CHEMICAL HERITAGE FOUNDATION

J. PAUL HOGAN

Transcript of an Interview
Conducted by

James J. Bohning

at

Bartlesville, Oklahoma

on

10 February 1995

(With Subsequent Additions and Corrections)

ACKNOWLEDGMENT

This oral history is one in a series initiated by the Chemical Heritage Foundation, on behalf of the Society of Chemical Industry (American Section). The series documents the personal perspectives of Perkin and the Chemical Industry Award recipients and records the human dimensions of the growth of the chemical sciences and chemical process industries during the twentieth century.

This project is made possible through the generosity of Society of Chemical Industry member companies.

THE CHEMICAL HERITAGE FOUNDATION
Oral History Program

RELEASE FORM

This document contains my understanding and agreement with the Chemical Heritage Foundation with respect to my participation in a tape-recorded interview conducted by Dr James Bohning on Feb. 1995. I have read the transcript supplied by the Chemical Heritage Foundation and returned it with my corrections and emendations.

1. The tapes and corrected transcript (collectively called the "Work") will be maintained by the Chemical Heritage Foundation and made available in accordance with general policies for research and other scholarly purposes.

2. I hereby grant, assign, and transfer to the Chemical Heritage Foundation all right, title, and interest in the Work, including the literary rights and the copyright, except that I shall retain the right to copy, use and publish the Work in part or in full until my death.

3. The manuscript may be read and the tape(s) heard by scholars approved by the Chemical Heritage Foundation subject to the restrictions listed below. The scholar pledges not to quote from, cite, or reproduce by any means this material except with the written permission of the Chemical Heritage Foundation.

4. I wish to place the following conditions that I have checked below upon the use of this interview. I understand that the Chemical Heritage Foundation will enforce my wishes until the time of my death, when any restrictions will be removed.

a.* ✓ No restrictions for access to corrected transcript only; provided the marked sentences on p. 14 as remained and other changes made as indicated. PH 9/23/96

b. _____ My permission required to quote, cite, or reproduce.

c. _____ My permission required for access to the entire document and all tapes.

This constitutes our entire and complete understanding.

(Signature) Paul Hogan

(Date) 9/23/96

(Revised 17 March 1993)

* Does not include permission to tapes access, which is to be denied. I request destruction of tapes.
Paul Hogan 9/23/96

This interview has been designated as **Free Access**.

One may view, quote from, cite, or reproduce the oral history with the permission of CHF.

***Please note*:** Users citing this interview for purposes of publication are obliged under the terms of the Chemical Heritage Foundation Oral History Program to credit CHF using the format below:

J. Paul Hogan, interview by James J. Bohning at Bartlesville, Oklahoma, 10 February 1995 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0136).

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

The Chemical Heritage Foundation (CHF) serves the community of the chemical and molecular sciences, and the wider public, by treasuring the past, educating the present, and inspiring the future. CHF maintains a world-class collection of materials that document the history and heritage of the chemical and molecular sciences, technologies, and industries; encourages research in CHF collections; and carries out a program of outreach and interpretation in order to advance an understanding of the role of the chemical and molecular sciences, technologies, and industries in shaping society.

J. PAUL HOGAN

1919 Born in Lowes, Kentucky, on 7 August

Education

1942 B.S., chemistry and physics, Murray State University

Professional Experience

1942-1943 Chemistry and Physics Instructor, Mayfield High School
1943-1944 Physics Instructor, Oklahoma State University of Agriculture and Applied Science

Phillips Petroleum Company
1944-1947 Research Chemist
1947-1954 Project Leader
1954-1960 Group Leader
1960-1977 Section Supervisor
1977-1985 Senior Research Associate
1985 Retired
1985-1986 Consultant

1986-1993 Independent Consultant, Neuman, Williams, Anderson, and Olson

Honors

1969 ACS Award for Creative Invention, American Chemical Society
1971 Honarary D.Sc., Murray State University
1972 Pioneer Chemist Award, American Institute of Chemists
1972 Distinguished Alumnus Award, Murray State University
1972 Lifetime Appointment as Kentucky Colonel by Governor of Kentucky
1976 Inventor of the Year Award, Oklahoma Bar Association, Copyright and Patent Section
1981 Man of the Year Award for Outstanding Achievement in Polymeric Materials, Society of Plastics Engineers
1987 Perkin Medal Award, Society of Chemical Industry

ABSTRACT

The interview begins as **J. Paul Hogan** discusses his family background and early education in Lowes, Kentucky. Next follows a description of Hogan's college education at Murray State and teaching experiences at the high school and college levels. The central portion of the interview focuses on Hogan's career with Phillips Petroleum Company, which began after his position teaching physics at Oklahoma A&M was eliminated. Hogan's first work at Phillips was with Grant Bailey and Alfred Clark on double bond shifting. After about five years, he switched to the Fischer-Tropsch project, preparing and commercializing a process for the production of hydrocarbon. Next Hogan worked with Clark, and eventually others, beginning by investigating the nickel oxide catalyst and using it to produce 223-trimethylpentene and 223-trimethylpentane. Ultimately Hogan and Banks discovered polypropylene, and the interview examines some of the many patents and papers stemming from this research. Throughout the interview, Hogan comments on his relationships with Clark, Bailey, Robert L. Banks and Clarence Lanning. He also discusses the reaction of Phillips' management to the production of polymers, the work and decisions leading to the commercialization of polyethylene before polypropylene, and the legal situations surrounding the commercialization of polypropylene. Towards the end of the interview, Hogan examines Phillips' attitude toward publishing, records retention, and R&D; his own work on copolymerization; and his views on the research process and the roles of theory and intuition in it. The interview concludes with a discussion of the meaning of the Perkin Medal and the future of chemical R&D.

INTERVIEWER

James J. Bohning is Professor of Chemistry Emeritus at Wilkes University, where he was a faculty member from 1959 to 1990. He served there as chemistry department chair from 1970 to 1986 and environmental science department chair from 1987 to 1990. He was chair of the American Chemical Society's Division of the History of Chemistry in 1986, received the Division's outstanding paper award in 1989, and presented more than twenty-five papers before the Division at national meetings of the Society. He has been on the advisory committee of the Society's National Historic Chemical Landmarks committee since its inception in 1992. He developed the oral history program of the Chemical Heritage Foundation beginning in 1985, and was the Foundation's Director of Oral History from 1990 to 1995. He currently writes for the American Chemical Society News Service.

TABLE OF CONTENTS

INTERVIEWEE: J. Paul Hogan

INTERVIEWER: James J. Bohning

LOCATION: Bartlesville, Oklahoma

DATE: 10 February 1995

BOHNING: Mr. Hogan, I know you were born in Lowes, Kentucky, on August 7, 1919. Could you tell me something about your father and mother and your family background?

HOGAN: My father was a merchant in the little town of Lowes until about two years before my birth, when he got fed up with merchandising and bought a farm about a mile from Lowes. I was born on this farm. The only thing remarkable about the house that I was born in was that it was a former residence of a VIP when he was a boy.

BOHNING: Oh, was this Alben [W.] Barkley?

HOGAN: Yes, he and my Dad went to school together there at Lowes. They were roughly the same age.

BOHNING: I see.

HOGAN: My mother was the daughter of a part-time carpenter, part-time Baptist preacher, and my father was the son of a full-time Baptist preacher. My mother had gone to what was called a normal school in those days, which is now Western Kentucky University at Bowling Green. She got a teacher's certificate and taught in some little towns around, before she married. After my father bought the farm, he never did quite give up one thing that he had done even before he was in the merchandising business, and that was, he had a repair shop. He repaired bicycles and farm machinery. He fell back into that as he farmed, and by the time I was grown, he had most of his income from repair work on other people's farm machinery and lawn mowers and sharpening saws. He was a very good mechanic. He retired and moved off of the farm into Lowes when I was about ready to graduate from college, and he continued his repair work there until he was ninety-five, when he decided that he'd back off a little bit. [laughter]

BOHNING: That's marvelous. You were the third of four children, is that correct?

HOGAN: Right.

BOHNING: I think one of your brothers and your sister also had college degrees.

HOGAN: Yes. I had two older brothers. My oldest brother took a radio and television correspondence course after he graduated from high school, and he became a radio and television repairman and also sold TVs and radios. He had that business all of his life until he passed away. People came from as much as thirty miles away, because he was pretty good at fixing radios and televisions. My sister has been a professor of chemistry at Glendale Community College in Glendale, California up until the last two or three years, and now she's teaching part-time at a smaller college. She's semi-retired. My other brother got a degree at Murray State, the same place that I did, and he sold insurance and stuff like that.

BOHNING: I'm not familiar with where Lowes is. Where in Kentucky is it?

HOGAN: It's in the western end of Kentucky, in what's called the Jackson Purchase, and it's about eighteen miles from Paducah, which is on the Ohio River across the river from Illinois.

BOHNING: You lived on the farm then until you went to college. Is that correct?

HOGAN: Right.

BOHNING: Where did you receive your early schooling?

HOGAN: Lowes High School. At the time that I graduated there, Lowes had fifty to seventy-five high school students. It became consolidated later and got quite a bit bigger than that after I left, but I graduated in a class of eighteen.

BOHNING: As you were going through high school, what had you thought about doing?

HOGAN: I don't think I gave it a lot of thought. My aunt, my mother's sister, was a professor of elementary education at Murray State University, Murray State College then. She kind of took my brother into tow—not my oldest brother but the brother who was a couple of years older—and she got him into college. I graduated from high school at the tender age of sixteen. I had skipped a grade back in the seventh or eighth grade, and I laid out a year, and then she insisted that I should get to Murray. Knowing that I liked to tinker like my father did, she said, "You ought to major in physics." I said, "Well, that sounds interesting," and it was, but I switched from the physics major to the chemistry major during my junior year. I had been taking chemistry and just simply switched over and got a major in chemistry and a minor in physics.

BOHNING: Had you had any chemistry or physics at Lowes High School?

HOGAN: No, just general science in the eighth grade, that kind of stuff. I went through what's called Smith-Hughes Agriculture all the way through school, and that had a little science in it, but not much.

BOHNING: No real laboratory experience.

HOGAN: No, none at all.

BOHNING: What was it about chemistry that made you decide to switch when you were at Murray State?

HOGAN: Well, it was really a little more of a hands-on science than physics. I enjoyed physics and was good at it, but one reason I switched was that I discovered that to get a major in physics, I had to have a course that was being offered only every other year. That was one influence that changed me to majoring in chemistry, because it was not going to be offered in my senior year. I was really glad I did, because a B.S. in physics doesn't really get you much of a job.

I taught high school for a year in Mayfield, Kentucky, a town of about eight thousand then, and I taught physics and chemistry. In fact, I was the chemistry and physics department. [laughter]

BOHNING: Let me go back to Murray State for a moment. This was sort of the Depression. You started there in 1938, if I'm correct.

HOGAN: In 1937.

BOHNING: Okay. I was going to ask how the Depression affected your family and how you financed going to Murray State?

HOGAN: My aunt was the main financier. Both my brother and I lived at her house. She was not married, and she had a house with three bedrooms, so we just stayed with her and helped out around the house and that sort of thing. That was mainly the thing that got both of us through college. We weren't as desperate during the Depression as some farmers because my father had some war bonds. He had sold them for the government during the First World War. When things got tough he just cashed one of those which he had bought during the war.

BOHNING: You spent a year teaching high school. Was your degree an education degree?

HOGAN: I had a teacher's certificate, yes.

BOHNING: You've also listed Dr. [R. A.] Johnston, one of your professors at Murray State, as having influenced you, and I was wondering in what matter?

HOGAN: Well, he was an excellent chemistry teacher, number one. Number two, in my junior year he made a trip to Oklahoma, to Oklahoma A&M College [Oklahoma Agricultural and Mechanical College] and some other universities, and he came back with some interesting things to say about Oklahoma A&M. He got me kind of interested in it.

To back up a little bit, I had gone to California in 1940. I dropped out of Murray after my junior year because I had a stomach ulcer and had lost a lot of weight. My aunt and uncle, who lived in Redlands, California, about sixty miles east of L.A., said, "Come on out to California and live with us a while. We've got a good doctor and we'll get you straightened out." I went out there, and sure enough, by the time the spring semester at the University of Redlands rolled around, they said, "Hey, you ought to get back to school," so I took a semester there. Then I went back the next fall and finished up at Murray. In the spring semester I was feeling so much better that I thought maybe I should be in the service of some kind, so I put in my bid for a commission but was turned down because of my 4-F status. They didn't want to take a chance on me. In the course of deciding what to do next, I decided to send my transcript to Oklahoma A&M College, Oklahoma State now, because of Professor Johnston's influence in that direction.

That turned out to be a real good thing to do, because in about the middle of the second semester as I was teaching high school—I guess in February—I got a call from Oklahoma A&M College. They'd gotten desperate for physics instructors because they were conducting a number of military programs. They had just about all kinds of officers training schools there, for women and men both. They wanted somebody to teach physics in the Army pre-flight school. They called me, wanting to know if I could come out and work. I quickly got loose from the high school and headed out to Stillwater. That was all because of Professor Johnston. They got me out there, and I worked there a year, and then moved into research at Phillips [Phillips Petroleum Company].

BOHNING: Before we get to that point, let me back up one more time to Murray State and ask what the chemistry department was like when you were there. How many majors were there?

HOGAN: I'd have trouble coming up with an accurate number on that. There was a good number. We had fairly full classes, and as far as I was concerned, a good choice of subjects. I kind of took all I could get in chemistry. At that particular time an outstanding professor of chemistry was on leave to the government, so I missed him, but the people who were there were good people, I felt, like Johnston and a younger fellow. I don't know whether they had two or three chemistry professors at that time—three, probably.

BOHNING: What was that year at Oklahoma teaching physics like? You took some chemistry courses, too, didn't you, while you were there?

HOGAN: Yes. I started out both lecturing and doing lab demonstrations. They decided that I was better at the laboratory demonstrations than some of their other people, so they just put me full-time in the laboratory demonstrations. However, I didn't do badly as a lecturer. They did an unusual thing. They gave the students—who were, most of them, about as old as me—a questionnaire on all of the professors who were teaching and asked them to rate them.

BOHNING: Way ahead of its time.

HOGAN: They gave the results to me after I left, and I was amazed at how high they had rated me. I guess, being about their age, I could communicate with them maybe better than some of the older professors.

BOHNING: Why did it last only a year?

HOGAN: I started teaching March 1, 1943, and the Army Pre-Flight school closed at the end of May, 1944. The powers that be said, "We've got enough pilots and navigators in the pipe to win the war, so let's close the school." So it was closed, but they gave me plenty of warning and I got my applications out to several companies around. I chose Phillips. I had offers from several, but decided I liked Bartlesville and Phillips.

BOHNING: Had you thought about going back and doing any more graduate work at that point, or was it just getting a job?

HOGAN: No, I didn't seriously consider going full-time to get an advanced degree, because I had been serving the military and Phillips was desperate for people. They had many contracts with the government, doing research and coming up with different things like synthetic rubber instead of natural rubber. That looked good to me, and I took one or two courses on extension from Oklahoma State during the first few years at Phillips. As far as formal schooling, that was it.

BOHNING: Do you remember the interview you had at Phillips before you came?

HOGAN: Yes, I do. There were two people who interviewed me. They were set up in fuels and lubricants, and in chemicals. Those were the two departments that I interviewed in. The division manager of chemicals interviewed me, and he seemed to be less interested in what I knew than in the fact that I had gotten a not-so-great recommendation from the high school back in Mayfield because I'd let a class of eighth graders get out of hand. [laughter] I had no trouble with my high school students and had some good students, but I had these eighth graders the first semester. Then I got another class of eighth graders the next semester, and I landed in the middle of them immediately and got total control over them, but they nevertheless rated me down on discipline and that's all Walter Schultz could talk about. [laughter] "Are you sure you can handle these people? You might be supervising somebody."

When I got to the fuels and lubricants, I had an entirely different interview. Dr. [Grant] Bailey asked me a question that was kind of prophetic. He said, "What is the difference between heterogeneous catalysis and homogeneous catalysis? What are the differences in the problems?" Well, I hadn't had any catalysis, but I had done some reading in the library over there at Stillwater and I knew the answer, and I answered it exactly. Then he asked me another question, "Draw xylene or something on this paper," and that was a piece of cake. I remembered those things, and they decided they had to have me. [laughter]

BOHNING: Did they give you any indication of what you'd be doing when you came to Phillips?

HOGAN: No. I thought I was going to be working in the research building, but they had a little pilot plant out halfway between Bartlesville and the little town of Dewey, which is four or five miles north of Bartlesville, and that's where I ended up for five years.

BOHNING: Out at the pilot plant?

HOGAN: Well, there were pilot plants out there, but there were laboratories too, and I was in a laboratory out there. It was a poor place to work, but it didn't bother me as long as the work was challenging, which it was.

BOHNING: Do you remember what your first salary was?

HOGAN: Yes. Do you want to hear it? [laughter]

BOHNING: Yes.

HOGAN: It was exactly twice what I had been getting in my high-school job, and twenty-five dollars more than I'd been getting over at Oklahoma State—two hundred and fifty dollars. That was 1944. They hired another guy at the same time, and I made the mistake later of telling him how much I made. He asked me and I told him. Well, he was unhappy and left Phillips because I was making more than he was. [laughter]

BOHNING: I think that was the time when they still relied on people not disclosing their salaries to others, and I think that was probably pretty common.

HOGAN: Well, I found out that was wise, after that experience.

BOHNING: Who were you reporting to when you went out to the laboratories there?

HOGAN: Grant Bailey, who interviewed me, assigned me to work with an older chemist, fifty-five or sixty at the time, and I reported to him. They put me on a project and just he and I were working on it, and that lasted about a year. He had an idea of something new that he wanted to work on and they wouldn't let him, so he quit and went over to Arkansas and started selling liquor. I heard later that he said he could make a lot more money doing that than working for Phillips. [laughter] Then I was put under Dr. Alfred Clark who had just come to Phillips, and he was an outstanding scientist and I really benefited from him. My first guy didn't teach me a lot except how to blow glass. He did show me how to blow glass.

BOHNING: You started in 1944.

HOGAN: Right.

BOHNING: What was that first project you were working on with Bailey?

HOGAN: It was double bond shifting, shifting the double bond in 1-butene to produce 2-butene, which made a much better, higher octane alkylate when it was alkylated with isobutane. That was the project we worked on for a year, and that was over a heterogeneous catalyst.

BOHNING: So then in 1945 you were moved to Clark.

HOGAN: Right.

BOHNING: Still out at that facility?

HOGAN: Yes, I was out there until 1949.

BOHNING: By 1945 the war was over or winding down at least, and Phillips was probably turning its attention back to moving on to other things now that the war was ending. As I understand it, there was a lot of natural gas, which resulted essentially in a lot of propylene and ethylene being available from cracking of gas. Is that what you were working on, trying to convert that to gasoline?

HOGAN: This double bond shifting, yes, converting 1-butene to 2-butene so it would produce a higher octane aviation gasoline.

BOHNING: What were you then doing with Clark?

HOGAN: I switched over almost immediately to a project called Fischer-Tropsch—those are two German names—which involved a mixture of hydrogen and carbon monoxide passed over a catalyst to produce a liquid hydrocarbon or even a solid hydrocarbon. I worked on that for between three and four years. We worked out a really good process and built a pilot plant, and I was in charge of that, but by the time we had the process ready for commercialization, the price or value of natural gas had increased so much that it was no longer feasible. When we started working on it, it made good sense, but three or four years later the situation had changed so much, pipelines were being built and natural gas was beginning to be piped all over the country.

BOHNING: When did you first meet [Robert L.] Banks?

HOGAN: He came in 1946, I think, to work for Clark, and that's when I met him.

BOHNING: Okay. Was he working on the Fischer-Tropsch project with you?

HOGAN: Yes.

BOHNING: How many other people were involved?

HOGAN: During the pilot plant a couple of years, there were six or eight chemists and chemical engineers. Banks was a chemical engineer. By 1948, I had four people working under me, and we were running shifts with those guys on that pilot plant.

BOHNING: What kind of person was Clark to work for?

HOGAN: I worked for him either directly or indirectly under his supervision for many years, from 1945 to sometime in 1956. Up until the last couple of years, I really benefited from working for him. He had a keen mind and he encouraged us to stretch our ideas, and in his

branch he would have seminars in which we would take turns discussing some of the more complicated things that were coming out, catalysis particularly, and that sort of thing. The last couple of years I began to butt heads with him a little bit. He had some other guys in another group working for him and researching a catalyst that Banks and I had discovered, to get the mechanism. Between the two of them, they came out with a mechanism and a deduction of what the actual catalyst consisted of after it was polymerizing.

[END OF TAPE, SIDE 1]

HOGAN: It turned out they were totally wrong, and I discovered it. I wrote a letter to him telling him what I'd discovered, and he wouldn't even let it be circulated to his boss. He didn't want to be embarrassed, I guess. But over the whole I benefited by getting to work under him. I learned an awful lot from him.

BOHNING: Your principal focus was still in catalysis.

HOGAN: Oh, yes.

BOHNING: I'm trying to put a time frame together, working up to 1951. You mentioned 1948. Let's see. Banks came in 1946, and you worked on the Fischer-Tropsch project. When did the Fischer-Tropsch project end?

HOGAN: It ended in 1949, probably the early part of 1949.

BOHNING: Okay.

HOGAN: Along about 1947, Clark became a branch manager, so I was under the direct supervision of a new section manager who came in, a young Ph.D. He was my supervisor from 1948 to 1956, and Clark was the branch manager.

BOHNING: Was that [Clarence] Lanning?

HOGAN: Yes, Lanning.

BOHNING: The nickel oxide catalyst had been discovered before the war but hadn't really been developed.

HOGAN: Yes.

BOHNING: Was that Clark and somebody else?

HOGAN: No, it was Bailey and [James A.] Reid. Reid at that time was the Director of Research. He was the head man in research.

BOHNING: Okay, so they had discovered the nickel oxide catalyst.

HOGAN: Yes, they had made this discovery and had a patent (1) by the time we took it up and started investigating it.

BOHNING: So when Fischer-Tropsch ended, that's when you and Clark went back to the nickel oxide catalyst?

HOGAN: Right.

BOHNING: Whose decision was that, to go back to that catalyst?

HOGAN: I imagine that Jim Reid, being Clark's boss, had pointed Clark in that direction. Reid got promoted far enough that he didn't get to finish his project. He didn't feel like he had really exploited that discovery sufficiently, so I have an idea it was he who pointed Clark in that direction, or it could have been Grant Bailey.

BOHNING: And then Clark assigned you and Banks together to work on it?

HOGAN: Right. The Fischer-Tropsch group broke up, and it was just Banks and I for a while working together, at the beginning. We actually started that work out at the pilot plant, but about the middle of 1949 we moved to what's now called the Research Center. It was called a proving station then. After we moved, we began to collect some other people, technical and non-technical both, to work with us, as we expanded that work.

BOHNING: What kind of reactions were you working with, using the nickel oxide catalyst?

HOGAN: The first project we had was codimerizing 2-butene and isobutene to produce a C_8. The one that we were trying to produce, and did produce, was 223-trimethylpentene, which was hydrogenated to produce 223-trimethylpentane. The 224-trimethylpentane was called isooctane then. It was a 100 octane fuel that went into aviation gas. The 223-trimethylpentane had a higher octane. It was up around 108 or 110, so we were trying to optimize the production of that by codimerization. The challenge was to separate out all the isomers that you get in a situation like that and determine what percentage you're getting of the one you really want.

BOHNING: How successful was that?

HOGAN: We had good success with it. We would do the dimerization, and of course we'd get some trimer too, but we'd separate out the dimer and hydrogenate it. That made it easier to separate the branching of the isomers. It was pretty successful, but the war was over and there was less of a push to get super high octanes. There didn't seem to be as much interest in that as in some other things, so we began to branch out into processing some cat cracker streams that were coming off the catalytic crackers and that contained ethylene and propylene and butenes, to produce gasoline component hydrocarbons from those.

BOHNING: Where were these crackers? There was nothing production-wise here in Bartlesville.

HOGAN: No, it's in Borger and in Sweeny, Texas.

BOHNING: What was the working relationship between you and Banks? How did you determine who was doing what? Did you meet regularly to discuss the project?

HOGAN: I was kind of the leading man. I had gotten there ahead of him by two years, so he was reporting to me, mainly. As soon as we got some technicians, then the way we worked a good part of the time was, during the day shift he would work with a technician with one reactor, and I would have another one going, probably with another technician. When we got to the point where we were having some success in running the three shifts, then we would use technicians for that.

BOHNING: What were the reactors like? As I understand it, these were very small operations.

HOGAN: Yes, they were an inch in diameter and two or three feet long, and had a jacket around them with a liquid in it that we controlled the boiling on by varying the pressure so that we could control the temperature. It worked pretty precisely with automatically controlled temperatures. These were what we call a fixed bed catalyst, a granular catalyst that was put in this tube, with a thermocouple well coming up through the catalyst and a little plate brazed to the bottom of it with holes drilled in it, a screen, to support the catalyst. We'd feed down through that and into a recovery system to see what we were making.

BOHNING: You really were starting with gases, though.

HOGAN: Right.

BOHNING: Did these come in tanks?

HOGAN: Yes, they came in tanks. We were using tanks of the normal size that nitrogen and hydrogen and different gases would be shipped in, and we'd send those down to the refinery for filling. In fact, there were some huge, long tanks that they used part of the time that had three times the capacity of the standard stand-up tanks; these were long ones that lay down, and they'd ship those and we'd actually use those streams right off of the plant for dimerization/trimerization tests.

BOHNING: Were they relatively pure, or were they a mixture?

HOGAN: They were a mixture. They had some hydrogen and a little bit of carbon monoxide and various low-boiling olefins.

BOHNING: So that made your job more difficult then.

HOGAN: And some sulfur compounds, too. [laughter] One problem was that by the time they got them in those tanks and got them shipped up to Bartlesville, and we got around to using them, the sulfur, mainly H_2S, had converted to mercaptans, and they were pretty hard to remove. So I remember one time I proposed to Lanning that I should go to Sweeny, take a technician with me, take a side stream right off the fractionator, run it directly through a scrubbing column to get all that H_2S out before it had time to turn into mercaptans, and then bring it back to Bartlesville and work with that. Well, that worked much nicer that way. No mercaptans. [laughter]

BOHNING: Were you experimenting with different catalysts?

HOGAN: Mainly it was nickel oxide promoted, but we did test different supports quite a bit at first. One project that we got into when we then had some success was studying how to regenerate these catalysts. They would get poisoned from the impurities that would be in this stuff—a little oxygen, a little bit of sulfur, or even nitrogen compounds. They'd get poisoned, and the problem was to regenerate those so that they'd have a number of cycles of regeneration. We found that in the course of repeated regenerations, the catalysts gradually lost activity, so we began to experiment with ways of trying to stabilize that catalyst so it would stand up better under regeneration, and that's how we discovered the catalyst that would produce solid polymers, when we put some chromium oxide promoter in there.

BOHNING: That was 1951 when that happened, so you had been doing this about three years before this particular event occurred.

HOGAN: Well, 1949 to 1951, about two years. We started working on it in the spring of 1949, and it was June of 1951. I'll show you something. When we made that discovery, I wrote a patent idea. This is the patent idea.

BOHNING: Oh, my goodness.

HOGAN: We had a celebration when we finally won the polypropylene suit, and the patent firm that was representing Phillips came to Bartlesville and had a big celebration out at Woolarock, which is the Frank Phillips ranch.

BOHNING: Oh, yes, I have been there.

HOGAN: They made up a number of those patent exhibits, and I got one for each child and one for myself.

BOHNING: Wonderful.

HOGAN: That's the patent there, on polypropylene, finally issued thirty-two years after we discovered it (2).

BOHNING: Is there a larger copy of that somewhere?

HOGAN: Yes, I think I've got one.

BOHNING: I'd like to have a copy of that for the file.

HOGAN: Yes, that's hard to read.

BOHNING: During this two-year period, somebody had to keep looking at the management. From a management standpoint, how much success were you generating, and who was selling management on keeping the project going before the solid polymer discovery?

HOGAN: There was enough interest in it that people were coming to see our work, and Al Clark was taking care of most of this, talking to people in the refining department and in the engineering department and so forth. He was interesting those people enough that they would come out occasionally and see our laboratory and talk with us. Clark was doing a pretty good job of keeping lots of interest going on this project.

BOHNING: Because that's usually a very crucial part, that you have someone who's going to bat for you in management and going to keep the funding coming.

HOGAN: Well, Clark had Jim Reid's ear. As Lanning once said, he would go down the hall to Jim Reid's office pretty often, and Jim had a lot of respect for him and listened to him, so we didn't have any problem there.

BOHNING: I have a quote here from you that says, "As for catalysts, I was trying to develop some fundamental knowledge of what makes them work, even though a lot of our work involved the Edisonian approach of taking chemicals right off the shelf" (3). Would you comment a little more about that aspect?

HOGAN: Well, when Banks and I conferred and decided we ought to put chromium on the catalyst, he had been reading the literature and he and I both had read something about chromium having possibly the effects we were looking for, even though he was reading a different article than I was reading. It wasn't just a blind Edisonian approach; it was looking at the literature, trying to study transition metals and see what their nature was that differed from nickel and how they might help out. But when this discovery happened, we certainly hadn't theorized that, "Hey, we might get solid polymers." Once the discovery was made, I began to set up some equipment and do some research. I wasn't sold on what Clark and his other scientists were doing on some of the things that were coming up with it. It just didn't quite add up to me, so I set up some equipment on my own and began to do research on it, and that's what I was referring to there, really trying to find out exactly what was going on.

BOHNING: June 5, 1951 was the date of that experiment. This was the first time you had added some chromium to the catalyst.

HOGAN: Right.

BOHNING: Basically you have this long tube in which you're using propylene. Was this purified propylene at this point?

HOGAN: Yes, it was a fairly pure grade of fractionated propylene, and it was mixed with a paraffin hydrocarbon in the first experiment, probably mixed with propane. We were diluting it just in order to keep up with what was happening better than trying to feed it a hundred percent.

BOHNING: You had said earlier your feed gases were mixtures. Then you reached the point where you're using more purified material. This wasn't coming straight off the cracker anymore.

HOGAN: Right.

BOHNING: The time frame was about twelve hours, and what you were looking for was gasoline; I'm going to use that generic term.

HOGAN: You mean during that run?

BOHNING: During that run.

HOGAN: Oh, no, that was just a run during the day. We could get a run started—get the catalyst charged, get everything heated up and ready to go and started—by probably ten o'clock. It was within an hour or two later that we realized we had something new. We were running out of the reactor through a motor valve into a little fractionation column, to separate the propane and propylene from whatever product we got. I was outside the building on the slab getting into a cabinet to get something out, and I remember Banks coming out and saying, "Hey, we've got something new coming in our kettle that we've never seen before." I went in and looked at it, and of course we had nickel on the catalyst and were still making lots of liquids, but the chromium was making some semi-solids.

BOHNING: What was your first reaction when you saw the solids?

HOGAN: I think we had that patent idea written in a matter of thirty minutes. [laughter] I went in and sat down at my desk and wrote that thing out, and showed it to Bob and he said, "Sounds good to me," [laughter] so we both signed it.

BOHNING: That's interesting—in thirty minutes.

HOGAN: Well, it might have been a little longer, but it wasn't much longer. It was that same day, I know that.

BOHNING: Not many people were familiar with polymers at that point. Polymer chemistry was still in its infancy.

HOGAN: Yes. We had some rubber chemists, and that's a polymer, but they were not in our group at all and they didn't really play a part in this until we began to see what we could do with this product we were making. They got some of the samples and looked at them.

BOHNING: But had you realized immediately what you had, that it was polypropylene?

HOGAN: It had to be. There was propylene going in and solid polymer going out. I go to the library and start searching for polypropylene and solid polypropylene, and I couldn't find any reference anywhere to any solid polypropylene. Well, I take that back. There were some solids that were produced, which were more coke than anything else, but at a much higher temperature. They were not precise polymers like these were.

BOHNING: What temperature were you running?

HOGAN: It was 160 degrees Fahrenheit, something like that. Well under the boiling point of water. [laughter]

BOHNING: Which way did you go first—characterize this material better or see if you could polymerize other things? What was your next step?

HOGAN: Our next step was to leave the nickel out, just to make sure that the chromium was doing this by itself, and then the ethylene feed was next. About that time a new Ph.D. right out of school went to work for me, and we put him to work screening other olefins as Banks and I went ahead to develop the ethylene and propylene polymers. We got one more solid out of that, the poly (4-methyl-1-pentene), which is now a commercial polymer.

BOHNING: I know it was June 5 when you did propylene, but I'm not quite sure when you did ethylene. I couldn't find a date anywhere.

HOGAN: I can't give you the date. It was later that year; that's all I can tell you.

BOHNING: So your first step was to improve production of polypropylene by getting rid of the nickel and working only with the chromium.

HOGAN: Yes. Exploring the concentrations of chromium, the temperatures, and the monomer concentrations, all sorts of variables that you have to look at to optimize. One problem that showed up immediately was that these polymers plugged the bed pretty quickly. So we went to a higher volume of diluents and higher temperatures and that sort of thing to try to alleviate that, and within less than a year we went to a stirred bed to get away from that plugging problem.

BOHNING: So the propylene and the ethylene sort of developed side by side?

HOGAN: Yes.

BOHNING: I think I'm right here but I'm not sure. There were four patents filed in January of 1953 (4).

[END OF TAPE, SIDE 2]

BOHNING: One was the production process, another was the catalysts that were used, the third was the 4-methyl-1-pentene polymer, and the fourth was polypropylene.

HOGAN: Yes.

BOHNING: I didn't see anything for polyethylene, per se.

HOGAN: Well, polyethylene had been invented long before this at ICI [Imperial Chemical Industries] in England. I guess they made the discovery in the mid-1930s.

BOHNING: But theirs was very high-pressure, extreme conditions which they were using. What you were doing was the process, that first patent.

HOGAN: I'm a little vague on how much we tried to get a composition of matter patent on

polyethylene, but the thing that shot it out of the water was that it had been made from diazomethane, and it had been made by the Fischer-Tropsch process, and both were the same thing—polymethylene is what you've got. So the concentration was on process as far as ethylene was concerned.

BOHNING: One of the things I was curious about is, you said you wrote that up in thirty minutes, but it was a year and a half, from June of 1951 to January of 1953, before the patents were filed. Does it usually take that long? In other words, why wasn't the process patent filed earlier?

HOGAN: The patent division wanted to cover as many variables as we could, just to keep other people from finding a place to get in. That was the main reason. However, patent ideas that are worth filing for but not too exciting might take that long just to go through the pipeline.

BOHNING: The first patent was granted in 1958, the process patent, which seems like that happened without any problem and there shouldn't have been any problem in getting that particular patent.

HOGAN: Well, it took awhile. There were some problems because researchers at Standard Oil of Indiana had made polyethylene and polypropylene, and they had even put some chromium on some of their catalysts and maybe mentioned it. We finally got around that by reciting that their catalysts before polymerization contained trivalent chromium but no hexavalent chromium. We got around those other patents. That took awhile. That slowed it down.

BOHNING: But wasn't there a Belgian patent issued earlier (5)?

HOGAN: Oh, yes, they come out immediately. They just print them, in some of those countries. You file them over there and they're printed within a month or two. It's not like it is in the United States, where they search the literature and tell you whether you've got a patent or not or whether they think you've got a patent. In some of those countries they just let different companies fight it out.

BOHNING: What is the reason for doing something like that quickly? Just to get your foot in the door?

HOGAN: Doing what quickly?

BOHNING: Going to Belgium and getting a Belgian patent that early.

HOGAN: I guess so, yes, just to get it out there. Once we'd gotten reams of data, we wanted to get it out there and head off what we could by getting it published.

BOHNING: Let's go back to June 5, 1951. You've done the experiment in a few hours. You knew you had something new; you had written up the patent idea. Who do you tell next?

HOGAN: The patent idea goes to a liaison man between research and the patent division. He makes it into a disclosure and works with the patent division.

BOHNING: But did you tell Clark or Lanning?

HOGAN: Oh, as far as those guys, yes, as soon as we got that patent idea in the mail. [laughter]

BOHNING: Coming back to something I mentioned earlier, what is management's reaction to this? You're supposed to be making gasoline, and instead you're making polymers, which could be a whole new direction for the company.

HOGAN: I think I can answer that fairly well. Phil Arnold, who was a vice president and had R&D under his supervision, gave a paper in which he said, "Hogan and Banks were chasing a deer and bagged a rabbit." Later I thought that was a ridiculous way to say it. It should have been the other way around. We were chasing a rabbit and bagged a deer, [laughter] but that's the way he put it. He also said something about the fact that whereas somebody might have thrown the catalyst away and said, "Forget it," because it plugged the bed, Hogan and Banks found out what was plugging it. In other words, he was saying that management was right on top of it and wanting to exploit it if there was something there to exploit. We closed out our other project as quickly as possible, just to finish up some loose ends, and switched entirely over to this. Management began to throw men at us whom we didn't have. [laughter]

BOHNING: So they were committed to going into the petrochemical business at this point.

HOGAN: Oh, yes.

BOHNING: You stuck with it. I'm trying to remember when Banks left. Banks went off to do some other things.

HOGAN: It was in 1956. Bob was still under my supervision, but he began to do other research, and this was something that Clark wanted him to do. Clark wanted him to try to develop some solid alkylation catalysts. Phillips used fluorides, hydrofluoric acid, and he wanted to try to develop some solid catalysts. So Banks took off on that, and soon discovered what became the triolefin process. Have you heard of that?

BOHNING: No.

HOGAN: You put an olefin in and you get a higher and a lower olefin out. We called it disproportionation at first, and Banks became well known all over the world from that discovery. That was after he was switched away from polyethylene.

BOHNING: Basically you had two great polymers. Which of these did you try to commercialize first, or did you work on both together?

HOGAN: Well, there was a big problem with polypropylene because it was a smear of some solids and liquids over this chromium catalyst. We divided the project into two phases, one on polypropylene and one on polyethylene, and within a year we saw that polyethylene was the one that was going to get commercialized way ahead of polypropylene, because there were so many problems there. That's the reason, and that was a fairly quick success story, really. It was discovered in 1951 and commercialized; we made the first commercial scale polymers before 1956 was over—well, on the last day of 1956. [laughter]

BOHNING: Which was before the patent was granted.

HOGAN: Oh, yes. Yes, we had already licensed it to companies all around by then.

BOHNING: That was another aspect that I wanted to talk about, because I'm not sure with Phillips, but I know with other companies it was really a traumatic event to license procedures to other companies. The older thinking was, you keep everything to yourself; you keep everything in-house. You're going to get lots of risks by licensing. There were a number of companies that for a long time refused to do that, but around this time, this attitude started changing on the part of chemical companies. I wondered if you could comment about that aspect of the management's decision to license the process to other people.

HOGAN: I don't know whose idea this was. I never did know who said, "Let's do it this way," but by 1954, they were inviting people in. We had a big pilot plant going by sometime in 1954. They began to invite several companies in and several of them did license it in 1954 or 1955. We barely beat one or two of them on-stream. We came on-stream just a few months ahead of Solvay and Celanese.

BOHNING: That would have been ironic, if you discovered it and they were on-stream before you were.

HOGAN: The arrangement was probably a first for anybody in the world, because we had a very open exchange as far as the licensees of Phillips were concerned. Of course, nobody but the ones who had licensed it could come, but we had what you might call seminars; they would have topped open seminars anywhere in the world when it comes to scientific brains and technological discussions. This was all going on starting in late 1954.

BOHNING: That early.

HOGAN: Yes. It was my job to indoctrinate these people on the process. As they would come in with their scientists and engineers, I would indoctrinate them on the technical part of it, and then of course we had the engineering people who did that part of it.

BOHNING: How did you feel about it?

HOGAN: Oh, it suited me fine. I was making some good contacts from all over the world, and it was fun.

BOHNING: The polypropylene story is a little more complex, however.

HOGAN: Yes. This catalyst was never commercialized to produce polypropylene, but we made the first crystalline polypropylene that anybody made in the world. It took a while to prove it, but we proved it, and that was the composition of matter patent. Even though it was not commercially feasible, the composition of matter patent was ours, and anybody who makes it or sells it in the United States is liable for paying royalties until the patent runs out.

BOHNING: Because it wasn't granted until 1983, that means it still has a few years to go.

HOGAN: Right. You know about the interference, I guess, and who was in it.

BOHNING: Basically.

HOGAN: At first there were five companies. Hercules [Incorporated] was eliminated before it got to court because their date was just too late to be considered as part of the interference. Then DuPont [E. I. DuPont de Nemours & Co., Inc.], Standard Oil, Montecatini and Phillips were the four that then fought it out in court. That court thing ran from sometime in October to sometime in April the next year. I was the first witness. I was on the witness stand for four days. Every hot lawyer in those three companies was trying to tear me apart.

BOHNING: If you were to read court documents, you could probably get more technological information in chemistry out of court proceedings of a trial than you would out of the scientific literature. Is that true in this case?

HOGAN: I suppose, yes. I've got the volumes if you want to see them. [laughter] They are nicely bound volumes. They gave me a set of them.

BOHNING: I would like to see them later, if I could. Coming back to the polypropylene, how soon did you get rid of the nickel in the original catalyst? How soon did you find out that you didn't need the nickel at all?

HOGAN: Well, you asked me earlier, and all I can tell you is that it was that year, and it was within a month or two. I don't remember exactly.

BOHNING: Okay. But you had to continue to modify the catalyst used with the propylene in order to get the right product that you wanted, the pure crystalline polypropylene. Is that correct?

HOGAN: Yes, we just did our best to optimize the yield of the solid polymer. That went on for at least two or three years. In fact, I think I recall having somebody working under me on that as late as 1955, somewhere in there.

BOHNING: Yes, that's when the patents were filed. No, they were filed in 1953.

HOGAN: Yes. By that time we were working with much better catalysts, titanium-type catalysts. When I say "we," I mean the people in our division.

BOHNING: With polyethylene, you were involved right through to the production stage. Is that true?

HOGAN: Until I retired, I always made frequent trips to the plant. I was on the start-up committee to get the original plant started and on the main committees of that sort, right on up until I retired.

BOHNING: In 1955, you wrote a paper titled, "Polymerization of Light Olefins over Nickel Oxide(Silica-Alumina" (6). I had two questions. The first is, what was the attitude of Phillips towards publishing papers?

HOGAN: Well, that was a bone of contention. I did not get to publish anything on the chromium catalyst until 1956, I believe.

BOHNING: Okay. I have another paper in 1970 that I wanted to ask you about also.

HOGAN: The patent hadn't issued, and it was very difficult to get anything published that was scientific. Now, strangely enough, you could give technology information more than you could give scientific information. Finally, after the patent issued and the dust settled, by 1970 I was able to get some of my scientific discoveries out. Even then they didn't want me to say that I had

proven that it was divalent chromium that produced the active site, and it was not until a man named Zucchini in Italy came out with that, even though I'd known it for ten years, that I got to say that in print. [laughter] It was ridiculous that they wouldn't let me do that, but I lived through it.

BOHNING: That's a common frustration that I've found with people in research who have done things like this, that if you were in an academic setting, it would be totally different. In the 1970 paper, which was titled "Ethylene Polymerization Catalysis over Chromium Oxide," (7) you started out saying that "Conflicting views of the nature of the catalytic site have come out of recent world-wide research. The chemistry of chromium was occasionally neglected and sometimes almost repealed."

HOGAN: That was Clark's work.

BOHNING: Oh, okay.

HOGAN: It was incredible that he would say that he had proven that the active site was hexavalent chromium, when hexavalent chromium is such a strong oxidizing agent that you could expose it to ethylene at room temperature and you could see it change color and reduce. But it wasn't Clark's fault; it was the fault of this guy who was doing the research—well, Clark should have seen through it. That was what I meant there.

BOHNING: What do you think, through all of this, about the Phillips management attitude towards research and development?

HOGAN: I had no problem with Phillips' attitude towards R&D, right up to the time I retired. Right after that, Phillips became another company. Two different attempted takeovers just about put them under. It's like the old nursery rhyme about killing the goose that laid the golden egg. They decided to kill the goose that laid the golden egg, because the people who got to make those decisions were not in R&D, they were somewhere else. But that happened after I retired, so my experience with Phillips was very nice as far as management was concerned.

The CEO of Phillips was very nice to me at the time that Banks and I received the Perkin Medal. In fact, he and his wife were right there at the presentation of the medal. I got sick while I was up there, and he had business in Washington, D.C. He had his pilot come on up to New York to get me in his plane, and then stop in Washington to pick him up. He and I and my wife

came home in his jet. So I had a good experience with Phillips, but the people who are working in R&D now are not having a good experience at all, many of them.

BOHNING: I'd like to come back to that in a moment. Let me just follow up on a few other isolated things. You've already indicated that you changed the catalytic bed and developed a slurry process, and that was on-stream and optimized by 1961 or so. Also, you worked on copolymerization. There were some early problems with your high-density polyethylene in terms of stress crack resistance, so you worked on copolymerization. Could you comment any more about either of those two aspects?

HOGAN: When we went on-stream, we started making the homopolymer of seven-tenths melt index. People at the Sales Service Laboratory who had figured out what the market was going to be said, "It's going to be a seven-tenths melt index homopolymer," even though we were already making copolymers in Research and Development. They didn't know too much about the thing. They were not people who had worked in polyethylene much. Well, that didn't last very long, with our warehouse being filled up. [laughter] Sales weren't going so well. So it wasn't long until they were ready for copolymers, which we had <u>already</u> developed, and we developed some much better ones in the years following that.

The hexene copolymer is the one that really became a world-beater, just super environmental stress crack resistance. You could make the best pipe in the world out of it, gasoline tanks and everything else. That came along in the sixties. We had a big battle with Du Pont on that, another big lawsuit, for which I spent four days on the witness stand. We won that piecemeal until finally we won it all. Just in the last year, finally, DuPont gave their patent to the public, threw up their hands, and gave up. But it took a while on that.

BOHNING: You certainly have had your share of trial situations, over a long period of time.

HOGAN: Oh, yes. The first one, I was still working for Phillips, but this one I'm talking about with DuPont I was already retired. I worked for a law firm in Chicago, who represented Phillips, for several years. In fact, two years ago was the last time I did some work for them.

BOHNING: This is a quote not from you, but about you. "Around Phillips, the Hogan men are known as a hustling group with a great sense of urgency, a talent for <u>sensing</u> what is important and what is not, and a tremendous loyalty to Paul" (3). I wanted to follow up on this aspect of sensing, of intuition, because a number of other people have told me that in their decision-making process, part of it involves a gut feeling or an intuition. I wonder if you could comment on your experience with that.

HOGAN: As I said, I was on the start-up committee, and of course we began to encounter problems. Even though we had a big pilot plant running in Bartlesville, it wouldn't find out all the problems. A good part of the work of my group for several years was putting out those fires and solving the problems, and I guess there was some intuitive stuff there. One of the first problems that we ran into was, the first plant was a solution polymerization plant where the polymer was made in hot hydrocarbon solution from a very finely divided catalyst in a big stirred vessel, and then the catalyst had to be filtered out. We ran into a tremendous problem with some of the higher molecular weight polymers; the filtration process was bogging down, just stopping up.

[END OF TAPE, SIDE 3]

HOGAN: Kind of intuitively, I solved that problem with a new catalyst support. It took a while to get them to switch over to it. In fact, this is kind of an interesting comment on the inertia. One of the licensee plants tried my idea before Phillips, and this just solved the problem completely before Phillips would even try it. [laughter] That was one example. Another one was this hexene copolymer.

One of the problems they had, too, in getting the plant going, and this goes back to the question that Bailey asked me when I came to interview, "What's the big difference between the homogeneous and heterogeneous catalysts as far as the problems are concerned?" I said, "Catalyst poisoning," because I'd read that in the library at Stillwater, and that was a big problem. When the plant first went on-stream, it had all kinds of problems with not getting high enough yields. It had taken too much catalyst, you know. I just went down there, and even though I wasn't an engineer, I'd had a little bit of engineering at this school in California. I just started walking through the plant, figuring out what was wrong. Finally, I realized that the engineering had gone wrong on some of it. [laughter] I called a meeting and told them what they had done wrong, and they believed me, went out and shut the plant down, did a lot of repiping and stuff, and solved the problem. A friend of mine, who was an engineer, thought that was pretty good, that I had snowed all the engineers. [laughter] So intuition helps, and serendipity helps too, I'll tell you. That was serendipity when we made that discovery, of course. It was something that we weren't looking for.

BOHNING: But the key is that you recognize that. The old saying is, "Chance favors the prepared mind." You recognize that you're with something new that should also be looked at, rather than discarding it as an unwanted material.

HOGAN: That was another problem that Clark had. While he was still my boss, he thought that since the plant was on-stream, we ought to switch over to something else. I could see that we couldn't do that. Here we had licensed it to the whole world; we were inviting those people in; every eighteen months we'd have a big licensee meeting, and we were making more money that way, from licensing, than we were from our own plant, certainly. That had to continue on and on and on. He complained once, "Paul Hogan doesn't want to work on anything besides Marlex." [laughter] That was the trade name of the plastic. I didn't argue with that at all. That's exactly what I wanted to work on.

BOHNING: I have one other quote that I was interested in, because as all of this developed, Phillips did not bring in outside experts, as it were, in polymer chemistry, but you developed from within. Here it is. I think this is a quote from you. The key people were Phillips people. They were "without much, if any, previous plastics experience. This had one advantage that there were few preconceived ideas about plastics or the plastics industry. So most of the ideas were at least fresh if not always sophisticated" (8). This has shown up in another situation, in which not having preconceived ideas was an advantage.

HOGAN: Yes. I mentioned this young Ph.D. who started working for me, working on some of these other monomers. His Ph.D. had been done partly on rubber, and he knew about gels, and he unfortunately referred to our polypropylene solids as gels. [laughter] That cost us dearly in the lawsuit. It was just a preconceived idea that got written down on paper. They weren't gels; they were crystalline polymers. He was running the melting points and getting melting points higher than anybody had ever heard of for a hydrocarbon, and this was polypropylene, and yet he put down "gels." [laughter] He was using a crystalline melting point apparatus and measuring these things, and he still didn't call it crystalline polypropylene. It just about ruined us in the courts.

BOHNING: After these experiences with the long court cases, what is the attitude of Phillips towards records retention? Is it any different than it was in your day?

HOGAN: They had a good attitude. When I went to work there, they issued notebooks and they had to be witnessed everyday and they had to be carefully filed. The only breakdown in that was that people just wouldn't keep good data, just wouldn't write everything down sometimes, and I'm sure that has been emphasized over and over and gotten better, certainly. Other than that, I think from way back Phillips, theoretically at least, knew how to keep good records. Some of those old notebooks that we did our early records in were used in the courts for years. They almost wore those things out. [laughter]

BOHNING: Well, certainly in the polypropylene case you were able to show that you had predated everybody.

HOGAN: Yes.

BOHNING: It must have been quite a thrill for you to come out on top of that, even though it was after such a long period of time.

HOGAN: Yes, we were finally vindicated. Thank goodness it happened before Banks died.

BOHNING: When did he pass away? I don't know that date.

HOGAN: I'm going to say 1990.

BOHNING: So it was quite recent, then.

HOGAN: Yes.

BOHNING: A couple of other things. You once said, "My work interest is to see fruition of technical research which is mission oriented and based on a theoretical approach" (8).

HOGAN: I felt that what really helped my research was to have some hypothesis or some theory to guide my experiments, instead of just grabbing something on the shelf and seeing if it worked. I came out with lots of theories. In fact, people laughed at me sometimes, I had so many theories. [laughter] But most of them turned out to be true. Some of them didn't, but most of them did, and they really helped. If you have a theory, that helps you go in the right direction in your research. I saw early on that it was much better to modify the polymer with the catalyst and with process variations than to try to do it downstream. A lot of companies early on were modifying the polymer after they had it made, by doing different things to it, but it was much more efficient to do it with the catalyst or to change the process. That was a guiding light for me, to follow that trend, and it worked almost entirely.

One of the problems was that the polymer has a double bond at one end. Regardless of how big a chain, it's always got one double bond in it. Then, you can stabilize a vinyl group

that's a little too reactive, but it would be nice to have it more stable. We worked out a process of hydrogenating that double bond, and it worked slicker than a button, but the engineers killed it. They said they couldn't afford to do it. [laughter] That was the only time I ventured away from trying to do it with a catalyst or the process.

BOHNING: So what you're saying is that you could literally dial in the property of the product by adjusting the catalyst or the process conditions.

HOGAN: Or the pressure of ethylene or ethylene concentration, comonomer, or what have you.

BOHNING: Which gives you incredible control over what's going on.

HOGAN: Yes, very fine-tuned.

BOHNING: As Phillips got into the plastics business, at one point they were actually buying and reselling low-density polyethylene, until your process for low-density. In other words, as I understand it, they were looking for a whole range of products. There were customers who wanted low-density polyethylene and you didn't have it, so you were actually buying and reselling it. I was intrigued by that.

HOGAN: Well, after all, you've got a big organization of people out there selling polymers, so it doesn't cost much more. The overhead stays the same. You just buy it in quantity and sell it at a little higher price.

BOHNING: Were you involved in a low-density process as well?

HOGAN: Oh, yes, we developed that in the laboratory. There again, it was done with catalysts and process variables. I have a patent on that. It's a joint patent with one of the engineers who ran the pilot plant.

BOHNING: You came a long way from being a fuel chemist.

HOGAN: Yes. [laughter] I didn't mind.

BOHNING: Another thing I was intrigued by happened back in 1973. There you are, you have been responsible for the production of these huge quantities of two important polymers, yet you were already talking about recycling materials.

HOGAN: That was about the time that lines formed for buying gasoline, in 1973, and that got my mind onto that. In fact, people predicted then that we were going to run out of this stuff in twenty-five more years. Of course, it wasn't true, but. . . .

BOHNING: Well, recycling plastics at that time still was not very important, and not much thought was being given to it.

HOGAN: No. Did you know that Phillips has a plant now that recycles polyethylene? It's in Tulsa. When they started it up, they invited me over. I was then retired. They introduced me to the mayor of Tulsa, who was there.

BOHNING: The other thing you commented on was that your research should show a payout for the company. This comes back to what I was looking at earlier—how you keep a project going without somebody like Clark, who supported you at one point and didn't support you at another point, and how you avoid having the plug pulled on you when you've got something good going.

HOGAN: I don't think you can do it anymore, but I didn't run into that problem so I don't know how you'd do it. I didn't have to learn. [laughter] I had so many contacts outside of R&D that that really helped. I was on the start-up committee, and the people who were running that plant when it first started up later became high-up executives in the company. I was on a first-name basis with them, so that helped as far as getting the kind of support we needed.

BOHNING: You spent your entire career in Phillips in research. Had you ever thought about doing anything else?

HOGAN: Yes, one time I did, when I had been at Phillips two or three years. My brother was doing well in the TV business, and my folks were still living then and my wife's folks were living, both back in Kentucky, and I talked to my oldest brother about joining him in the TV business. He was kind of an introvert and didn't communicate well with people, and I'd had a

course in radio when I was in California, so I gave it some consideration. My wife said, "Nothing doing." [laughter] That was the only time that I had ever considered another job. That was before things really began to happen at Phillips.

BOHNING: A close call.

HOGAN: Well, not really, because she wasn't going to listen to me. [laughter]

BOHNING: Did Phillips have much of a technical ladder in terms of progression, as opposed to a management ladder?

HOGAN: They didn't when I first went to work for Phillips, but within a few years they had a technical ladder of three steps—research associate, senior research associate, and senior scientist or senior chemical engineer or whatever. That started back in about 1960. I eventually got on it in 1977 as a senior research associate, and I felt that if I had been willing to fight for it I could have gotten senior scientist in a few more years, but I'd rather do research than get into politics. They had some senior scientists who hadn't done near what I'd done, but I didn't have a doctor's degree. I had an honorary doctor's degree only, and that's the reason they didn't voluntarily give it to me.

BOHNING: Yes, because after all those years it's the accomplishment and not the formal degree that matters.

HOGAN: People will say, "Do you have an earned doctor's degree?" I tell them, "Yes, I think I earned it." [laughter]

BOHNING: What does scientific innovation mean to you from your experience? How would you describe it?

HOGAN: In simple terms, I think it's discovering new things and exploiting them. I never did have that much interest in science just for pure science's sake. Even on the work that I did for a few years on mechanisms, that was something I felt we needed to know so we'd know how to improve the catalysts. However, I enjoyed participating in the scientific meetings and things like that. I was invited to Yugoslavia and was made a fellow of the Yugoslav Academy of Science. I got more recognition abroad than I did here, until the Perkin Medal.

BOHNING: What did it mean to you to win the Perkin Medal?

HOGAN: I thought it was nice. People used to ask me, "[Karl] Ziegler and [Guilio] Natta got the Nobel Prize for doing what you had done first. Don't you feel bad about that?" I said, "No, I'm just glad they did. It would be fine if they'd give it to me, too, [laughter] but I wouldn't want to take it away from them, because they did some excellent work, both of them, just fantastic work." But it is an honor, and I got my share of honors eventually.

I might mention something that Phillips did for Banks and I that you may not know about. We both retired in January of 1985, and both of us continued going to work. I was writing papers and he was doing this, that, and the other for a while. Phillips threw a big retirement party for us, and invited Peter Grace to give the after-dinner speech. At that time he had just finished his study on how to improve the government, what [Ronald] Reagan had asked him to do, so he was well known for that and already well known of course among companies. They also invited professors from the big universities all over the country to come and have a scientific catalysis seminar that week. They did all that for us.

BOHNING: That's wonderful. I was going to ask you how Phillips treated you in terms of what your discovery did for the company.

HOGAN: Then when we had this celebration, they admitted that Hogan and Banks had made Phillips multibillion dollars, but we didn't get much of it.

BOHNING: Did you get the traditional one dollar per patent?

HOGAN: No, it went up to where a patent was worth three hundred dollars. You got a hundred for filing and two hundred when it issued, a total of three hundred. It was split if there were co-inventors.

BOHNING: So when the polypropylene patent was finally issued in 1983, you got a hundred and fifty and Banks got a hundred and fifty?

HOGAN: That's right. [laughter] If you got fifty patents, you got five thousand dollars. I finally collected that four or five years after I retired. Some patent applications finally jelled and I got my fiftieth patent. If you got a hundred patents, you got ten thousand dollars.

BOHNING: A hundred patents is quite a goal to achieve.

HOGAN: There were a few, three or four guys, whom I know of who got a hundred. One guy worked under me a few years, and he would patent anything. He had a brilliant mind and he passed the hundred mark after I retired.

BOHNING: You've already talked about the company's attitude towards R&D while you were there, and you mentioned something about the current attitude of the company about R&D, but in a general sense, what do you think is important for the future of chemical R&D?

HOGAN: I've been out of this so long that I'd be hard pressed to give a very brilliant answer to that question. Phillips is beginning to at least sound like they're swinging back to support R&D. The chief executive recently made a statement to the effect that he believed in R&D and wanted to increase their R&D effort again, so certainly that's really important. I don't know how you get people who are getting doctor's degrees to come out of that and be innovative. Some are and some aren't, and I don't know whether it's mainly the person or whether it's their experience. The guy whom I just mentioned who got a hundred patents—I think that was partly because of the influence that he ran into at Northwestern [University]. He went to Northwestern, and the professor whom he worked under there in catalysis turned out to be a good friend of mine because he had done some work on chromium, not for polymerization but in cracking and other things, so we had some things in common and I learned from that. This professor was a brilliant, innovative kind of guy, and I'm sure this inventor got some good stuff out of that. Then he got a postdoc at the Catalysis Institute in Lyon, France. I visited that place. In fact, we'd had one of his innovative professors over to speak to us here in R&D. He had some real good influence there in catalysis, so I think it makes a difference whom you're working under and whether they're innovative or not.

BOHNING: I've run out of my questions. Is there anything else you want to add that I haven't covered?

HOGAN: Is that that list? Let me look at it and see if I get inspired to say anything else.

[END OF TAPE, SIDE 4]

HOGAN: With regard to professional networking with peers inside and outside of the company, I think the licensing agreements we had with the other companies facilitated that. We had some good companies in the United States—Celanese, Allied Chemical, Union Carbide. They had some great people and I got to have discussions with them and got some inspiration out of that. I also was able to travel to Europe a number of times to visit the Phillips licensee plants over there, and in the States too, and talk to the people who were doing research at the plants. So that was quite helpful. That networking was just automatic because of the licensing agreements that we had.

BOHNING: There's another question on that list about teamwork. Would you say there was a real teamwork kind of philosophy at Phillips?

HOGAN: There certainly was then. Of course, there are always guys who are buttering their own bread, working harder at getting ahead than at doing good things for Phillips, but that was minor and teamwork was just super during the development of polyethylene. Within a year of its discovery, sometime in 1952, we already had a pilot plant on-stream, even though at that time there was a research division and a development division—they were separate. They put them together later. So even though they were separate, we worked real close with each other. When the big pilot plant came on-stream, I attended their weekly meetings that they had, and they were involved in attending ours, so that we both were on top of what was going on.

BOHNING: Thank you again for spending a couple of hours with me this morning. I appreciate it.

HOGAN: Well, I enjoyed it. Good to meet you. You asked some good questions.

[END OF TAPE, SIDE 5]

NOTES

1. Grant Bailey and James A. Reid, discovery of nickel oxide catalyst, U.S. Patents 2,381,198, issued 1945, 2,581,228, issued 1952, and 2,606,940, issued 1952.

2. J. P. Hogan and R. L. Banks, the invention of crystalline polypropylene, U.S. Patent 4,376,851, issued March 15, 1983 (application filed 27 January, 1953, Serial Number 333,576).

3. John F. Henahan, ed. "The Chemical Innovators—8. John P. Hogan: The near-perfect molecule," *Chemical & Engineering News*, Nov. 2, 1970: 38-40. Quotes, pp. 38, 40.

4. J. P. Hogan and R. L. Banks, low-pressure process for production of high-density polyethylene," U.S. Patent 2,825,721, issued March 4, 1958 (application filed 27 January, 1953, Serial Number 333,576).

   J. P. Hogan and R. L. Banks, catalyst [hexavalent chromium oxide on silica, alumina, or silica-alumina] used in the low-pressure process for production of high-density polyethylene, U.S. Patent 2,951,816, issued September 6, 1960 (application filed 27 January, 1953, Serial Number 333,576).

   J. P. Hogan and R. L. Banks, production of 4-methyl-1-pentene polymer, U.S. Patent 4,342,854, issued August 3, 1982 (application filed 27 January, 1953, Serial Number 333,576).

   J. P. Hogan and R. L. Banks, the invention of crystalline polypropylene, U.S. Patent 4,376,851, issued March 15, 1983 (application filed 27 January, 1953, Serial Number 333,576).

5. J. P. Hogan and R. L. Banks, production of low-density polyethylene, Belgian Patent 530,617, issued 1955 (application filed 24 January 1955).

6. J. P. Hogan, R. L. Banks, W. C. Lanning, and Alfred Clark, "Polymerization of Light Olefins over Nickel Oxide—Silica-Alumina," *Industrial and Engineering Chemistry*, 47 (1955): 752-757.

7. J. P. Hogan, "Ethylene Polymerization Catalysis over Chromium Oxide," *Journal of Polymer Science: Part A-1*, 8 (1970): 2637-2652.

8. J. Paul Hogan, "How an Oil Company Got into Plastics," *The Chemist*, February, 1973: 46-50. Quotes, pp. 47, 49.

INDEX