all-trans materials with the Ziegler catalyst rather than by our method.

BOHNING: In 1955 you were appointed to the ACS Committee Advisory to the Chemical Corps? I know very little about that committee.

BAILEY: Well, I guess it is no longer in existence. The ACS had a long standing arrangement with the Chemical Corps. There were quite a few people who felt that the ACS should be getting more involved in advising the government in various ways, so this was not just an ordinary advisory committee set up by the Chemical Corps but through ACS. There were a lot of people involved in chemical warfare, certainly through World War II, that were on that committee and were quite familiar with its work.

BOHNING: I think Price was chairman of that committee, wasn't he?

BAILEY: Yes, I think so. [H. F.] Johnstone at Illinois was an influential member and Morris Kharasch was on it and so there were quite a few, both academic and others.

BOHNING: How long did you serve on that committee?

BAILEY: Must have been four or five years. It was interesting. Morris Kharasch started that little program when he talked General Stubbs into funding some young chemists around the country to just do fundamental work on anything they wanted. He recruited guys like Overberger, Per K. Frolich and myself. There were a number of people like that where he would just give us some money and say, "Work on anything that you think is related to chemical warfare." So, a lot of the stuff that I did on phosphorus came out of that (9). It was very fundamental work.

BOHNING: How much money was involved with this program?

BAILEY: Well, it wasn't a large amount of money, but what they did do was to assign some army personnel that were drafted during the Korean War and instead of doing their fighting in Korea or being on active duty at Edgewood, they were assigned to these universities. One of the guys I had was Shelly [Sheldon A.] Buckler who is now director of research at Polaroid. He had got his Ph.D. with Cheves Walling at Columbia and then was drafted and assigned here. It was ideal duty for him. He did his postdoc here.

BOHNING: I never heard of that program. In terms of your service on the committee, what were your experiences? Anything specific that you remember about them?

BAILEY: Well, essentially we were supposed to review their research effort, so we'd meet several times a year up at Edgewood and they would present their overall view of what they were trying to do and we'd give them some advice. And it was on wide-ranging topics and some of them overlapped what I knew, but then there was always somebody there who knew something about the science involved so it was always interesting to see the interplay of a bunch of people from outside, essentially unpaid consultants. It is a little different from consulting for a company where you are paid to do something. Many of these people had a lot of experience with chemical warfare problems, so it was interesting to make constructive suggestions on approaches to their problems.

BOHNING: Did you get to see any demonstrations of things the Chemical Corps was doing?

BAILEY: Well, just on a rare occasion we would get something demonstrated. But obviously you really can't see real demonstrations. We could see how fast you could kill a rabbit with a nerve gas. That was always amazing. You just take one drop and put it on the rabbit's skin and he was gone--almost immediately. And we did see a few munition-type things. But not anything that you would call large scale demonstrations.

BOHNING: So the chemical warfare materials were still a pretty active project at that time?

BAILEY: That's right.

BOHNING: In the early 1950s you synthesized the first ladder polymers here. How did that develop?

BAILEY: Well, of course, this was an offshoot of our ester pyrolysis. One of the first things we were always trying to do was to make Diels-Alder polymers. We wanted to make a difunctional diene and then react it with a difunctional dieneophile. It was Jim [James] Economy, who is now director of research at IBM and I guess the immediate Past-President or Chairman of the ACS Polymer Division. He is also the previous winner of the ACS award in Applied Polymer Chemistry, just before I was. He made vinyl butadiene (10). Again, that was a small molecule that no one had been able to make before. We were trying to synthesize unusual but important compounds. Vinyl butadiene was one of those that fitted that category and Jim was able to make it and react it with some cyclic dieneophiles. If you do that with quinone you get a nice ladder polymer (11). Since then we've gone on to make a whole bunch of other bifunctional dienes and other bifunctional dieneophiles and we have made a whole series of perfect ladders. For very few other ladder polymers does anybody really know anything about the structure. Ours are some of the few that are made by unequivocal methods and are perfect ladders.

BOHNING: When did you get into protein analogs?

BAILEY: I guess we again were thinking about some important small molecules and we wanted to make some very active amino acid derivatives. We started with lysine and tried to see if we could make some very active little monomers from that, and one that we aimed for was the glycine azide hydrobromide (12). That was a very highly reactive small molecule that no one had ever been able to make before. When we were successful with that then we just sort of branched out. That's led us into many different directions making polypeptides (13) and ordered copolyamides (14) and then the biodegradable polyamides (15). All those follow from that one initial discovery.

BOHNING: I guess in most recent years most of your work has been in the area of either so-called expanding or non-contracting polymers (16).

BAILEY: That's been one that's got the most publicity.Certainly our work on free radical ring opening and our biodegradable things are also of current interest and have attracted a lot of attention. But our work on monomers that expand on polymerization is one that has a lot of potential use and applications. From that point it has attracted a lot of attention both from academe and industry.

BOHNING: I guess the idea of whether expansion or contraction occurred in polymerization was known, but were you the first to achieve the expansion?

BAILEY: Yes, we were the first to achieve expansion. The closest anyone had really come was [Arthur V.] Tobolsky. He pointed out that ring opening polymerizations had lower contraction than ordinary polymerizations (17), but he never really carried it all the way through. He pointed out this correlation but all his systems still contracted. We took his observation and decided that if we could find monomers where we open two rings for every new bond that is formed then we should have a chance for expansion. When we looked for examples in the literature we found that there weren't any, so essentially what we had to do was to invent a new type of polymerization and that's our multiple ring-opening polymerization. That was the key to the concept.

BOHNING: How did you select your target molecules?

BAILEY: We just hunted for highly reactive intermediates that had a chance of undergoing double ring opening. The first we tried were the spiro ortho esters (18) and that was successful. Then we went on to the ketal-lactones (19) and then the trioxabicyclo octanes and the spiro ortho carbonates (20).

BOHNING: There was something in C & E News a few weeks ago about the Epolin Corporation.

BAILEY: That story was not quite accurate. I'm a minority stockholder in that company, and have nothing to do with the day-to-day operation of it. I do have a patent, just an American patent, on monomers that expand (21). The university has a patent policy that anything discovered at the university belongs to the university unless they release the rights back to the inventor. The university has this arrangement with the Research Corporation. They looked at it after we had done research for a year and decided that we hadn't carried it far enough for them to be interested in it. They released the rights back to me and I filed for a patent. I made very broad claims and it took ten years to get the patent. The claims aren't quite as broad as when I started out, but they are still pretty broad. The patent was issued in 1983 and it still has fourteen years to run. We had a contract with the Army Ballistic Missile Command to do work on some spiro ortho carbonates for use as rocket mortar casings. We developed some pretty attractive materials. They wanted to do some testing for which they needed 50 pounds. I wasn't about to synthesize 50 pounds, so they put an RFP out and Murray Cohen, who has a consulting lab at Newark, bid on it and got the contract. He made 50 pounds of the material and then became intrigued with the compounds and decided he would like to form a little company. So, essentially what I did was to give him an option on the patent. Then he formed this little Epolin company. As part of the arrangement I own 2% of the company but they pay me a royalty on everything they produce. He's floating a $2

million dollar stock issue as soon as the SEC approves the sale. That's what they are waiting on now. Anyway, I haven't spent very much of the money yet.

BOHNING: In connection with this, you said that the ring opening work has perhaps more fundamental possibilities. You do have papers on some dental fillings.

BAILEY: That's right. That's an offshoot of this expansion polymerization. We did some joint research with the Dental School up here in Maryland. Van Thompson and I did some joint research (22) and he evaluated some of these materials which look interesting. Then he stopped his work and went to dental school. He figures that if he is going to be a professor in the Dental School he should have a dental degree. So he has his Ph.D. in material science and now a dental degree and now we are talking about going back and doing some more joint research. He is spending a year at the National Bureau of Standards. I have a student that's working for me out there and so we'll get together this summer and for the next year and do some additional research in that field. The only trouble is it takes an awful long time testing before they will let you put something like that in somebody's mouth. But it does look attractive for that. The material commonly used in fillings is bisGMA which is a big bis glycidyl methacrylate. It shrinks about 6 1/2 % by itself and if you put it in with some other monomers you may get 9% shrinkage. Well, the filling pulls away from the tooth, bacteria can get between the filling and the tooth and decay then enlarges the cavity and your filling falls out after two or three years. We think we can solve that problem.

BOHNING: I think I remember reading that the adhesion characteristics are good, particularly on the enamel surface.

BAILEY: Yes, all these things that expand turn out to be super adhesives. We had predicted that in the property of ice. You know when water freezes it expands by 4% and as a consequence ice will adhere to anything. An icicle will form on your eaves but if you strike it you will break the icicle not part it from the eaves. It is a cohesive failure, not an adhesive failure. Early in the airplane industry, it was thought that the way to prevent ice from forming on wings of airplanes was to put a Teflon coating on the wings. Since water doesn't wet Teflon any ice that formed would just fall off, but that doesn't happen. It sticks better to Teflon than it does to aluminum. We had predicted that these monomers would be very good adhesives and we can increase the adhesion of polymethylmethacrylate to aluminum by a factor of 40. So one of the areas that Epolin wants to exploit is coatings and adhesives which depend on this strong adhesive property of the expanding monomers.

BOHNING: Are there many other materials that have this type of property?

BAILEY: None other than some inorganic materials. Water has that property and some of the amalgams used in fillings will expand a little bit and a precision typeface is made with an alloy that will expand a bit. But there are no other organic monomers, except these.

BOHNING: Is there any competition in this area? Are other people looking?

BAILEY: There's a whole bunch of Japanese companies that are looking at things of this kind. A lot of the American companies are evaluating it--Westinghouse, for instance. I have a little joint research with them. They are interested in the electrical properties and it's very good for that because we're producing void-free materials and most electrical breakdowns are across cracks and voids. We're producing void-free materials which have potentially good electrical properties.

BOHNING: I had a statement that you may or may not want to comment on, but it summarizes a number of things that we have been talking about here. This is a quote, I think from one of your awards nominations: "Bailey's work starts with fundamental concepts, continues with experimental artistry and winds up with results of great practical value."

BAILEY: Well, I hope that's true.

BOHNING: Let's turn to some of your other activities. You were President of the ACS in 1975. Did you see any important issues ten years ago? Why did you pursue that particular position?

BAILEY: Of course, I have been active in the ACS for a long time. That wasn't in a vacuum. Within ACS, I had been involved in changing a lot of things within the structure of the Society. One of the things that I didn't like was how difficult it was for somebody to work their way up in the superstructure. For instance, I was on the Council for years and years and never got appointed to a committee. So I ran for the Council Nominations and Elections committee and got elected.

[END OF TAPE, SIDE 5]

BAILEY: But I was never appointed. I thought that was a peculiar thing. One of the things I wanted to do was to have some of the decisions made outside of a closed group. For instance, for years and years no one could go to a Board of Directors meeting. They were not open; they were always closed. As chairman of the Nominations and Elections Committee I couldn't even find out who was an effective director, and to a large extent even chairmen of committees weren't allowed to give their own reports to the Board. There would be a staff liaison who would present the material. I thought that should be changed and that there should be an open forum. So one of the things I did was to start the Middle Atlantic Regional Councilors Group. They were called MARC at that time. They would meet a couple times a year for a whole day and talk about things that could be changed in the Society. As a result I'd write letters to people on the Board and to the President to tell them what the Middle Atlantic Regional Councilors thought about certain things. It was kind of surprising how much effect we had because they weren't really used to hearing from their constituents quite like that.

As a part of this, a committee was appointed to study restructuring the Society. As a consequence we got [Robert W.] Cairns to open up the Board of Directors meeting for the first time, and then made the powers-to-be so nervous that they started the Arthur D. Little reports. I don't know if you are familiar with that or have heard about it. They brought in the Arthur D. Little organization to analyze the Society and recommend some changes. A whole bunch of changes came about. One is the demand for professionalism in the Society as exemplified by Alan C. Nixon's election. I had been pushing for more democratic decisions within the Society, and then I thought that the ACS should be doing a lot more in public affairs and public relations than they were doing. I thought I could have an impact on quite a few of those things if I was elected. So I ran against Bryce Crawford and Emerson Venable. I had one of the old guards, who was very conservative, and I had Emerson Venable who was one of the radicals. I had the whole middle to myself. I was elected by a pretty good plurality at that time. Then I started to implement things. There was an arrangement by which associates could be appointed to the committees, but it had never been implemented. I expanded, for instance, the number of associates appointed to committees and I was able to find some sort of appointment for every councilor who said he wanted one. I either wrote or called every councilor that was not on a committee and asked them what they would like. I made a lot of friends this way. Things have regressed somewhat but they have never gotten back to the way they were. We have a lot more people on committees and associates and things like that. Now we have a Committee on Committees that worries about appointments. If the President doesn't want to implement it then they don't appoint the full complement of associates, but at least now its a lot easier to work your way up. You can get to be an associate and if you are doing good work then you'll get to be a full member of the committee and so on up. Then I also implemented a deal where a person couldn't be on the Board of Directors more than three terms. So that kicked some old-timers off the Board. We put a

limit of three years on the chairmen of committees as well. Even the chairman of the Board of Directors can't be on for more than three years.

BOHNING: How do you see the Society now, ten years later?

BAILEY: It still needs some more progress, but it's doing better. We had quite a conservative group in charge. For instance, we had no new journals for fourteen years until I got in there. Then we had Organometallics and Langmuir and we are going to have quite a few more now. I think one of the big decisions that I was partly responsible for is with Chemical Abstracts. That was the decision to go from being a wholesaler to being a retailer of data. For years we only marketed Chemical Abstracts through other supply houses such as SDS and Dialog. Now we have CAS Online. There's no limit to what they are going to do. Every issue of their little blurb tells how they have added a new service. They are now in full control of their own destiny and before they weren't. I think that was a big decision. Another one that I pushed is for this new building that is going up. It came a few years too late, I think, but it's going now. And as you know, I pushed for the Center for History of Chemistry.

BOHNING: Yes, yes. Or I wouldn't be here.

BAILEY: Right. You wouldn't be here. Most projects need to have a champion. We don't always have enough champions in ACS for new projects. It's so easy to say we're going to keep on doing just what we have done in the past. It takes a little bit more to say let's expand, let's do something else. I think that members will support us if they understand what their money is being used for. They don't like to support things where they don't see where their money is going.

BOHNING: Along this line, do you think that chemists are mostly conservative by nature?

BAILEY: They are, in general, politically, financially and everything else on the conservative side. I think that is because they are so intent on what they are doing as scientists that they don't always branch out and see what effect it will have on things outside their group. There are many exceptions, of course, but on the average it takes a sort of a revolution to get them started. Professionalism--it took the mass layoffs of the early 1970s to galvanize the chemists into making a stand and incorporating the employment guidelines and things like that

needed in the profession. It takes something very dramatic before they will enter into public affairs or public relations.

BOHNING: Chemistry tends to have a poor public image right now for a number of reasons, but I don't know if chemists have always been as active as they should have.

BAILEY: I don't think they are active enough. The ordinary guy in the street is quite ill-informed. I don't know whether you saw this little blurb from the AAAS meeting. It was a survey for the National Science Foundation. How many of the people in the street believe in evolution? There's almost as many who believe in astrology as in evolution. They don't know what the word molecule means. If we can't even tell them what a molecule is, how can they.....? This guy just took a telephone survey of 2000 scientifically selected people but couldn't find very many who knew what a molecule was. So we just aren't telling a very good story. I think we need to spend more time on this when half the people don't know what a molecule is, presumably having gone through a whole school system and when they get out they still don't believe in evolution. I don't know what's going on but I think that we need to do more in public relations. We have got to find some way of raising money for TV shows and other programs that will educate people. We have got to work on Congress a little bit more than we do. I was on the Committee on Chemistry and Public Affairs for a long time. It always is amazing what could be done but what we fail to get done.

BOHNING: About the same time, I think we are looking at 1975 or thereabouts, you talked about the caliber of students attracted to the profession compared to the numbers. You said that statistics can be misleading and that we could attract higher quality students.

BAILEY: Right. It's always bothered me that we don't attract the top students into chemistry. Even now if you take, say, a beginning organic course and go down the list a disproportionate number of the top students will go on to medical school. To a large extent it is that the image of the M.D. is greater, the income potential is greater, but we are just not competing with those people. If you look around on campus here you'll notice that the fight to get into law school is tremendous even though they turn out ten times as many lawyers as we do scientists and engineers. I just saw starting salaries for lawyers in New York. After a starting salary of $70,000 they were guaranteed, by a New York firm, $130,000 within three years. When you see figures like that you realize that it's just disproportionate. The business school here can't expand fast enough to take care of the business students. You have to be almost an A student to get into the business school. You don't see them fighting to get into chemistry or engineering or things like that.

Again, it's partly money but it's also partly the image.These kids are turned off by the publicity they read. I've always thought that industry should give ACS some money to really publicize. But a lot of people in the ACS disagreed, because it would be tainted if we took industrial money. We would be just the same as they are. But I don't believe that. It's true that when industry says something it is automatically suspect. I am sure that if we accepted funds from industry and put on good public information without bias it would clean the air quite a bit and do a lot of good. Without a source of money we will never be able to do anything. But it's kind of funny that some people will not accept any money from industry because they feel it is tainted.

BOHNING: Well, children don't get chemistry sets anymore either.

BAILEY: That's right.

BOHNING: Someone told me recently that even if they did, because of current regulations you couldn't put anything worthwhile in a chemistry set. You'd have to reduce drastically the kinds of things that could be done with it.

BAILEY: Yes. You'd have to put in baking soda and that's about all. Just things around the kitchen. There's a lot of chemistry you can do with things around the kitchen. I'm not sure that's valid. You might not make any money putting together a kit like that.

BOHNING: We just interviewed Hubert Alyea (23) and he gave us one of his armchair chemistry kits. It's quite intriguing. While we are talking about it, let's look at polymer education. We already mentioned the number of early courses. How do you see the place of polymers in both the undergraduate and graduate curricula?

BAILEY: What I see and what I would like to see are two different things. Polymer chemistry is systematically being eliminated from chemistry departments. In a little while it will all disappear. Most big departments don't have a polymer chemist on their staff. The trend is getting worse. When I was at Illinois there were four people there who were somewhat interested in polymers and now there are none. When I was at MIT there were four people there who were interested in polymers and now there are none. When Tobolsky died he was not replaced at Princeton. When [Raymond M.] Fuoss retired at Yale he was not replaced. Flory retired from Stanford and has since died and he was not replaced. [John K.] Stille left Iowa and he has not been replaced. You can go right on through the list. Harvard has no

polymer chemist on the staff. The University of California at Berkeley has none.

What has happened is that polymer science is being moved into chemical engineering departments, into material science departments or separate institutes--places like Case Western Reserve where they have an Institute of Macromolecular Science which is in the engineering school. Eric Baer was the Dean of Engineering for a long time. People in chemistry who were interested in polymers retire or those with joint appointments disappear. Now very few people take courses across departments.If they have a seminar hardly anyone from chemistry shows up. It is almost that they don't talk to one another. People who write chemistry textbooks really don't put any polymer chemistry before chapter 43, and the instructor never gets to chapter 43. You'll find that most chemists now can get all the way through to the Ph.D. and really never hear of a polymer. It's going to get worse rather than better. It'll probably be that way until industry steps in and tries to tell the academic people what kind of training they would like their new recruits to have. But at the present time you don't see that happening. Companies like Du Pont would as soon train their own polymer chemists. They would get organic chemists from Illinois who never heard of a polymer and they'll train them themselves. So that as long as that's the case there won't be any change in that trend. Probably when I retire from here I won't be replaced with another polymer chemist, although on campus we have other departments with polymer chemistry going on, particularly Chemical Engineering and Textile Chemistry.

BOHNING: What's responsible for that movement out of chemistry?

BAILEY: Polymer chemistry is looked on as a sort of applied practical chemistry, which doesn't have the prestige of more theoretical kinds of chemistry. For a while the fashion was physical organic and now it is stereo-synthesis and natural products, but most of that is not practical and is regarded as being much more academic than something that might have some utility. The reward system means that people just aren't going into areas that their colleagues think are not suitable for research. And then it gets worse because your colleagues don't know any polymer chemistry. Here they don't understand what I am doing. It feeds on itself, because they don't know how to evaluate my work. They are surprised when I win a prize.

BOHNING: And yet much of your work is good solid synthesis. There are practical applications involved but synthesis is good solid basic research.

BAILEY: It has a beauty if you know what the final objective is.You might consider that it looks interesting, including some of the intermediates in their own right, but most people

appreciate what you can do with the products, not just synthesizing material. 2-vinyl butadiene is all right just as an interesting synthesis but if that's all you are going to do with it then you might as well have made some natural product that looks like it's more difficult to synthesize. So many of the things we do, things that we are making, do not look that complicated. It's the beauty of what you can do with the product, rather than just being able to synthesize it.

BOHNING: Going back to what we were just saying--is this part of the reason chemistry has the image it does with the public,because they really don't understand?

BAILEY: The public doesn't understand, sure, that's right. That's somewhat true among chemists. They don't understand what some of their other colleagues are doing. And chemistry is a big field. It's hard to keep up in more than one field.

BOHNING: True. In 1974 you wrote about the possibility of tenure in industry. How was that received then, and has anything happened in ten years?

BAILEY: Well, it really hasn't developed very much. The employment guidelines have a statement that's been there tenyears. So there is this element now built into the employment guidelines. But when you see these mass layoffs, that's one of the items that's quite frequently violated.

Of course, we now have, you know, some things on discrimination according to age that makes it a little more difficult. When I see these early retirements I think that that's kind of a mistake too. You can see the push of a few things that are making that more of a reality and I think we will eventually probably get to that point. I think that the ACS has been remiss about pushing those guidelines more than they have. You have to push the guidelines during good times. You can't do it when times are bad. That's how the American Association of University Professors got their guidelines accepted, right after the war when they couldn't find teachers. They started to put them down in 1941 when there weren't very many teachers around. They were able to get all the universities to accept them when they couldn't hire enough college teachers. And when times got tough, they had the guidelines in place so that those AAUP guidelines worked out fine. If we had our employment guidelines accepted in the same way the universities accepted some of the AAUP guidelines, we wouldn't be in some of these difficulties.

[END OF TAPE, SIDE 6]

BOHNING: There's just a few isolated things. You had one or two isolated papers on mycotoxins (24, 25). How did you get into that?

BAILEY: One of my students had worked at the Food and Drug Administration and he wanted to work more along the lines of natural products. I agreed, provided we could find a problem. He picked out one of these rare diseases that killed all the horses in Russia and we were able to get some of the fungus. So I gave the go-ahead to see if we could isolate the active ingredient. He worked along these lines with my help. It was a problem with a change of pace. He did all the backbreaking work.I was a consultant on the problem more than a director.

BOHNING: At one point you were editor of Macromolecular Syntheses. I remember reading an introduction in which you asked for submissions of syntheses (26). Were people reluctant to send in such papers?

BAILEY: We had a little trouble with that series at the beginning and it's not completely out of the woods yet. The theory was to make up something that was like Organic Syntheses. If a polymer chemist wants to run a particular type of polymerization or something like that, he can go to Macromolecular Syntheses and find out exactly how to do it. We would get preparations submitted and it would be sent to someone else to check who might give it to a student to run through. If they got the same results then we would accept it for publication. But we've always had trouble getting enough people to submit papers. The reason, of course, is probably that there aren't so many synthetic polymer chemists in academic work, so you don't draw from as many. If you look through Organic Syntheses, practically all of them come from academic work and you don't get very many from industry. Almost all of the polymer chemists are out in industry and not in academic work. You've got to search out industry to get them to write up one of their syntheses, but using the techniques and equipment common in ordinary laboratories. Often they're on a much bigger scale or something like that, and no thought has been given about how to run it in a 500 milliliter flask. So we've had problems soliciting material. I'm still on the editorial board of Macromolecular Syntheses. The editors act in turn and it was my turn to be editor of that volume. We still have problems getting enough good papers to turn out one volume every year. We haven't really met that goal completely.

BOHNING: I got the impression from reading your introduction, that you probably had personally called most of the people in the volume to contribute.

BAILEY: Well, quite a few of them.

BOHNING: Just a few questions to sum up. What has changed in chemistry since you started--chemistry in general and in polymers?

BAILEY: Polymer chemistry was almost in its infancy when I got to Illinois. When we got on the rubber program it was only just starting out to make synthetic rubber. Carothers had made nylon but nylon stockings were really not widely available even at that time. Most of the synthetic polymers really weren't available at that time. Polyethylene just celebrated a 50th anniversary not long ago and so I was there essentially at the beginning of the polymer era. The growth in plastics, rubbers, fibers, films and things like that is beyond comprehension. It is still growing at a tremendous rate as, one by one, we replace metals and natural materials with more and more synthetics. There seems no end to this growth, so it's really intriguing. The push now is that we have too many non-biodegradable plastics. Recently there was this major wildlife meeting where it was pointed out how many birds and fish are killed from eating things that people throw in the sea. The lakes in northern Michigan and Minnesota and Maine are inundated with abandoned fishing lines, and now a large number of swimmers drown each year because they get tangled up. It's just going to get worse and worse. The problems with these polymers have got to be solved by biodegradable modifications. That is one of the areas that we are working in now. We have a biodegradable polyethylene, for instance, if I can interest people in it.

BOHNING: You somewhat touched on the next part of my question, which was to ask what you see in the future for, let's say, a young chemist just entering the profession. Recently I heard of someone at Du Pont asking the people in the lab, "When are you going to make another nylon?" In other words, how frequently do we get a nylon or a Teflon, a major breakthrough?

BAILEY: I don't think that we'll get very many new bulk polymers, but there will be a lot of breakthroughs in specialty materials. The use of polymers in medicine is just beginning. You don't have to turn out tons of material as long as you can get a good price for it. For instance, the durable suture market is $90 million a year. Tons aren't there, but the money is. You have to decide whether you are going to make a 5% net profit or whether you are going to make a couple thousand percent net profit. You really can't expect that in a reasonable length of time you are going to come up with a number of new polystyrenes. But just think, if you take something like polyethylene, of all the new polyethylenes that are being invented and studied. First we had the high pressure version and then Marlex and then the Ziegler catalyst and now the linear low density. They are all

polyethylenes but nowhere near the same structures and chemistries. The only reason that nylon-6,6 is commercially viable is that the factories are already built to make the intermediates, but it isn't the best material. Qiana has a hard time because in many respects it's much better than nylon-6,6, but it's more expensive. For every bit of Qiana Du Pont sells they lose some sales of nylon-6,6. So they're competing with themselves. And they've got a few other nylons out there that are even better but they aren't going to commercialize them because they aren't going to compete with themselves.

BOHNING: A number of people that I've talked to have recently retired from industry and have almost all said the same thing,that industry is changing. They've said that management decisions are not made by scientists, or that scientists are no longer as involved as they may have been in the past in making management decisions. The MBAs are much more involved in making those decisions.

BAILEY: I think that's certainly true. Du Pont at least has had some scientists at the top of their organization for some time. But it's still hard even for [Richard E.] Heckert now to control many of the decisions. But other companies, you look at the top and there are no scientists there at all. And that scares you. It's one of those real sticky problems. Still, some of the decisions are business decisions and it's a question of what they perceive. But if somebody doesn't have the guts to go into a certain field, then you're in trouble. We have had too much fad stuff in company policies such as early retirement. When one company starts an early retirement program, they all do. The reasons that the first company did it really have no bearing on the second one. But they say, "Well, they must know something we don't know." The result is that all the companies lay off at the same time. Now for all the big companies the magic word is biotechnology. They all jumped into biotechnology, whether they know anything about it or not. They just don't want to miss something, so they all go into the same thing. That's really not the way to make money. You want to do something that's a little different than the rest.

BOHNING: Is there anything else that you might want to add?

BAILEY: We've covered a lot of things.

BOHNING: Yes, we have. If not, I certainly want to thank you for spending time with me to share your experiences and thoughts for the record. Thank you very much.

[END OF TAPE, SIDE 7]

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