CHEMICAL HERITAGE FOUNDATION

JAMES D. IDOL, JR.

Transcript of an Interview
Conducted by

James J. Bohning

at

Rutgers University

on

8 December 1994

(With Subsequent Additions and Corrections)

ACKNOWLEDGEMENT

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

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(Signature) James D Idol
(James D. Idol)

(Date) December 8, 1994

(Revised 17 March 1993)

JAMES D. IDOL, JR.

1928	Born in Harrisonville, Missouri, on 7 August

Education

1949	A.B., chemistry, William Jewell College
1952	M.S., chemistry/organic, Purdue University
1955	Ph.D., chemistry/organic, minor chemical engineering, Purdue University

Professional Experience

	Standard Oil Company, Ohio [SOHIO]
1955-1956	Project Associate
1956-1960	Project Leader
1960-1963	Research Associate
1963-1965	Section Supervisor
1965-1968	Research Supervisor
1968-1977	Research Manager
	Ashland Chemical Company of Ashland Oil, Inc.
1977-1979	Research Manager
1979-1988	Vice President & Research Director
	Rutgers University
1988-	Distinguished Professor
	Director, Center for Packaging Science and Engineering
	Deputy Director, National Center for Plastics Recycling Research

Honors

1965	Modern Pioneer Award, National Association of Manufacturers
1968	Chemical Pioneer Award, American Institute of Chemists
1971	Citation for Achievement, William Jewell College
1974	Joseph P. Stewart Distinguished Service Award, American Chemical Society
1975	Creative Invention Award, American Chemical Society

1976	Special Merit Award, Standard Oil of Ohio [SOHIO] Board of Directors
1978	Life Fellow, American Institute of Chemists
1979	Perkin Medal, Society of Chemical Industry
1980	Honorary D.Sc., Purdue University
1986	Member, National Academy of Engineering
1988	Fellow, American Association for Advancement of Science
1988	F.G. Ciapetti Award and Lectureship, Catalysis Society of North America
1991	Rutgers University Diploma of Recognition, Distinguished/Named Chairs
1994	American Management Association Council Service Award
1996	National Historic Chemical Landmark Designation to SOHIO Acrylonitrile Process

ABSTRACT

James D. Idol begins his interview with a description of his childhood in Harrisonville, Missouri. His interest in chemistry was encouraged by neighbors and by friends. He attended William Jewell College in Liberty, Missouri, where he studied chemistry under Professor Frank Edson and graduated with an A.B. in 1949. He immediately went on to graduate school at Purdue University, where he studied under Dr. Earl McBee. His interest in industrial chemistry led him to minor in chemical engineering. Upon receiving his Ph.D. in 1955, he went to work for Standard Oil of Ohio, where he soon pioneered an economically advantageous process for the production of acrylonitrile. He then served as part of the team that developed a plant in Lima, Ohio for the commercial production of acrylonitrile in a record-breaking three years. He then turned his attention to novel uses for acrylonitrile, which led to *Barex*® resin, among other things. In 1977, he moved on the Ashland Chemical, where he occupied a position that combined management and scientific duties. At Ashland, he developed the propylene-CO process for methyl methacrylate. His career in industry ended in 1988 when he was invited to become a professor at Rutgers University and head of the Center for Packaging Science and Engineering. He pays special attention to individuals who have influenced him throughout his life, and he concludes with some personal insights on the meanings of innovation, teamwork, and research in science and technology.

INTERVIEWER

James J. Bohning is currently Visiting Research Scientist at Lehigh University. He has served as 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 written for the American Chemical Society News Service, and 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.

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INTERVIEWER: James J. Bohning

LOCATION: Rutgers University

DATE: 8 December 1994

BOHNING: Dr. Idol, I know you were born in—

IDOL: How about just Jim?

BOHNING: All right. I know you were born in Harrisonville, Missouri, on August 7, 1928. Could you tell me something about your father and mother, and your family background?

IDOL: My father was a third-generation newspaper editor and publisher. We published a country weekly called the *Cass County Democrat*, which was not as political as the name sounds; it was just the name of the paper as it came into being. My mother, a graduate of the University of Missouri [UM] and the University of Chicago, was a teacher of languages in Missouri schools, and spoke five languages and played the piano. I have one sister who is now married and lives in Arlington, Virginia.

We had a very happy childhood. Harrisonville was a city of twenty-three hundred people, and in the early 1930s, we endured the dust storms and Depression. You'll probably want to know about this, so I'll save us a little time and trouble. The Clatworthy family who lived next door to me had a son who was eight years older than I. He was an Eagle Scout and was in the process of getting his chemistry merit badge. At the age of six, I was exposed to his chemistry set and became fascinated with it and helped him do his Boy Scout [of America] experiments.

BOHNING: I didn't realize Boy Scouts had a chemistry merit badge.

IDOL: Yes, indeed. [laughter]

BOHNING: Your local schooling was all in Harrisonville?

IDOL: All in Harrisonville. We had the standard eight grades of elementary school and four years of high school, which I managed to get through in three-and-one-half years.

BOHNING: What about extracurricular activities and athletics?

IDOL: Not too much in athletics. Even as a boy, I enjoyed golf and tennis. I was never tough enough, I guess, for football, and I was too slow for basketball. As I said, I did enjoy golf at an early age, did a lot of fishing and camping with my grandfather and father, and became something of an outdoorsman.

BOHNING: You said you started doing chemistry experiments at the age of six?

IDOL: Well, helping is a better way to put it.

BOHNING: What was it about those experiments that fascinated you?

IDOL: Well, simply that one seemed to have some power to observe things that you could control, and you could hopefully expect what was going to happen; when it didn't, it gave you something to puzzle and wonder about.

BOHNING: Did you ever acquire a set of your own?

IDOL: Oh, yes! I started on that very quickly after having early lessons from Ed Clatworthy. I began to assemble my own chemistry set by the time I was in the second grade, and it gradually grew until it occupied about a third of the basement of my house. [laughter]

BOHNING: I'm assuming your mother and father supported you in this, or were they tolerant, at least?

IDOL: With some apprehension, I would say. [laughter]

BOHNING: Did you manage to smell up the house on occasion?

IDOL: On a few occasions, and once I blew out a basement window drying some nitrogen iodide. [laughter]

BOHNING: But you were still encouraged to do this?

IDOL: Yes, I was. My father, I think, and the rest of his side of the family were a bit disappointed that I didn't take up the journalistic profession, which my sister obligingly did—she was a journalism graduate from Missouri University—but I seemed always to enjoy chemistry and science. Chemistry was always the favorite. I considered medicine for a while, but chemistry was really more fascinating. My family was of average means during the Depression, but the price of a medical education was not in our pocket at that time. That probably played a role.

BOHNING: I'm assuming that because of the newspaper business, your family survived the Depression reasonably well.

IDOL: Yes, the newspaper business was a good business to be in, and it brought with it the opportunity to get acquainted with some fairly prominent people. My father was not a close friend, but a good friend of Harry Truman. I believe I recall him being at dinner at our house a couple of times when he was a judge of Jackson County Court in Kansas City.

BOHNING: That's interesting. I'm also assuming that from your mother's standpoint, being as well educated and prolific in languages as she was, she must have had some influence on you.

IDOL: Very much so. I suppose one must always lean a little bit towards one parent or the other, and possibly because she was a little bit more sympathetic to the smells and explosions than father was, that had its effect. During World War II, Dad was just a little bit too old to be drafted, but he was determined to serve, so he went to the [American] Red Cross and served four years as Field Director of the Red Cross for the China, Burma, India Theater. Then he was gone four years, and it was a single-parent home all of that time, so I guess that unavoidably got me quite a bit closer to my mother than my father. Let's be sure to get my wife's importance and support on the record. We met in Washington just at the time I was changing jobs and cities in the mid-1970s. Without her support, life after SOHIO [Standard Oil of Ohio] might have been a different story than the great one it was.

BOHNING: What was your science experience like in high school?

IDOL: Well, it was, I think, above average. I think Harrisonville High School was not as particularly noted as some others are for science instruction. It was a small high school; it had two hundred fifty students in it, and the superintendent taught chemistry and physics courses, ably.

I was more influenced during those years by a close family friend, who was the superintendent of the city light and water plant, Mr. Cleo Brown. He and my father were business acquaintances, because Father was mayor of the city in the years just prior to World War II. Cleo was not a chemist himself. I believe he was not a college graduate, but was, of necessity, conversant in water chemistry testing and related things. He loaned me an old chemistry book, which really was my first introduction to any organized form of chemistry education, and that was handed to me when I was in the sixth grade. Then he helped with a steady supply of laboratory equipment and chemicals, which I never had enough money for.

BOHNING: [laughter] You don't remember what the book was, do you?

IDOL: I wish I did. I preserved it for many years in one of the laboratory drawers in the basement. I arrived home from vacation one time many years later, when I was first employed, to find that mother had finally decided that the collection of materials in the basement, which hadn't been touched for fifteen years, was becoming a hazard, and it was all gone. Donated, I must say, to the local high school, but after they had taken their pick, I don't know where the rest of it went. That book was in one of those drawers.

BOHNING: I guess I'm not familiar with William Jewell College; I know it's a Baptist college. I don't know where Liberty, Missouri, is in comparison to Harrisonville.

IDOL: Well, Harrisonville is 40 miles south of Kansas City, near the Kansas line, and Liberty is 15 miles northeast of Kansas City. Liberty is halfway between Kansas City and Excelsior Springs, which might ring more of a bell. It's a very old city. Supposedly, one of the wagon trains that went on the Oregon Trail, I think it was, left from the courthouse steps in Liberty.

William Jewell was founded in 1949 by a wealthy physician from Columbia, Missouri, who was a Baptist and wanted to have a Baptist college. I had no connection with the Baptist Church, except the fact my grandmother was a Baptist. It had, for a small college in western Missouri, quite a good reputation in science and mathematics especially, so, as a matter of elimination I suppose, combining the best available with what could be afforded, I wound up going to William Jewell.

I'm glad I did. I think there's very, very much to be said with the idea of the small liberal-arts college, at least in those days. It's a beautiful campus and I would do it again, whatever choices were given me. I could have gone to UM, or UKansas [University of Kansas], or something else, but I chose William Jewell.

BOHNING: What was the chemistry department like?

IDOL: The chemistry department was a two, sometimes two-and-a-half man department, staffed by two professors, who became, really, my tutors, and in later years, fast friends. The person in charge of the freshman chemistry sections was a gentleman by the name of Professor Henri Godfriaux, who in fact had, it turns out, known my mother, since she was a native of Marshall, Missouri, and he had taught in Marshall High School. He also coached, part-time, the football team. The real figurehead there, who probably did more to get my career directed and launched than anyone else, was Professor Frank Edson, a Ph.D. from the University of Colorado, who taught the upper-level undergraduate courses. I just can't really say enough about Dr. Edson. He was a scholar's scholar and a scientist's scientist.

BOHNING: What were their physical facilities like?

IDOL: Well, they were quite good, for the size of the school and the time. We had pretty modern equipment in all of the analytical and organic and physical chemistry courses. We had first-class spectrometers to work with, as physical chemistry students, and we had the latest in organic laboratory equipment. The freshmen laboratories were a bit Spartan, as many of them are, but the equipment was pretty good.

BOHNING: Were there many chem majors?

IDOL: During the years I was going through, I believe we graduated probably eight to ten chem majors per year, out of a graduating class of probably—I'm sorry I don't have that figure—a hundred or one hundred and fifty a year.

BOHNING: That's a good percentage.

IDOL: Yes it is. Physics was also strong at William Jewell and so was math, and biology was quite good as well.

BOHNING: What part of chemistry interested you most, as you took different types of courses?

IDOL: Well, I don't think I really made a choice, by the time I had come out of school. I had looked forward to physical chemistry, because I was always fascinated with the way physical chemistry enabled one to mathematically correlate observations with theory and offered one, at least presumably, the opportunity to calculate what might happen, instead of watching it happen. But I liked organic very much, from the standpoint of synthetic chemistry.

BOHNING: Did you have any chance to do any research of any kind?

IDOL: In the senior year, during the time I was there, Dr. Edson offered a senior seminar-type course in "Introduction to Chemical Literature and Research." I did a kind of short little senior research project, which he of course had devised, and it was to assess the corrosivity of different types of acidic materials on the metal cadmium. I'm not just quite sure why he chose cadmium, but at any rate, I was presented with a series of cadmium coupons and told to go ahead and do the research on it. I set up a series of experiments to determine, largely by weight-loss techniques, what this corrosive effect would be.

So the project got launched, and most of it got done by the time I had to graduate at the senior commencement; it was a second-semester course. It was very fascinating. It was kind of irritating to find that some of the coupons didn't behave as they were supposed to. Since some of the corrosion products were not soluble in the solvent, the coupon gained weight instead of losing it. We had to make some baseline adjustments, but he was fairly pleased, I think, with the little bit of work I did. It was my first introduction to *Chemical Abstracts*.

BOHNING: I was going to ask if you had had a course either in the history of chemistry or chemical literature.

IDOL: Well, I had [Prof. Melvin G.] Mellon's course at Purdue [University].

BOHNING: Oh, yes.

IDOL: Dr. Mellon's Chemical Literature course is, I think, without peer. But even at William Jewell, Dr. Edson got us into that. In his lectures in organic and physical chemistry, he was very careful to point out the contribution of historical figures in chemistry. We were lucky to have him do that.

BOHNING: What was your thought as you were going through William Jewell, about what you were going to do with your career after you left?

IDOL: Well, I really never thought of anything other than graduate school. When I got my degree from William Jewell, in three-and-a-half years, I was urged to take a couple of job offers just to make sure that I wasn't depriving myself of something that I would have been interested in, so I interviewed with an insurance company, which made me a rather astonishing offer, which I almost took. [laughter] The money sounded pretty good, but I applied to four graduate schools and was accepted by all. These were Cornell [University], Purdue, [University of] Illinois, and Iowa State [University]. I chose Purdue, largely upon the recommendation of Dr. Edson. I had begun to become interested in industrial chemistry and chemical engineering, and he thought that would be a fine solution to that ambition.

BOHNING: What developed your interest in industrial chemistry?

IDOL: I don't really know. I suppose the study of processes for production of materials, as we were presented with them in inorganic and freshmen chemistry, and later in organic chemistry.

BOHNING: You went to Purdue in 1949. What was Purdue like when you got there?

IDOL: Well, it was eleven thousand students, and the chemistry building was a third of the size that it was when I left, and a sixth of the size that it is now, since they've built the second chemistry building. The faculty, as I recall, was about twenty or twenty-five, and at the time I arrived, Earl McBee, who was a William Jewell graduate, had just become chairman of chemistry at Purdue, so I guess it was foreordained that I would work for him. [laughter] I had developed an interest in halogen chemistry, even before leaving William Jewell, so that made the choice pretty easy.

BOHNING: What kind of research did you do? Well, let me back up a moment. You've mentioned Mellon's course. I'd like to ask you a little bit more about that course, about Mellon and what your reaction was to him.

IDOL: Well, Dr. Mellon was an unusually talented and absolutely fabulous teacher. It was a pleasure just to sit in the class and listen to him lecture about the value of chemical literature, and how it should be approached and handled. That was, of course, before the days of computers, and it was all hand-work. We were led through Beilstein [*Beilsteins Handbuch Der Organischen Chemie*] and *Chemical Abstracts* and the other reference works in inorganic chemistry, like Gmelin [*Gmelins Handbuch Der Anorganischen Chemie*]. How they organized the literature was totally fascinating.

BOHNING: I have a copy of your paper here with McBee. It was a paper from your Ph.D. thesis, "Ultraviolet Spectra of Chlorine-containing Cyclopentenes" (1).

IDOL: My, you've done your homework. [laughter] Actually, my master's thesis, which for some reason we didn't publish—I think maybe he was waiting for some others to come along that he could combine it with—was on substitution reactions of 1, 2-bistrifluoromethylbenzene, hexafluoroxylene, if you will. That was interesting. We went through some exercises to make some novel nitro, amino, and diazonium compounds, and things like that. It was sponsored by Westinghouse [Corporation], so I was a Westinghouse Fellow at the master's level, which was slightly unusual.

BOHNING: So you didn't do any teaching?

IDOL: Yes, I taught extensively. Due to some reverses in the family business, the financial situation in the family had gotten pretty rugged at that time, and my sister had also entered college, so I did quite a bit of extra teaching. The result was that I stretched my Ph.D. education out a little bit longer than it otherwise might have been.

BOHNING: I see.

IDOL: That also was complicated by the fact that I'd decided I was going to take a chemical engineering minor for the Ph.D. I started that a year later than I should have, so that kind of spread things out. But I have no regrets for the slightly longer period of time I stayed at Purdue. It was a fascinating place, and I enjoyed it.

BOHNING: You had this interest in industrial chemistry. Is that why you selected chemical engineering as a minor?

IDOL: I think so, and Dr. McBee was very strong in his support of the idea. He was a very industrially oriented person himself, and the combination worked out to be exactly what I expected. Also, the person under whom I took most of my chemical engineering was Norris Shreve, who served on my doctoral committee, and it was interesting to sit in his classes.

BOHNING: How many chemical engineering courses did you take?

IDOL: Twelve credit hours at the graduate level. I took "Chemical Process Industries" two semesters, which used Shreve's famous textbook of the same name (2). Then one semester, as I recall, of unit operations followed. Then I took graduate courses for two semesters in unit processes in organic technology, basically an organic chemical engineering graduate course combined with a pilot-scale organic synthesis laboratory. We synthesized chemicals in ten-gallon reactors, and used plate and frame filters, and things like that, so it was really an education.

BOHNING: So you were able to combine this interest in organic chemistry, but with a leaning towards the industrial aspect?

IDOL: Yes.

BOHNING: This was despite that fact that your Ph.D. thesis was almost strictly organic chemistry.

IDOL: Yes, it was entirely organic chemistry, largely synthetic and proof of structures.

BOHNING: I'm curious how Shreve served on your committee, then.

IDOL: Well, at Purdue, one had a committee composed of one's major professor, and a professor representing each of the two minor fields. One of my minor fields was chemical engineering and the other was a split minor between inorganic and analytical, so I tried to cover as much ground as I could.

BOHNING: I was going to say you covered pretty much all of the bases, in that respect.

IDOL: It took a year longer to do it, but it was worthwhile.

BOHNING: What were your thoughts, as you were finishing up your graduate work, as to what you would do?

IDOL: Well, I was sure I was going to take a job in industry, at that point, and I had rather thought that I would like to return to my native Missouri or at least the Midwest. I had made a few trips to the East by then, largely on interviews trips, and enjoyed it, but my heart was, and perhaps still is, largely west of the Mississippi. But fate determined otherwise. When I was in the interviewing process, one day Dr. McBee brought a gentleman by the name of Dr. Everett Hughes, Mike Hughes, through my laboratory. He turned out to be the Research Director of Standard Oil of Ohio. He said, “Jim, this is Dr. Hughes; tell him some things about your work.” So I gave him my fifteen-minute, carefully rehearsed speech for guided tours. Dr. McBee was always bringing his visitors through the labs to introduce them to his students; he was very good about that. I was somewhat surprised, a week later, to receive an application form [laughter] from Dr. Hughes, so I filled it out and sent it in, and an interview trip was scheduled. An offer resulted, and I took it.

BOHNING: At what other places had you interviewed?

IDOL: Well, I interviewed at DuPont [E. I. DuPont de Nemours and Co., Inc.] and at Velsicol Chemical in Chicago, 3M, and one or two others, I don’t remember now.

BOHNING: What was it about SOHIO that made you lean in that direction?

IDOL: Well, they were not a small company, but they were not one of the giants, like DuPont, in which I was a little bit afraid of getting lost. I was very intrigued by the interview there, particularly by a gentleman by the name of Dr. Frank [Franklin] Veatch, who was a Stanford [University] organic chemist. It’s not widely known, but Standard [Oil] of Ohio was one of the earliest companies to do pioneering partial oxidation work on hydrocarbons. It’s often thought that Shell Chemical and [Union] Carbide, perhaps, pioneered that area, but if you examine the dates, I think one would find that SOHIO was among the earliest.

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IDOL: Veatch was directing research in those areas, and while he didn't reveal the details of it to me at the time, he indicated that was their area. I had, by that time, felt the need of some relief from halogen chemistry, though I continued to be fascinated by it. Partial oxidation was something I was interested in, and we had made some phthalic anhydride by catalytic oxidation in my chemical engineering course at Purdue. I thought, "This sounds pretty interesting, so let's think about this one carefully." They made an offer and I accepted, with the condition that I would work in his group. [laughter]

BOHNING: Was their salary offer competitive?

IDOL: More than quite good. I can't remember at the time, but it was close to or over six hundred dollars a month, which was at the top range that Ph.D.'s were then getting.

BOHNING: You started at Standard Oil in 1955 and moved to Cleveland. Cleveland is still in the Midwest.

IDOL: Yes. The transition to Cleveland wasn't too difficult. I seemed not to have too much trouble making friends most places, and it was a bigger city than I had ever lived in before, but I was lucky to alight in a nice part of town. The place where I lived, as we talked at lunch, was only two blocks down the street from the Grace Lutheran Church. I, having been mostly always a churchgoer and still am, found my way down there the very first Sunday and made some friends. My life in Cleveland was never dull.

BOHNING: What was the group like when you joined it? Then, for a broader picture, what was research like at Standard Oil that time?

IDOL: Well, the group that I joined was a group of about fifteen or twenty people, all reporting to Dr. Veatch, who had the title of Section Supervisor. He, along with the rest of us, advanced through the ranks largely because of the work that we all did and his leadership skills. At the time he retired, he was Director of Research for Polymers, Petrochemicals and New Processes. The original group of fifteen or twenty mostly focused on partial oxidation chemistry. It had its own small but effective analytical group, which included infrared spectroscopy capability in the laboratory, and then there was one project that was polymer related and had to do with the manufacture of microballoons, which you've probably heard of.

BOHNING: No, I'm sorry, I haven't.

IDOL: Microballoons are very, very small, less-than-a-hundred-microns hollow spheres of plastic that float on the surface in oil tanks and help prevent the evaporation of volatiles. That had already been accomplished; that was Veatch's invention. At that time, they began working on glass microballoons. I was never involved in that but was a good friend of the project leader who worked on it. I began working in his group.

BOHNING: Were you given a specific assignment when you first started?

IDOL: No, I was not given a specific one, which was something that really intrigued me. They had identified propylene-selective oxidation—or perhaps propane, but primarily propylene—as the area of probably the greatest opportunity, and they had made significant progress in a selective catalyst for oxidizing propylene to acrolein. My task was to read through as many of their reports as I could digest in a three-month period and concoct a suggestion as to what kind of research I felt should be done on the further utilization of acrolein, which was of course a very reactive intermediate.

So, with a little bit of coaching, but largely based on ideas I had developed myself, Dr. Veatch and I concluded that we should work on the further oxidation of acrolein to acrylates and acrylic acid derivatives, and/or try and selectively oxidize the double-bond to glycidaldehyde, which would have been an intermediate for glycerine. I took off in those two directions and pursued them simultaneously.

BOHNING: It's intriguing that you were allowed or asked to spend time to come up with your own suggestions for research at that time.

IDOL: I felt very good about that; I was taken completely by surprise. I was given a desk and an office, as much secretarial help as I needed, access to the library, and was pretty much left alone, except for Frank and the other fellows who would come around once or twice a week and ask, "What's new today?"

BOHNING: I was going to ask if you had group meetings, or seminars, or how you communicated amongst yourselves about what was going on.

IDOL: Well, within Veatch's group, it was small enough that communication was never a problem. We had weekly group meetings for all the senior professionals. Veatch himself was a communicator par excellence, and so there was no problem of any kind there, and there was quite a free interchange of ideas.

Within the laboratory, Hughes had each project reviewed annually, and this took the form of speaker/audience project reviews, with transparencies, lectures, and so on. He made quite a thing of these reviews, and it was a chance for the individual investigator to respond to the group as well as to the management about what they had done. I suppose they did the same thing in other companies, but Hughes did it very well, so that was a method of communicating across disciplinary lines.

Within the laboratory, there was the Petrochemical Research Group, which Veatch had. There was the Fuels Research Group, which Sam [Samuel M.] Darling had. Sam Darling was one of the other people with whom I had interviewed on my first trip there, and he was a pretty fine organic chemist himself, a graduate of [Case] Western Reserve [University]. He spent quite a bit of time explaining things to me. If I hadn't worked for Veatch, I would have worked for Darling.

Then there was the Heavy Products and Asphalt Research Division. Apart from the Fuels Research Division, there was the Fuels Evaluation, which include the motor laboratories, engine test blocks, and things like that. There was what was called the Basic Research Division, headed by Arthur L. Jones, which was doing work in thermal diffusion, some different types of catalytic conversion of materials, and other things I don't remember by now. Then there was the Analytical Section and Library, and a very modest Pilot Plant Division.

When we moved from the old Cornell Road laboratory, which is where this was all occurring in the fall of 1957, to the new research center on Warrensville Road, the structure of the organization remained very much the same. This, I think, was a tribute to Hughes' foresight. The main thing that was added at the Warrensville location, where there was room for it, was a major expansion in pilot plant capability, which by that time was needed, partly because I had made the acrylonitrile invention, which had to be pilot planted.

BOHNING: This is one thing that came up in my discussion yesterday with Edith [M.] Flanigen (3): when they moved from Tonawanda to Tarrytown, it was a disruptive influence in the research organization. Did that happen in this case?

IDOL: No. I think that organizationally it was remarkably <u>non</u>-disruptive, perhaps because it was carried out in stages, and the petrochemical group was, in fact, the first to move. We were in our laboratories and had our bench scale equipment (as opposed to exploratory scale) installed and operating at the time the rest of the groups moved in. That was Veatch; he was always one jump ahead of the rest. [laughter]

BOHNING: Do you feel that, at that time, there was a real emphasis on the part of the company—and I'm not sure how much feeling you had for the higher-up administrative part of the company—towards what Willard Dow called "patient money and a prayerful attitude"; that there was time and money to do the exploratory work, or did you feel that there was more emphasis on direction of work?

IDOL: Well, I have the impression that the senior management was very much oriented toward process research and support, because I have forgotten to mention that there was a completely different and additional laboratory—larger than the Cornell Road research laboratory, which was a development laboratory—adjacent to the refinery. A complete refinery pilot plant was in operation there at that time, and related research services and things like that. There was a little bit of competition, but in general, quite a lot of collaboration between these two laboratories.

I made some pretty good friends at that laboratory also. One I remember particularly is Oscar Kropp, who was one of the section managers there, who delighted in exchanging humors with me, [laughter] so it was pretty good.

But I would say that the organization, even at the top levels—and I think that I was able to shake hands with several of the vice presidents and the president, within the first year of my arrival—was supportive of research probably because Dr. Hughes was very, shall I use the words, "diplomatically skilled" at presenting the research case to the senior management. His skill in guiding programs and people must be recognized in a lot of what SOHIO achieved.

BOHNING: I've noticed that the success of a research group often depends on someone being able to sell the senior management on their projects and ideas.

IDOL: Well, I think that he was pretty good at that, and he was lucky in having as his superior a fellow by the name of Elliott McConnell, who was the senior vice president for manufacturing. The way Standard of Ohio was organized was that all of the R&D department was in the manufacturing part, except for a small sales tech service operation—come to think of it, not so small—that served the sales department. As expected, it was pretty well focused on solving manufacturing problems, but in those days each oil company was developing its own version of catalysts, reforming, cracking, whatever, so it was not too difficult to do some fairly scientific research, based on pretty sound engineering and science, while pursuing the catalyst research. The catalyst background was of great value in my catalytic research, so there was a combination of fortuitous circumstances.

McConnell was very forbearing and patient with the R&D group about funding and balance of exploratory versus applied research. I have to say we—I guess maybe partly by luck—made it easier for him, by giving him the acrylate and acrylonitrile processes in a very timely fashion.

BOHNING: Well, that's what I'd like to come back to. You've said that, after your three months of study, you agreed on a direction, and within two years you were in acrylonitrile. I'm wondering if you could go through some of the details of that time period.

IDOL: Sure. It's fairly well documented, by the way, in a nice article that *Chemical and Engineering News* did in 1971, where I was declared one of their chemical innovators (4).

BOHNING: I don't have that article.

IDOL: It's in the July issue. It kind of puts the whole picture together. I forget which July issue it was, but I'm sure it was 1971. The history goes something like this. While I was doing this deskwork, I reported to another project leader. I was made a project leader approximately a year after I joined the group. It was decided, by consensus I suppose, that acrylic acid derivatives and glycerine precursors would be a good strategy to pursue, so I was allowed to take off in that direction. I actually pursued essentially two projects at the same time. I've always tried to do that; I think working on one project is a mistake. Now, it is possible to work on too many, but one needs an alter ego even among projects, so when one is a little slow, you can work on the other one, and then alternate back and forth. That's what I did.

Both projects were successful. We were able to oxidize acrolein in small yields, but a group at Shell Chemical—I forget the gentleman's name; I believe he worked with Charlie Adams of catalytic fame at Shell—in all honesty beat us to the draw and got to glycerine by an alternate route, using a vapor-phase Meervein-Ponndort-Veerly reduction, which you're familiar with, allyl alcohol followed by hydroxylation. We were locally successful, at any rate, in both projects.

The other one, in oxidizing acrolein to acrylic acid, I had the literature on, and found that several other companies, Carbide, Shell, and Distillers [Corporation] at that time, had been pretty well all over the act there, so the only thing they hadn't done was develop a high-yield process. A lot of the work had focused on liquid-phase processes for oxidation or esterification. Of course, those were a total disaster when dealing with something like acrolein.

Given the fact that we were a petroleum company with skills in vapor-phase heterogeneous catalysis, it was predictable that we would take the vapor-phase approach, with a lot of trepidation, though, because we were dealing with sort of a technology fog at that times. Catalytic science was nowhere near the stage it is at now. This was in the mid-1950s and ESCA [electron spectroscopy for chemical analysis] had been dreamed of, but there were no machines available. Vapor chromatography had <u>just</u> appeared on the scene.

Our first determinations of propylene conversion to various aldehydes, acrolein plus other by-products such as formaldehyde and acetaldehyde, were originally done by bubbling the reactor stream through a 2, 4-dinitrophenylhydrazine solution [laughter] and separating the solid and doing an infrared scan to determine differences in banding of the different phenyl hydrozones. Of course, when the first vapor chromatograph came along, it removed all of that and speeded up things immensely.

Several of the other senior chemists, notably Jim [James L.] Callahan, Ernie [E. C.] Milberger, Bob [Robert W.] Foreman, and Bob Grasselli, had previously done work on screening a lot of compounds in search of an agent for the selective oxidation of propylene to anything of interest. They had made acetone and a certain amount of acrolein, which was what had our attention at the time. I stepped up on the shoulders of giants, I guess you would say, to borrow an expression from the literature. From their teachings, I began work in the area of the Group VI metal oxides. A couple of the fellows, including Callahan especially, and Grasselli, had looked at the molybdenum compounds as oxidation agents for propylene to acrolein. The now-renowned bismuth-phosphorous-molybdate acrolein catalyst had been put together by Callahan and Foreman, I think it was, and shown to give rather remarkable selectivity and conversions of propylene to acrolein.

No one seems to have given any thought to acrylic acid, much less acrylonitrile. The idea was to make acrolein and see what could be done with it, so borrowing from them, I rather easily developed a series of vanadate and molybdate vapor phase catalysts, impregnated on or co-gelled with silica. We were able to convert acrolein to acrylic acid in commercially attractive yields with these catalysts. The marketing group immediately began an investigation into what the opportunities for acrylic acid acrylates were.

Acrylates were mostly made, at that time, by the Rohm & Haas technology of adding CO [carbon monoxide] and an alcohol to acetylene, which was the process of choice for a long time. It was a serious contender; we were not sure we would ever be able to equal it or top it, but we carried forward, and after a while some calculations emerged that acrylic acid via acrolein propylene was in fact economically viable, and the markets began to look interesting.

Veatch was very good in persuading his staff to think in terms of the economic consequences or impact of their work, so I would have to say that even at that time a lot of our strategic technical thinking was economically flavored, a pretty powerful combination. I brought the acrylic acid process to a place where it could be handed over to Dr. Milberger's process advancement group. Their job was bench-scale process simulation and advancement. We defined this as the first level of operation in terms of process scale and product and feed quantities, where variables could be economically assessed. Sound economics could then be developed based on yield structures obtained on large enough quantities of feed and products that you felt confident of cost projections. At that point, the question was what to do with acrylic acid, and everybody knew that the use for acrylic acid was mostly acrylates or their polymers. I then pursued further work on the vapor-phase esterification of acrylic acid over silica gel, which turned out to be much easier than I had thought. Commercially attractive yields in that area were achieved within a matter of a few months, so then we had the basis of a new acrylics petrochemical enterprise.

About that time, I began to realize—again, with a lot of encouragement and guidance from Veatch and Hughes—that I should start looking for something else to do other than acrylic acid and acrylates. Acrylonitrile was always in the background, and no one had thought of doing everything in one single reaction or reactor, so my first approach to acrylonitrile was a vapor-phase conversion of acrylic acid to acrylonitrile with ammonia over a dehydration catalyst. This work was done knowing that [Hans] Reppe in Germany, had already explored this chemistry, but not with economically significant results. My approach with that was really to see if process refinements would lead to something that would make economic sense, given low-cost acrylic acid as a starting material.

It wasn't too long after that that we came across Distillers's work that had been uncovered in my earlier researches, in which they had explored the conversion of acrolein and ammonia to acrylonitrile. I repeated some of their work and looked at alternate chemical variations of their catalysts that might be more economically advantageous, because their yields and conversions needed some improvement for commercial viability.

[END OF TAPE, SIDE 2]

IDOL: Rather quickly, also in the earlier searches, I had turned up the John Cosby work at Allied [Chemical Corporation]; he had done work on conversion of propylene—and propane too, as I recall—and isobutylene to nitriles. One can only take his hat off to salute Dr. Cosby. It is, I guess, a twist of fate that he wasn't working with the right catalytic compositions. Cosby worked with vanadium molybdate and obtained identifiable conversions, but very low yields, of acrylonitrile. It turned out, as I recall, that when he ammoxidized propylene, and only five to six percent of acrylo was obtained, but also larger amounts of HCN [hydrogen cyanide] and acetonitrile by-products, so he clipped off a methyl group. When one used isobutylene, the principle product was acrylonitrile, instead of <u>meth</u>acrylonitrile.

Following that, it was rather obvious that one should try some of our own selective catalysts that worked for acrolein and acrylic acid and other propylene conversions to that one. Screening several of those from my own and Dr. Callahan's library of catalysts, I very quickly identified two or three candidates that gave very promising acrylonitrile conversions and selectivities from propylene in one pass, and from there not too much reactor and reaction optimization was needed to get it into an <u>economically</u> interesting region. The rest is sort of history. [laughter]

So, when people ask, "Why wasn't this done before?" I guess the best answer I can give is that for some reason, the tools for putting it all together had not been available to a number of other people, and no one had been quite in the same position of having simultaneously the idea and a selection of catalysts designed for site selective oxidation of propylene that were more effective than any previously reported. We also had the advantage of gas chromatography to analyze quickly the experimental reactor results. We knew we had made a significant quantity of acrylonitrile five minutes after the experiment was done, and I remember very well, my assistant, Evy [Eva] Jonak was working in the reactor, which I designed and built partly with my own hands, with help from the mechanical shop. It was a mini fluid-bed reactor. I told her how to set the instruments and what the feed ratios were to be, and it was all set up for propylene, air, ammonia, or any other feed we selected. The fluid bed contained about 200 cc of ground catalyst, and, as I went off to a meeting with Veatch in his office, I remember telling her, “Evy, if this by any chance works, you can come up and break down the door, because we’ll really have something big.” Half an hour later, there was Evy beating on the door, [laughter] screaming, “It worked! It worked!” So I said to Frank, “Well, I guess we can take the rest of the day off.” [laughter]

BOHNING: I guess I’m not quite clear how important acrylonitrile was as a chemical/plastics run [starting] material at that time. Was it realized that here was an important material that could be used for a number of other end-products?

IDOL: That’s a very perceptive question. Yes, it was realized by a number of people, I think, in the chemical industry, but I really don’t think, at that time, any but a few—including myself—realized what its capabilities really were. The gasoline resistant properties of nitrile rubber was, of course, what led the Germans to develop it for hose before World War I. Acrylic fiber as a wool substitute had been commercialized by DuPont, Monsanto [Company], and American Cyanamid. Orlons, Acrilans, and Dynel had just become commercial, and the first ABS [acrylonitrile-butadiene-styrene copolymer] resin had just been made by Carbide, Dow [Chemical Company], and Borg Warner (and then Marbon [Chemical]). Almost simultaneously, everybody really felt that it was going to be important, but I don’t think anyone really knew <u>how</u> important.

There were a few other monomer uses, like acrylamide manufacture. As I recall, U.S. production of acrylonitrile at that time was around three hundred million pounds, so our market people went to work on it, and projected that in ten years the market might double. Actually, world capacity went from three hundred million pounds a year in 1957 to two or three <u>billion</u> pounds a year in 1965 or so, and world production of acrylonitrile now, I think, is somewhere between eight and ten billion.

I will claim a little credit for having been optimistic about where it would go. Acrylonitrile was a pretty unusual monomer; it was different from the other monomers, the acrylates, the methacrylates, and vinyls, in that it’s a pretty refractory monomer. It is capable of withstanding high temperatures, and because of the nitrile group and its electronegative effect on the double bond, the double bond in acrylonitrile is not very susceptible to oxidation. A lot of circumstances combined to predict that, particularly in a fluid-bed reactor, acrylonitrile had a chance of surviving the recirculation, and that if the reactants of propylene and ammonia could be combined to <u>form</u> the acrylonitrile, you had a high probability of getting <u>most</u> of it out of the reactor. I'll also claim a small amount of credit for having recognized that at the outset of the experiments.

BOHNING: One of the things I was struck by in an article you wrote was the very simplicity of the reactor diagram. You couldn't get it any simpler than that.

IDOL: No, you really couldn't. I remember some remarks I overheard one day, when Henry Haas was visiting Purdue, and I was lucky enough to be included at the luncheon table. Henry was a pretty direct, analytical, and decisive individual, and we later became good friends—among other things because I went to Purdue half thinking I would work for him, [laughter] but he had left the year before I got there. Henry once said, "If you're going to produce any chemical, you must do three things: you must do it the <u>simplest</u> way; you must use the most <u>available</u> raw materials; and you must do it the <u>least</u> <u>costly</u> way." I guess that's rather obvious, but I've always applied those guidelines to any process or product research I ever undertook.

BOHNING: You said you were instilled with economic thinking when you went to SOHIO. I'm assuming that at that time economics was not part of chemical engineering education; the economic considerations were not really discussed in terms of chemical engineering. Am I correct?

IDOL: Yes, I think so. My Purdue chemical engineering mentor, Norris Shreve—Benny, we called him—was quite firm about that, and we got a strong dose of that in his introductory chemical process industries course, which was a course available to either a senior or a first-year graduate student. He also had a famous quotation. "When one writes equations for chemical reactions, if it's to be industrially applied, somewhere the dollar sign must appear in the denominator." [laughter] I never forgot that.

BOHNING: The economic aspect is something that chemistry majors rarely ever see.

IDOL: Well, I don't mean to suggest that my career was guided by nothing but the dollar sign, but I think that for an <u>industrial</u> chemist, the dollar sign has to be there always. On the other hand, it can be an anchor, and I think for an industrial chemist, or even an academic chemist or scientist, you must not let your horizons be clouded by economic constraints, because that will prevent you from thinking as broadly as you should. Eventually there has to be a reconciliation, if there is to be an industrial process. Now, you can have products that are valued at millions of dollars a pound, such as interferons and things like that, but still there is an economic constraint.

BOHNING: Well, the plant in Lima went on-stream three years later.

IDOL: Right. Which I think was a new record for the industry.

BOHNING: It's amazing; in your 1960 paper you wrote, and I'm quoting now, "Simultaneous research development and design highlight the rapid commercialization of this new product" (5).

IDOL: Yes, and I'll be happy to enlarge on that. Again, the Hughes-Veatch combine, perhaps with a little bit of Idol, Callahan, and Milberger injected, made us recognize that if we did have anything really good here, it wouldn't wait. Somebody else would have done it if we hadn't, and we were lucky, I think, at that time, to have three people on the scene who deserve a lot of credit and whom I haven't mentioned yet. One was our vice president for the chemical division, Ed Morrill, who was busy running SOHIO's newly constructed ammonia and nitric acid and urea plants and businesses, while trying to "godfather" the acrylates and acrylonitrile work and finance it out of his budget. He was quite a patient person, and a chemical engineer himself and very insightful. He's passed on now, bless his soul.

Another is Ed Sann, who came from Hercules [Chemical]. I can't remember if he was from Hercules or one of the other companies in Delaware, but at any rate, he was our market research person who was very encouraging of the laboratory work as being economically and industrially significant. He had retained a New York consultant, Herman Nieuwenhaus, who apparently had the attention and the respect of the SOHIO management, all the way up to the president. He said to them, when the acrylonitrile discovery was made known to him, that if they really wanted to do anything of significance, they should focus on this acrylonitrile discovery, because it <u>alone</u> had the <u>possibility</u> of making Standard Oil of Ohio, overnight, a factor in the chemical industry as well as the petroleum industry. They apparently listened to him.

BOHNING: Very perceptive. [laughter]

IDOL: I agreed with him, of course.

BOHNING: It just so happens I have a quote here from Spitz's book on the petrochemical industry (6). "Idol's discovery helped turn SOHIO into a major force in petrochemicals and brought the company hundreds of millions of dollars in licensing fees."

IDOL: I would have to say, "accurately put." [laughter]

BOHNING: I'm just curious: why the plant in Lima? Why was that the site?

IDOL: Because we had a refinery there, and we had "across the fence" propylene and ammonia.

BOHNING: All right. You see, I'm old enough to only associate Lima with the Locomotive Works. [laughter]

IDOL: Yes, right. SOHIO had one of their large refineries there, and that's where the ammonia plant was built.

BOHNING: All right. As I said earlier, you had written this article in 1960 in *Chemical Engineering Progress* (5).

IDOL: That was one co-authored by Veatch and Milberger, I think.

BOHNING: Yes. I'm curious about the attitude of companies towards writing papers in the literature about their new processes, so I was wondering what the impetus was behind this paper.

IDOL: Well, I would have to say that given the economic opportunity that had been presented them, SOHIO management was more encouraging than I would have expected about certain publications. I think they did realize that "firstest with the mostest" was going to be very important, if they—a midget compared to the Shells, the Exxons, and the Carbides—were to get this done. Some public knowledge of who SOHIO was and what they had done was in order, so they encouraged, rather than discouraged, these kind of publications.

I think they understandably took the position that most other companies do about technology publications, which tend to reveal their secrets, and even after patents are issued. This sounds like heresy in an academic environment, but there is an understandable reason for the companies to use great caution about the public exposure that they give to their science and technology at scientific meetings. There has to be a balance. If you want to maintain image and attract people, you can't be silent, but you can give away the kitchen sink if you're not careful.

I remember an invited paper that I was asked to give. I believe it was at the Minneapolis ACS [American Chemical Society] meeting in 1969. I think Alex Oblad was getting the

Murphree Award at that time. My good friend Heinz Heineman, formerly of Mobil, was organizing a symposium and asked me to give a paper on the acrylo processes. The theme of the symposium was new processes, and Oblad, I believe, was getting the award for the Kelchlor process.

I put together what I thought was a reasonably restrictive paper on the acrylonitrile process that would present some attractive features and some good technology content, at the same time combined with a little bit of "commercial initiative." I sent it down to the patents department to have it screened. What I got back was virtually a paper with a lot of white-out on it. [laughter] So the usual tussle ensued, and I got to present something that was between where I had started and where they had finished. It was interesting. Although they're supposed to be disallowed in ACS meetings, I think I must have counted two dozen cameras in the audience, [laughter] snapping every slide I gave.

BOHNING: Well, that leads me to several questions relating to patents. One was, given that there <u>was</u> some other work in the area already, did you have any problems in terms of getting your discovery patented in short order?

IDOL: Well, not as much as I would have expected, but I really can't take too much credit for the patent process. That was carried out with great skill by two very talented patent attorneys at SOHIO. One was Leland L. Chapman, who was the person who strongly took the position that Idol was the first person to put this whole combination of things together, and, therefore, he is the sole inventor, instead of four or five co-inventors. I had some serious concern about whose name should really be on it, and I just laid the matter in his lap and said, "Whatever decision you make I will happily live with, because I've drawn on the work of several other people."

But, given that, perhaps the biggest obstacle was the prior reference to the Cosby patent, and that was simply resolved on the basis, as I understand it, that the chemical composition of the <u>catalyst</u> used in the Idol (or SOHIO) process was different. It didn't matter whether I made it, got it off a SOHIO shelf, or ordered it from a commercial supplier. The patentable concept was the <u>combination</u> of the simplest feedstocks, the simplest reactor, a single-step process, and an efficient catalyst all operating under the right reaction conditions. Secondly, since one of the tests for patentability is reduction to practice and utility, they argued that the Idol process had, on the basis of yields and conversions, process economics for above the other contenders.

BOHNING: Were there any suits that followed as a result of that?

IDOL: Yes. Of course, there was the famous Distillers suit, which all centered around whether or not acrolein was an intermediate, because if it was, they asserted, we had violated their patent. Much of my early conjecture had been on the basis that acrolein could be an intermediate—

some beautiful mechanistic work done by Art Strecker and Bob Grasselli. Jim Callahan and Ernie Milberger established the role of the famous allylic intermediate in the kinetics, which in fact reacted faster with adsorbed ammonia to form acrylonitrile than it could by converting itself first to acrolein and then to acrylonitrile. That's a classic in catalytic mechanistic work. On the basis of that work, the case was resolved out of court, but not without an awful lot of depositions and several years of hassling. [laughter]

BOHNING: I've talked to several people who tell me that that almost goes with the territory, but it's not one of their favorite things. [laughter]

IDOL: That's about right, yes, but it was a learning experience.

BOHNING: We discussed this at lunch earlier, when I cited John [E.] Franz's case (7). Were you involved in development as far as getting the whole process on-stream on a large scale?

IDOL: Yes, that's right, we sort of got sidetracked. When the process was discovered, it was quickly realized that we had to move it forward speedily. We interviewed several major construction firms, and on the basis of some past work with SOHIO, we selected the Badger company to do the work for us. Several of their people—Russ Sheely, a notable fellow, who is now passed on, bless his heart, Bill Seaver, who was a vice president at the time, and two or three others—came up with the idea of paralleling a number of the development projects. They would <u>design</u> the plant itself <u>while</u> the pilot work was still being done, with maximum interaction of all groups. We would simply <u>leap</u> into certain aspects of piloting and designing, where possible, on the premise that the money <u>earned</u>, by having the plant on-stream earlier, would more than compensate for final plant fix-it work, even if it had to be done.

Now, there had to be some limits put on this. We couldn't just be outrageously daring, but I think that strategy is responsible for the three-year elapsed time between the first laboratory-successful experiment, which was March 7, 1957, and spring 1960, when the first commercial plant was put into operation. We did operate an eighteen-inch diameter fluid-bed pilot reactor, which held, as I recall, give or take a thousand pounds of catalyst. That operated pretty much according to the predictions from the advancement scale lab work.

As far as the recovery section of the plant went, at that time, SOHIO had no one to take the responsibility for either pilot planning or doing the bench-scale work needed to prove out the Badger design for the recovery plant, so I volunteered to do that. We operated Oldershaw columns and did bench/pilot scale distillation experiments based on the perceived feedstreams that the Badger people, with some help from us, theorized would result from the reactor operation.

I worked these all up in the laboratory, using two- or three- to four-inch Oldershaw columns, and the Badger people daringly but expertly extrapolated these to six- and eight-foot diameter plant columns. We also operated a version of an absorber column and a recovery column to give a four-fold scale up factor from mine in the laboratory, but we telescoped all of that. The reactors for the plant were being designed while the pilot plant reactors were being operated, the theory being that changes in blueprints don't cost anything but paper, ink, and draftsmen's time. Changes in metal are expensive, but you can do a lot by telescoping, and that's what was done.

[END OF TAPE, SIDE 3]

IDOL: We had essentially a predictable operation, based on the development and advancement work. There was only one relatively major fix-it item in the plant, where one column had to be changed from what amounted to a knock-down column for heavies to a type of quenching tower. Incidentally, I had designed and operated my own quench tower at the bench-scale level and somehow that didn't get translated to the plant design.

There were some more troubles in the start-up of the plant, due to unexpected concentrations of by-products and some polymer formation here and there, but in general, the plant did in fact go into trouble-free operation within a year of its start-up. I think this is a pretty interesting illustration of what can be done to really shorten commercialization plants, when you're willing to take some risks. I spent a year in the plant, "feeling" columns and looking at temperature indicators, as well as sampling tops and bottoms, and doing material balances; it was an experience I wouldn't trade.

BOHNING: In terms of the catalyst, was it an in-house catalyst at that time?

IDOL: Entirely. We later licensed its production, but it was an in-house creation.

BOHNING: Was this because you had a bigger demand for this catalyst all of a sudden?

IDOL: Yes. That's another aspect of it that we should not pass. The management made what a few of us recognized was an extremely wise decision on how to commercialize, which outwardly seemed to many to go against the grain of chemical industry tradition. Early on, with the help of Morrill, Hughes, and Chapman—who was not only patent counsel, but a leading negotiator for our licensing agreements—the company decided that if we had something this good, it would be a classic mistake for SOHIO to try to use this economically advantageous technology to produce the world's supply of acrylonitrile by ourselves.

Industry will always look for sources of alternate supply. In addition, there were the existing acrylonitrile manufacturers using the existing HCN acetylene process, and it would have been, by hindsight, counter-productive for us to try to put them out of business. So the decision to license was wisely made after we had brought our first plant into operation and its commercial viability was proven. At that point, the old technologies, based on acetylene and HCN and ethylene oxide and HCN, all folded and ceased operation within five years, and the major producers, from the DuPonts and the Monsantos and the Carbides on, licensed the SOHIO technology. So, without question, SOHIO's management made the right decision, and the licensing incomes—which vastly exceeded the operating income from our own plant—proved them right.

BOHNING: Did the Lima plant continue production, though?

IDOL: I understand the Lima plant is now closed, although I'm not sure of that. When SOHIO and BP merged, a new facility at Green Lake, Texas, was put into operation, and that is now, I believe, operated by contract under Sterling Chemical. It was a six hundred million pound facility, which at that time was the largest acrylo plant that had ever been constructed.

BOHNING: Just as an aside, I have an awful time not seeing any SOHIO stations anymore, just BP, because I'm a native of Ohio. [laughter]

IDOL: Yes, I look wistfully back on that myself. SOHIO was a unique organization. And, off the record, I'm not sure if the best interest of the stockholders or the country was served by that merger. The old research center is a ghost of its former self, and it is probably closed by now, or almost.

BOHNING: I'll have to go by there, the next time I'm in Cleveland.

IDOL: I think it's still operating with a reduced staff.

BOHNING: Well, what did you do for an encore? You'd been at SOHIO only a few years, and here you were responsible for this major innovation.

IDOL: Well, that was difficult. I had a choice of continuing in the chemical and the catalyst area, which maybe was a mistake not to do. I continue to have, and always have had, a high interest in catalytic reactions, and I still do research in catalyst chemistry when I get the opportunity. We talked earlier about some things that I'm currently starting to work on.

At that time, the question was what to do with all this acrylonitrile, so we launched a polymer group to see if we could find any novel uses for acrylonitrile, outside the uses that were already commercial and where we would not have to compete with our customers. This polymer group grew rather rapidly, again under Veatch. It had a leader for a while, whose name I don't immediately remember, who was very competent but apparently interested in other areas, so he went to another section of the company. I had begun to try to become a "homegrown" polymer chemist, so with some fear I switched fields to polymer chemistry and was asked by Veatch to lead the polymer efforts at SOHIO. At that time, the barrier properties of high nitrile films were just beginning to be recognized, and SOHIO had, by the time I took the helm of polymers, developed a casting process for polyacrylonitrile [PAN] film, which was a wonderful product and probably still may find uses. It went through the Delaney amendment issue with acrylonitrile being a carcinogenic material, but that's now largely resolved, because if you want to, you can get the monomer contents of those films so low that you really don't have extractibles. As a food container, nitrile-containing resins of some formulations are acceptable, and even for beverages I believe it has now been cleared in some areas of the world.

Anyhow, I concluded that they were right about the nitrile film and barrier-type packaging material opportunities, but the fatal flaw was that you had to cast it, and that's a very expensive way to make a film. Some research had already been started on making a thermoplastic version of polyacrylonitrile. As you probably know, PAN itself, because of the high attraction between the nitrile dipoles, is not a thermoplastic. It cross-links and degrades before it melts to allow extrusion, but work done by June Duke, and three chemists, Drs. Frank Vincent, Bill Dunavant, and Ralph Isley, had shown that it is, of course, possible to make nitrile co-polymers with acrylate esters that could, with some difficulty, be fused. The question was: how much of the barrier property of acrylonitrile remained if you put enough acrylate co-monomer in it to internally plasticize it?

About that time, recalling the work of Norman Grassie who did some of the first nitrile teraphthiridene decomposition work, one of our own very bright young chemists, Dr. Larry Ball, found that there were certain critical impurities that, if removed from nitrile co-polymers, greatly improved their resistance to degradation. We were then able to develop a copolymer of methyl acrylate and acrylonitrile, which had a fine combination of barrier properties and melt pressability. It was a mediocre barrier to water, but it was a superb barrier to CO_2, oxygen, nitrogen, and some other things.

The only problem was, it was brittle. We could blow-mold it into bottles, but they were still brittle. It did respond to orientation, and later, we and Monsanto and some others found a way to stretch-blow that cured the brittleness problem, but that technology was not advanced at the time. Only Phillips' [Petroleum Company] Orbet process for oriented polypropylene containers was known at the time, and we didn't have access to it.

Again calling on the skills of the group, I directed the research towards developing a toughening agent that would impart impact resistance, so, borrowing from ABS technology, the group prepared an acrylonitrile a-butadiene rubber with enough butadiene in it that its elasticity was not harmed, but which also had the same refractive index as the base resin that we grafted onto it. Thus was born what is called *Barex®* resin, imaginatively named by the commercial development group, and this is now a commercial material for high-barrier packaging of foods, processed foods, meats, industrial products, and especially pharmaceuticals, which require oxygen and CO_2 protection.

Interestingly, we instantly recognized the opportunity in carbonated beverage bottles, so we approached some beverage companies—Coca Cola [Company], Pepsi Cola [PepsiCo, Inc.], and a few others—and Pepsi Cola was the first to jump. We asked them for a list of properties that they would like in a plastic barrier bottle, and of course the answer came back, "It should look as clear as glass, it should bounce like rubber, it must hold 75 psi internal pressure, be a barrier to CO_2," and everything like that. It turned out that *Barex®* did all of that, so the first plastic carbonated beverage bottles that were field tested in the United States were *Barex®*. That was twenty-five years ago, and here is one of those bottles.

BOHNING: It seems to be cloudy.

IDOL: It's cloudy today because the rubber stabilizers have long since been "used up" by exposure to light, but at one time, that bottle was clear as glass, and bounced off of concrete floors.

BOHNING: Is it brittle now?

IDOL: Probably. But as produced, you could slam a file drawer on it and it wouldn't crack! [laughter]

BOHNING: It says, "In New Plastic Bottles." That's interesting; they're making a point of it in their advertisement.

IDOL: Yes, that bottle is twenty-five years old, and the only thing it didn't have—and I told them it didn't—was sufficient thermal and creep resistance. If you put a filled bottle a hot car trunk in the summertime, it will expand and distort, but it does keep the material flavorful and everything else for two years.

What we did do, of course, unknowingly, was pave the way for PET [polyethylene terphthalate] bottles, because it turns out that most carbonated beverages are consumed within ten days of being bottled, so there's no shelf life problem with PET. It's not a good barrier, but it's good enough. At the time that this series of experiments was completed a market study was completed that showed that a carbonated beverage bottle, in plastic, would be a howling success, but it would have to be less expensive and it would have to have better heat resistance.

It also happened, at the same time, that the three billion pound PET capacity in the United States was only 50 percent utilized, and the PET industry was looking hungrily for something else to do with their material. [laughter] So it didn't take DuPont and the others very long to discover how to orient a PET bottle, and that's why they're where they are today.

BOHNING: So this would have been 1969; you said twenty-five years?

IDOL: Yes, that's about right. It was in the late 1960s and early 1970s. The work on nitrile resins began in about 1961, and I took over the plastics research at SOHIO in 1962. Then later I got into work on other polymers. We did work on unique synthesis of unsaturated polyesters by using olefin oxides directly in place of ethylene glycol, so you didn't have water as a by-product to deal with.

BOHNING: Before 1977, had you ever thought of leaving SOHIO?

IDOL: Not really, and I probably would still be there today, but for two things. A friend of mine had given my name, with my knowledge, to an executive recruiting firm. I had some concern about how the opportunities for upper management positions would be affected by a corporate acquisition, which in the end amounted to a takeover. So when the offer came, instead of <u>not</u> listening to it, I chose to listen.

BOHNING: One other thing, before we reach that 1977 point. You listed, starting in 1964, somebody who had an influence on your life was a Dr. Henry Gray.

IDOL: Yes. Rev. Henry David Gray was a remarkable person. He just passed away on September 3rd of this year, at the age of eighty-six. I met him in Hartford, Connecticut, where I spent a lot of time with a consulting firm that SOHIO had hired to do work on nitrile polymers. I was really raised in a believing, religious family, and that continues.

Dr. Gray was at once a <u>great</u> scholar and a <u>great</u> friend, and I was in need of a little guidance on career matters at the time; somehow we just sort of clicked. He earned my respect the instant I met him, and we became fast friends over the years. I have some interest—a lot of interest, I should say—in things religious and theological. I don't flaunt it much, but it's a matter of deep interest to me. Dr. Gray was, until his death, one of the leading congregational scholars in the world. He was invited to be the keynote speaker at the 350th Congregational Anniversary at Westminster Cathedral in England, for which he had written a sermon that his son David had to give, because Dr. Gray had fallen and broken his hip just two weeks prior to his anticipated departure. I remain almost an adopted member of their family, and I consider it an absolute miracle and privilege to have met a man like that.

BOHNING: Where was he located?

IDOL: He was Pastor of the Old South Congregational Church in Hartford, Connecticut, which is the tenth oldest church in the United States, and he was the thirteenth minister. Even among churches, that one is unusual for the tenure of its ministers; each tenure is around thirty-five years. [laughter]

BOHNING: That is unusual. [laughter]

IDOL: He is quite a guy. I could go on for hours about him.

BOHNING: Again, I'm quoting from something you wrote in 1974. "Acrylonitrile-containing polymers have grown from being a laboratory curiosity to one of the most prominent and useful types of polymers over the past forty years. The growth has occurred step-wise, as first synthetic rubber, then fiber, then molding, and sheeting, resin applications" (8).

IDOL: I would add packaging to that list. You should distinguish that from molding resins, because to most people this term denotes ABS and SAN [styrene-acrylonitrile polymer] resins, and they're a chapter of their own. ABS, I believe, is now a larger consumer of acrylonitrile than fibers. Then there is the chemical area, which is largely polyacrylamide, but that has extensive use in gelling agents, thickening agents, and drilling muds. It has become quite an interesting material.

Now acrylo monomer is the basis of a new form of polymer, the dendritic polymer or the dendrite, which is a cluster polymer; it looks sort of like a coral, if you will, which grows outward by the addition of Michael-type additions to α-ß unsaturates, which can be derived from acrylonitrile. The intermediates are then hydrogenated to the corresponding amines, which is reacted with more acrylonitrile and the polymer grows outward, rather like a sea anemone. It's a fascinating new form of polymer. Another one that is interesting is the so-called flat polymer, which has recently been described by George Whitesides of Harvard [University], sort of a "magic carpet" type, if you will!

So, polymer chemistry is always doing interesting things, much of it based on acrylonitrile.

BOHNING: But isn't it true that polymer chemistry has never really gotten the recognition it should have in chemistry departments?

IDOL: I agree with that. One has to recognize, of course, what one means by polymer. If we mean synthetic polymers, synthetic polymer methods, synthetic polymer characterization techniques, and polymer physics, I think that's probably right; it's just now coming into its own.

Polymer chemistry finally got legitimized in the 1960s, through schools like [the University of] Akron and Brooklyn Poly—now New York Polytechnic Institute—which were earlier pioneers in the field. One of the early ones was, of course, the Case Institute [Case Western Reserve University] at Cleveland.

But, if you look more broadly at polymers and quickly get to proteins and carbohydrates and other naturally occurring polymers, why, of course polymer chemistry has been around forever. I think the thing that makes it all the more significant now is that the methods for characterization of any polymer—organic or inorganic, or natural or synthetic—are beginning to get the overview treatment that they should have had. But it certainly is true that, in the early days, education in polymer chemistry was achieved primarily in the industrial and not in the academic sector. Many academicians were slow, if not reticent, to realize its importance and identity as a field of science.

BOHNING: But even today, isn't it true that a lot of polymer work is done in engineering departments?

IDOL: Oh, absolutely. Given the course that polymer science, if you will, has had to follow in the last twenty-five or thirty years, it's very understandable. The development of polymer science has been technology directed, and therefore commercially and industrially influenced. I suppose it's natural that the development of polymer science in universities would follow a sort of "retro" path in coming back through the engineering schools, who were the first ones to respond to industrial needs for fabrication and processing. It was necessary in some cases for it to leap the academic fence [laughter] between engineering and science. Now, there are some schools that manage that with a lot more skill than others. And I probably should not go further there. [laughter]

[END OF TAPE, SIDE 4]

BOHNING: Do you want to say something about your move to Ashland Chemical in 1977?

IDOL: Well, that was, I guess, a mid-career change that I was ready for. It certainly offered a combination of management and science opportunities that was not immediately open to me at SOHIO, and I took it for both career and economic reasons. It was a nice jump in remuneration and rank, as well.

The gentleman who persuaded me to come to Ashland was one of my classmates at Purdue, Jim Lewis—now resting in peace, bless his heart—who was another unique kind of industrial chemical administrator, himself a Ph.D. physical chemist. One of his requirements was that the person he hired for this position of Manager of Venture Research and Development, which translates as corporate research, was that he be both a scientist and a manager. The candidate was expected to remain a scientist, as well as a manager, so it had all the fittings that I sought.

It was pretty much a romance from the start. The position was not without its problems, as any job is. I've always in my life tried to be careful not to foist my name on somebody else's publication or patent; I've never done that. I think it's only there when it belongs. In deference to co-workers, there are probably a lot of publications where it wasn't included but should have been there.

Ashland wanted to develop a presence in the petrochemical industry, building on petrochemical monomers, just as SOHIO did. I think they were hoping for another acrylonitrile, [laughter] hoping lightning would strike twice. They had done some very nice polymer work on which a lot of their chemical company sales were based, so we developed some relevant raw material processes.

Among other things that are in my resume, you'll see, is a process for methyl methacrylate [MMA], based on propylene and CO, which was pilot planted during the economic crisis of the early 1980s. Ashland simply did not have the money to carry that project forward, or I think they would have, so it was, after some exploration, licensed to a European firm, who built a semiworks. I continue to think, at the right time, that they'll build a world-scale plant on it.

The propylene-CO based process offers between a 20 and 30 percent cost advantage for methacrylates over the time-honored cyanohydrin process. More importantly, you don't have to work with HCN as a feedstock. Anybody who has a refinery can have an MMA plant. Nobody's ever seen more than a three-hundred-million-pound-a-year MMA plant line, but with the propylene-CO process a billion pound plant is possible. If you consider a billion-pound plant, a typical styrene plant size, propylene-CO based MMA could be priced in the market probably 20 to 30 percent lower than styrene. Styrene derives from benzene at twenty-five cents a pound and ethylene at ten to twelve, and propylene and CO are roughly fourteen to fifteen and six to eight cents a pound, respectively, and I think the world could do a lot with MMA that it doesn't do at those prices.

We also developed the world's first engineering RIM [reaction injection molding] resin, which found its first use in 1986 General Motors automobiles. We developed a new type of thermoset resin, a new type of composite matrix resin, with a coefficient of linear thermal expansion five times lower than the epoxies which are now used. That is now being explored for aircraft uses by some major aircraft-manufacturing firms. Those were the sort of projects in progress at Ashland when I left the company. Shortly after that, the academic opportunity presented itself and I took it, so here I am at Rutgers [The State University of New Jersey].

BOHNING: I was not aware that there was a such a thing as a Packaging Engineering department.

IDOL: Yes—but it's a Center, not a Department.

BOHNING: Maybe we should have a brief word about this department and its origin and your role here.

IDOL: Well, to put it in perspective, the oldest and largest packaging school in the United States is at Michigan State [University], which is larger than all the rest of them combined, and they have a very fine school there. There are also fine schools at RIT [Rochester Institute of Technology], Clemson [University], Cal Poly [California Polytechnic State University], University of Wisconsin, and others. There are fourteen around the country.

The Rutgers program, which is the second oldest and the third largest, originated here in 1965 in the Food Science Department. As the science of materials began to become more important in packaging, the program moved to the Engineering College, where my predecessor recognized that the design and production of packages is really an engineering function.

If you look at the fact that nearly half of thermoplastics are used in packaging, and that some very significant portion—I keep trying to arrive at the proper number of the Gross National Product—is packaging related, packaging is a very pervasive and important subject.

There are two things I like to make note of. First, chemists used to correlate the "level" of an industrial society by how much sulfuric acid was being consumed. There is also a direct correlation between the quality of life and the amount and sophistication level of the packaging being used in the countries and regions of the world. Modern packages are engineered products made from engineered materials. Paper, glass, metals, and plastics are all very engineered materials. The forming of the package is also an engineering process.

So, yes, packaging engineering is, I think, emerging as an engineering field in the same way that aeronautical engineering emerged from the marriage of civil and mechanical engineering fifty years ago. Indeed, chemical engineering emerged from a combination of chemistry and mechanical engineering nearly one hundred years ago.

Like all the universities, Rutgers must work within limited budgets and the question is whether the small packaging tiger can compete with the larger tigers in the academic cage. [laughter] But so far the program is surviving and expanding. It's very challenging.

BOHNING: What I'd like to do now is turn to this list I sent you. We've covered a number of things on this list, but let me just pull in a few more questions here, if I may.

What does scientific innovation mean to you, from <u>your</u> experience?

IDOL: To me, "innovation," especially "scientific innovation," is characterized by borrowing techniques or tools <u>across disciplinary lines</u> that can then be adapted to solve problems or create capabilities that are not provided by the original discipline. "Invention" is not the same thing. Innovation made precede invention. But invention can also result from discovery of new knowledge <u>within</u> a given discipline that is then usefully applied and reduced to practice. Even though patents are granted only on the basis of novelty and utility, I think innovation to me connotes, in many if not most cases, a unique combination of disciplines—rubbing two technology or science areas together—that results in the development of a concept or a material or a technique that hadn't existed previously. Innovation also, to me, has again a utilitarian or an economic baseline.

BOHNING: Every time I ask this question, everyone has a different twist, because their answers are based on their experience, and everyone's experience is different. I'm intrigued by those differences.

IDOL: Am I in the mainstream? [laughter]

BOHNING: Oh, yes. It all fits very nicely, but there are no two that are alike, which is quite interesting.

IDOL: I think one thing I'd add about innovation is that innovations can have analogies in them that result from historic origins. I think sometimes you are led to innovations by looking backwards and then looking ahead, so I'm a great advocate of the study of science history.

BOHNING: I believe you used the expression "shoulders of giants" earlier. Is this what you are referring to?

IDOL: Yes, "shoulders of giants," if you will. Trying to learn from how other people have combined different areas.

BOHNING: You could also call it scientific teamwork. You have cited a number of people with whom you were involved. What has been your experience with scientific teamwork?

IDOL: Well, I think that first of all, you have to look at it in two categories. The shoulders of giants concept is one of the most valid ones that I've heard put forth in my lifetime. You don't do anything in science, without building on what others have done before you—however you want to put it. In that context, I think that when drawing on <u>that</u> background and the knowledge that others have built before you, you have to give proper credit for concepts or ideas that one may feel are totally original, but which, in fact, may not be. That process, I think, <u>usually</u> occurs in one brain, rather than many simultaneously. I think the idea of six people sitting around the table, all jumping up and shouting "Eureka!" at the same time, is a fairy tale.

On the other hand, once a concept has been created or an invention has been made, the laws of probability of its ever being put forward <u>without</u> a team effort are infinitesimal, simply because teams are, by nature, multidisciplinary, and that's how things get from the laboratory to the marketplace.

BOHNING: That sounds great.

What did you do in terms of contacts outside the company—peer relationships, but in other companies? Did you do much of that, or were you isolated?

IDOL: No, I was encouraged to do it, and I had lots of them. Many came through professional society work. The record, of course, shows that I was pretty active in the American Chemical

Society and in a number of others, especially the Industrial Research Institute. I became very active in the latter through Ashland and worked on many of their committees. I've always done a lot networking, but not always through societies; a lot of times just by reading the journals or articles, contacting people, and establishing acquaintances. I've been lucky to be in organizations that encouraged that.

BOHNING: Right. That's why I was asking that question.

What kind of changes did you see in the attitude of the senior management towards R&D support over the course of your career?

IDOL: Well, in my experience at SOHIO, it was nearly always positive. A couple of times at SOHIO, we went through the usual assessment processes by outside agencies, and these caused the predictable and understandable amount of shock and nervousness, but with the exception of <u>one</u> such experience or one such occurrence, I think the company dealt pretty wisely with the professional staff. There was one occasion when I think that they employed the wrong organization, and it was the wrong time to do it, but companies have to guard against dead wood. It's a fact of life. Dead wood, after a time, becomes ironwood, which becomes an anchor. [laughter]

I think also that in my experience with both SOHIO and Ashland, the management attitude was very largely positive. It underwent some stressful times when, because of economic circumstances, the company had to decide which operations to curtail, and usually research and advertising are the two that go first. I think those two companies were just following the pattern.

I am concerned, as a lot of people are, about the downsizing trend that's virtually sweeping across industry, and I'm also concerned—now that I can speak somewhat more securely from whatever security there is in an academic chair—that in many cases, this industrial downsizing is penny-wise and pound-foolish, particularly in technology-based corporations, where survival, much less a highly profitable operation, depends on advantageous technology. One is "eating the seed corn" to make the short-range profit picture look better to investors.

The concept that some companies seem to have taken—that we don't need to develop our own technology, whatever is needed, we can go out and buy it—is a big misconception. Purchased technology is rather like an automobile: the minute you drive it out of the showroom, it starts to depreciate, and even if you're prepared to support it, maintain it, and have it repaired, you'd better be prepared to buy a new one next year to keep up with the models of the competition.

BOHNING: Well, you've just answered my next question, [laughter] which is great, but I wanted you to repeat the analogy you gave me at lunch, with the 1950s through the 1990s. I really enjoyed that.

IDOL: Well, I can't remember just when this was; I believe it was in the early 1980s. I was asked to be the Members and Fellows Lecturer for the American Institute of Chemists, and I was asked to address economic trends across the chemical and the chemical-related industries in the timeframes that I had seen them, and make some projections, so I did this.

There's a copy of that paper somewhere, but I don't know where it is; it's probably in Ohio, but I remember using the expressions the Fabulous 1950s, the Soaring 1960s, the Sobering 1970s, the Awesome 1980s, and the 1990s—the 1990s would not be a very positively-outlooking term. Yet one must never abandon optimism. One door seldom closes but another opens.

BOHNING: I have one last question on my list, which is what did it mean to you to win the Perkin Medal?

IDOL: Well, it was a great surprise. One always hopes for something like that, but when I opened the letter and read it, at first it was a little bit unbelievable, and then I thought, "Well, I'd better make sure this is really what I think it is, and not just a mistake or a misreading on <u>my</u> part," so I timidly lifted the phone and put in a call to Ed Jefferson, never dreaming that he would answer the telephone at his own house. I said, "Dr. Jefferson, is this true?" And he said, "Who is this?" I said, "Well, it's Jim Idol," and he said, "Oh yes, congratulations." [laughter] So Ed and I became, if not close friends, rather good friends at that instant.

BOHNING: Is there anything else you'd like to add, at this point?

IDOL: I think another question that must be asked as a guideline for developing chemical technology (or new technology of any kind) is does it help people and where might it be used? What are the possibilities that it might harm people, and if so, is the cost benefit-ratio justifiable?

Well, I believe you've downloaded me very effectively. [laughter] If I think of anything, I'll be sure to pass it along.

BOHNING: If you find a copy of this talk that you mentioned, I'd appreciate it.

IDOL: Yes, I'll sure look for that.

BOHNING: I'd like to thank you for spending the afternoon with me. It was delightful. I have some very good information here.

IDOL: It's been a real pleasure, Jim.

BOHNING: Thank you.

[END OF TAPE, SIDE 5]

[END OF INTERVIEW]

NOTES

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