

CHEMICAL HERITAGE FOUNDATION

JAMES F. ROTH

Transcript of an Interview  
Conducted by

James J. Bohning

in

Sarasota, Florida

on

23 January 1995

(With Subsequent Corrections and Additions)

## ACKNOWLEDGEMENT

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THE CHEMICAL HERITAGE FOUNDATION  
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## JAMES F. ROTH

1925 Born in Rahway, New Jersey, on 7 December

### Education

1947 A.B., chemistry, University of West Virginia

1951 Ph.D., physical chemistry, University of Maryland

### Professional Experience

1951-1954 Senior Research Chemist, Franklin Institute, Philadelphia, PA  
1954-1956 Chief Chemist, Lehigh Paint and Chemicals, Inc., Allentown, PA  
1956-1959 Research Chemist, General Aniline & Film Corporation, Easton, PA  
1959-1960 Manager, Chemistry Laboratory, Franklin Institute, Philadelphia, PA  
Monsanto Company  
1960-1964 Research Specialist in Heterogeneous Catalysis  
1964-1967 Scientist  
1967-1973 Manager of Catalysis Research  
1973-1977 Director of Catalysis Research  
1977-1980 Director, Process Sciences, Corporate Research Laboratory

### Honors

1950-1951 National Institutes of Health Fellowship  
1986 Chemical Pioneer Award, American Institute of Chemists  
1988 Perkin Medal, Society of Chemical Industry (American Section)  
1991 Houdry Award, Catalysis Society  
1991 ACS Award in Industrial Chemistry

## ABSTRACT

James Roth begins this interview by discussing the origins of his interest in research and physical chemistry as well as the impacts of growing up in the Bronx, New York, attending the Bronx High School of Science, and serving in Iwo Jima at the age of nineteen. Next he examines his early intellectual strengths and proclivities and his undergraduate and graduate school work. He describes his early position with the Franklin Institute and his work there on solid propellants and photochemical smog. Then he discusses his move to General Aniline & Film Corporation, where he developed a safe process to produce synthetic rubber. He next discusses his move to Monsanto Company, where he developed heterogeneous catalyst characterization. Roth describes his work under Dr. Leo Spillane and the development of a technology that used noble metal catalysts to produce biodegradable linear olefins from linear paraffins. He also examines his discovery of a low-pressure technology for carbonylating methanol to acetic acid using a rhodium carbonyl iodide catalyst, and his work in homogeneous catalysis. In the process he expounds his views on successfully getting a plant from the pilot stage to full production stage. He touches on the patent competition between Monsanto and other companies, and airs his views on a successful patent process. He then discusses his move to Air Products and Chemicals, Inc., and his creating a world-class laboratory there. Finally, he ends the interview by reflecting on the learning curve for developing technology; the need for empowerment of chemists; and the chemical industry, its future, and the industrial parameters chemists need to achieve their full potential.

## INTERVIEWER

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

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LOCATION: Sarasota, Florida  
DATE: 23 January 1995

BOHNING: Dr. Roth, I know you were born in Rahway, New Jersey, on December 7, 1925. Could you tell me something about your father and mother and your family background?

ROTH: My father and mother were both immigrants to the U.S. My mother came here from Hungary and my father from Czechoslovakia. They came here shortly after the end of World War I.

My mother was a person of limited education. They both came here about 1919 or 1920.

My father had had some education, and he was in fact in the early stages of going to medical school in Czechoslovakia. When World War I broke out, he went into the military and served his time there. When he came back, he was not able to continue his education there because of religious persecution, and he immigrated to the United States.

BOHNING: Had he known your mother in Europe?

ROTH: No, they met here in the United States.

BOHNING: Did he settle in Rahway?

ROTH: Well, during his earliest years in the United States, he moved around, but I can't recount the details. I know he lived in Western Pennsylvania initially for a short period of time; then he moved to Rahway; and then, when I was still quite young, he moved to Yonkers, New York. I lived in Yonkers up to the age of about five, and then, from that point, we moved to the in New York. I lived in the Bronx through high school and through my first year of college, then the family moved to Western Pennsylvania, remained there, and never again returned to live in New York.



BOHNING: I started my graduate work at NYU in the Bronx. I'm wondering where in the Bronx you were living.

ROTH: Where were you?

BOHNING: At The Heights Campus.

ROTH: Okay. Well then, I was not terribly far. I lived in the west part of the Bronx that was called High Bridge. There was a bridge around 170th street, called Washington Bridge, that went from the Bronx to Manhattan. I lived one half block south of that, on a small street called Ogden Avenue.

BOHNING: Now, that I don't know.

ROTH: I lived there through most of my grade school and junior high school and high school education.

BOHNING: What was it like growing up in the Bronx at that time?

ROTH: We moved there near the early stages of the Depression, and I have a very vague recollection of going with my mother to various places to get food stamps and food, because we were getting that kind of help, at the time, in the Bronx.

It was an interesting time, because most of the schoolmates that I had were like myself, children of European immigrants, and there was very high parental guidance towards getting a good education, doing well in school, going to college, and availing oneself of the great opportunities that there were in the United States. As things evolved for me, I ended up being associated with a very bright group of people, because at that time they had in New York City something called the rapid advance classes, and I had the good fortune to enter them. In fact, my education was so accelerated that I graduated high school at the age of fifteen. I skipped a couple of classes in grade school, and skipped a year in junior high school, so that I got at least two years ahead of my time. Then I had the good fortune to be in the first class of the newly formed Bronx High School of Science.

BOHNING: Oh yes.

ROTH: If you're from the Bronx, then you know about it. I was in that first class and that was quite an experience because it was an experiment in that kind of specialized education. We had a really extraordinary faculty in the school and a hand-picked group of students. Since I graduated at fifteen, from the ages of twelve to fifteen I was surrounded by some very bright teachers and very bright classmates.

BOHNING: Had you developed an interest in science before this?

ROTH: No, not really. In fact, if I were asked why I chose to go there, I cannot attribute it to any specific prior experience or any parental guidance. In fact, there was no one in my family who had any background in the field of science, or who encouraged me in any way. It was just that I heard about the new high school, it was a new experiment in education, it sounded challenging and interesting, so I applied and got accepted.

BOHNING: Was it a competitive exam type of thing?

ROTH: I don't think there was a competitive exam per se, but I think it may have depended on my prior academic record. I'm sure they set certain minimum standards for our grade averages up to that point, before they selected the students, because they did have quite a bit more applications than they were able to handle. So that was it. In fact, I was told, and I don't know whether that was true or not, but it was kind of comforting to hear it, that there was some competition among the faculty to be able to teach at this school.

BOHNING: What did you find, having no previous interest in science? What was your first experience like when you got there?

ROTH: My most positive experiences were those in chemistry, because I had a very engaging teacher in chemistry, a man by the name of Saul Geffner. The fact that I remember his name, because I couldn't give you the name of any of the other teachers there, is some indication of the impression that he made. He just presented chemistry in a way that was always challenging.

I remember, one of the simple things that he kind of always stressed in his teaching was asking the question why. It was not just the question of learning equations and learning chemical facts, but of trying to understand the nature of chemical phenomenon. I would say that, probably, if there was any single thing that I particularly got out of my association with him, it was being

imbued with that spirit of understanding. The other thing, of course, was having science presented in a stimulating and engaging way.

BOHNING: Did you have a laboratory experience with that?

ROTH: I don't remember. I don't think so.

BOHNING: Okay. Well, the school was brand new.

ROTH: Well, the school actually was housed in an old, retired building in the Bronx, used formerly as a branch of DeWitt Clinton high school.

BOHNING: Oh yes, yes.

ROTH: We inherited an old building that was abandoned, and it was, in fact, probably not terribly far from the NYU campus. I think I would get on a streetcar that kind of swung by the NYU campus and then headed east. I do not know the address, although I probably have it upstairs somewhere. I have a yearbook from the Bronx High School of Science.

BOHNING: Being a high school of science, did that mean you concentrated on physics, math, chemistry and biology? Did you get anything else outside of the sciences?

ROTH: Well, yes. As a matter of fact, I was extremely interested in English and got involved on the editorial staff of the school newspaper. Probably, during those years, I spent more time being involved in the school newspaper than in some of the other extracurricular activities. By the time I graduated from the Bronx High School of Science, I don't think there was anything in my accomplishments or attitude that would have suggested that I was going to pursue and enjoy a fruitful career in chemistry. In fact, when I graduated, I tried to discuss with my father—who had absolutely no background or knowledge of the world of science—what I should do career-wise.

Well, I was going to try to get into City College, because I really couldn't afford to go anywhere else. But I had some choices. He encouraged me to enter the business school of City College, so I did that. I attended the business school of City College for one year.

BOHNING: This is when you were fifteen?

ROTH: Yes.

BOHNING: Okay.

ROTH: I really was extremely unhappy there, and in contrast to my prior academic record at the Bronx High School of Science, I barely got through. So I decided that, at the end of that year, I would go back to the pursuit of a career in science and specifically chemistry. That was coincident with the time when my family moved to Western Pennsylvania.

BOHNING: Had you had any more interaction with Geffner after your class?

ROTH: None, ever.

BOHNING: Okay. So that decision to pursue chemistry was strictly yours?

ROTH: Yes.

BOHNING: Okay.

ROTH: My family moved to Western Pennsylvania, and I entered the University of West Virginia as an aspiring graduate in chemistry.

BOHNING: Okay.

ROTH: I entered the chemistry curriculum.

BOHNING: Why West Virginia, as opposed to Pitt [The University of Pittsburgh] or Duquesne [University]?

ROTH: Well, at that particular time, Pitt was a private institution. There was always a financial problem, and the closest land grant college, which is the one of lowest cost, was the University of West Virginia, which was twenty-six miles from my home.

BOHNING: Okay.

ROTH: At the time, because I was out of state, the cost was forty dollars a semester. Had I been in state, it would have been twenty dollars a semester, so my choice there was dictated largely by financial issues.

BOHNING: What did you do your first year at West Virginia, in terms of being a chemistry major? Did it cement your choice?

ROTH: Yes, it did. I seemed to be reasonably happy there, and did fairly well academically at the University of West Virginia. The caliber of students and competition was not anywhere near as strong as it had been in City College or at the Bronx High School of Science. In fact, it was kind of on the other side of the spectrum. Academically, I became sort of a superstar at West Virginia.

BOHNING: So you were sixteen when you entered?

ROTH: Yes.

BOHNING: Okay. Did you feel that put you at a disadvantage, being several years younger than the traditional freshman?

ROTH: Socially, yes. [laughter]

BOHNING: But, academically?

ROTH: Academically, I think I held my own reasonably well.

BOHNING: What was the chemistry department like there, at that time? That would have been 1941?

ROTH: I graduated in high school 1941, so that would have been 1942.

BOHNING: The war had started before you went there?

ROTH: Yes. I was sixteen when the war started.

BOHNING: So you were still too young for the draft at that point?

ROTH: Yes. But at the end of one year at West Virginia I decided to enlist in the Navy. At age seventeen, I entered something called the Navy V12 Program, which was an officer training program. I was accepted for that and spent two academic semesters at the University of Pennsylvania in Philadelphia. At the end of that time, that was all that they were going to support me for, I was then sent to midshipman's school at Columbia University, at the age of eighteen.

BOHNING: Right in the middle of the war, this was about mid-1943?

ROTH: In the middle of the war. I got my commission as an ensign and then was assigned to the amphibious forces. I was in the Navy a total of three years.

BOHNING: Where did you serve after that, in the Navy?

ROTH: Well, after I got my commission, I was assigned to the amphibious forces, and I served in the Pacific.

BOHNING: Okay.

ROTH: I was involved in the battle of Iwo Jima and a number of others that came along. I became a navigator aboard the ship and was a navigator at the age of nineteen during the engagement at Iwo Jima.

BOHNING: That was probably a more maturing experience than you could have asked for at that age. It made you grow up in a hurry, I'm sure.

ROTH: Yes. Well, we had some exciting times there. The day after the invasion at Iwo Jima, we were carrying twenty-five medium Sherman tanks of the Third Marine Tank Battalion. They were having a terrible time aboard the island. The battle was not going well, so we were getting ready to provide our tanks to the battle, but we got hit by a Kamikaze plane with a five-hundred pound bomb.

Unfortunately, I was running the ship at the time. We had had an accident aboard the ship, one of the officers had accidentally killed himself that afternoon, and all the older officers were having a burial at sea ceremony when the attack came. We didn't think anybody thought we could survive that, but we did. Then we hung around, and about two days later they had desperate need for our tanks. We had taken on so much water that we couldn't beach. They had to get an armada of tugs to push us on to the beach, [laughter] to get those Marines and their tanks off of the ship, which they did. I think the Marine group made a contribution to the successful ending of that battle.

BOHNING: If I could back up for a moment, you said you had one year at West Virginia, then one year at Penn. Did you take chemistry classes at Penn?

ROTH: Yes. In fact, it was kind of curious, because most of the people who were in the V12 Unit at Penn were people who were there because of the Wharton Business School. That was the driving force for their attendance then. Somehow or other, I had requested and gotten assignment to Penn, and I was about the only one in the V12 group who pursued the curriculum in chemistry. I took several courses there. Then, after the war was over with, I returned home and finished my fourth and final year at the University of West Virginia.

BOHNING: Oh?

ROTH: I had one year at City College.

BOHNING: Oh, that's right.

ROTH: One at West Virginia. Two semesters at Penn, which is really the equivalent of a year, and then a final year at West Virginia. As a matter of fact I was kind of puzzled, because when I finished at the University of West Virginia, I did so rather suddenly. I was expecting to have to go another semester in order to fulfill the requirements for a B.S. degree, because of the time I lost at business college, at City College. I then learned that if I was satisfied with a B.A. degree in chemistry I could actually finish at the end of that fourth year, and I elected to do that. When I did that, I was totally unprepared as to what to do with my life. I knew absolutely nothing about the nature and existence of graduate school, so I had a meeting with my professor in physical chemistry, who I admired greatly.

BOHNING: Do you remember his name? I don't mean to put you on the spot.

ROTH: No, I can see his face; I think his last name was Collette, but I'm not sure.

BOHNING: Okay.

ROTH: He was a professor of physical chemistry at the University of West Virginia. Anyhow, he explained to me what graduate school was like, what you did at graduate school and so forth, and he also mentioned to me a few possibilities. At that time, the employment situation was quite bad. I was entitled under the GI bill to at least four years of college, so I made applications to three or four universities with no guidance from anybody. The first one that accepted me was the University of Maryland. They offered me an assistantship and a package that would permit me to go there at essentially no cost; all my expenses, between the assistantship and the GI bill, would be taken care of.

I went expecting to go there for a year or so to get a master's degree, but once I got there I learned something of the nature of research and started doing research. I knew within a week after I was involved in research that I was not going to stop with a master's degree; I was going to go on for a Ph.D. degree. I just kind of stumbled my way to that pathway, but the lure of doing original research was very exciting and appealing.

BOHNING: What was it about physical chemistry that attracted you?

ROTH: You'd think I'd be able to answer that question, but I really can't. At an early stage in graduate school I had to go and interview several faculty members. They represented a cross-section of analytical chemistry, organic chemistry, and physical chemistry, and for some reason or other, the area of kinetics and reaction mechanisms sounded more appealing. I've never thought about the question until you presented it here, but it might have been this instilling in me



the question of finding out why, why reactions occurred or happened as they did, the Geffner influence. Somehow or other, kinetics and reaction mechanisms may have had a linkage to that. I'm just speculating here.

[END OF TAPE, SIDE 1]

BOHNING: My next question then is how was your math background? Had you picked up enough mathematics along the way as an undergraduate?

ROTH: Yes, I had taken calculus as part of the requirements for getting the B.A. degree in physical chemistry. I had courses in thermodynamics and in electrochemistry, which required a background in calculus. I did adequately in mathematics, but I would not characterize it as one of my stronger subjects.

BOHNING: You then worked with William J. Svirbely.

ROTH: Bill Svirbely was my advisor, and that was kind of an interesting experience. He had selected a very narrow area of kinetics as his interest and research area. What he was trying to determine was the influence of dielectric constants of liquid reaction media on various types of reactions. For example, if they were reactions between ions, like or unlike ions, or between an ion and a neutral molecule, the theory predicted certain effects, which would be quantified, on the rates of those reactions. That was the area that all of his students had worked in.

When I got into it I studied the cyanohydrin reaction in which it became clear that the rates of the reaction were strongly influenced by the acidity and basicity. I had some early indications that this might be an example of generalized acid-base catalysis. This was an area that he had never gotten into himself, but since I was beginning to make some progress in the area, I ended up pursuing it and getting my thesis in that area. In fact, I never investigated the influence of dielectric constants, but rather pursued the area of generalized acid-base catalysis for reactions in aqueous solutions. Subsequently, after I left, he then pursued that area himself.  
[laughter]

BOHNING: Yes, because your paper based on a thesis published in 1953 was number 1 in those series; the one that you wrote with him in JACS was paper number 1 (1). So, I didn't look at the others because your name wasn't on them.

ROTH: No, no it wasn't. I never did follow up, but I know that he had at least one more student who pursued other aspects of that. It was kind of an interesting experience because it got me into the world of mechanistic studies and chemical reactions, but in addition to that, it aroused an interest on my part in what I'm going to call asymmetric catalysis. One of the examples of the references was a case in which someone had used optically active quinine or quinidine, and had produced optically active cyanohydrins. I was absolutely fascinated by that, because it had some mechanistic implications, but it took many, many years before I got around to trying to pursue that.

BOHNING: You did get into some asymmetric synthesis work much later on.

ROTH: Not really directly.

BOHNING: I was thinking of L-DoPA.

ROTH: Yes, yes. But the L-DoPA work was done by a man by the name of Bill Knowles, and Bill got into that work because of work that I and my colleagues had started in homogeneous catalysis with rhodium complexes. If time permits we may get into just how that happened, because I think that has now evolved into one of the major research themes in organic chemistry today, throughout the world.

BOHNING: Well, within a week after being in Maryland, you knew this is what you wanted? Your eyes were open to research. What had you thought about, in terms of life after the Ph.D.? Was academic work considered, or were you looking at an industrial career from the beginning?

ROTH: I was looking at a government or industrial position. I guess I never considered an academic career very much, because I never received any encouragement or guidance in that direction.

BOHNING: You ended up at the Franklin Institute. How did you arrive there from Maryland?

ROTH: Well, at that time, when I graduated in 1951, jobs were very scarce, and there were a fair number of my fellow graduates at Maryland who did not even get a job, initially. I think I ended up with possibly two job opportunities. There was that one at the Franklin Institute, and there may have been one at a government laboratory, but it was one of the few specific opportunities that I had.

BOHNING: What was the Franklin Institute like in 1951? You did work in photochemistry, combustion chemistry, and there was one paper that was published, or at least you gave it at a symposium, on solid propellants (2). I guess the general question is, what was it like at the Franklin Institute, and then what were you assigned to work on?

ROTH: Well, the Franklin Institute was a place that performed contract research almost entirely. When I got there, I joined a very small chemistry department. The Franklin Institute laboratories itself was quite small, and it always seemed to be barely surviving.

About the time I arrived, they received an invitation to propose some research. This was in the Korean War, and they were experiencing a very serious practical problem. Mortars, which were being used in the exceptionally cold climate of parts of Korea, would blow up and detonate when they were fired, injuring or killing the people who were operating the mortars. There had been almost no prior record of that happening, so they wanted to learn more about this. In fact, it was at that time that my path crossed that of Stuart [W.] Churchill. At the time, he was at the University of Michigan, and he must have been a younger person like myself. [laughter] He had a contract pursuing a certain path. I learned about this, and I decided to submit a proposal. I was given somebody who was more mechanically inclined than I was, and we designed a high-pressure experimental apparatus, in which we were going to attempt to simulate the ignition stages of solid propellants. We studied the kinetics and mechanism of that and thought that we had developed an understanding of what was going on. That was my first research experience at the Franklin Institute, doing a study of the kinetics and mechanism of the ignition of solid propellants.

We had a contract that went on for maybe three years on this project. We found that there is a sequence of very complex solid state/liquid-surface and gas phase reactions that go on when the solid propellant gets ignited. What can happen when the propellant is initially very cold is that it goes through a sequence of reactions leading to a build up of what are called fizz gases, products of partial combustion that do not smoothly proceed through the normal progression of combustion reactions. We get an accumulation of partially combusted gases. Then when the fizz gases are ignited, instead of proceeding in a normal combustion sequence, we get a denotation. That's what was causing detonation of the field mortars in Korea.

Now, we never did work on what to do about it, but at least we had achieved some understanding. About that time, we had a chance to bid on another job. The American Petroleum Institute had become very concerned about the smog problem in California. At that particular time, there were severe smog problems, and consideration was being given to shutting down parts, if not major parts, of the refinery industry in the Los Angeles basin area. Professor Haagen-Smit, who got a lot of well deserved recognition, had established that it was the photochemical oxidation of hydrocarbons that produces ozone, which caused crop damage, eye

irritation, deterioration of rubber tires, and so forth. We were asked to try to augment his work to gain a better understanding of the atmospheric photochemistry involved in smog formation.

So at that time, I collaborated with a couple of people around, including some physicists there, and eventually came up with the idea that a good way of studying these reactions was by something that we eventually called long-path infrared spectroscopy. What we would do is design and build a long-path chamber, that had reflecting mirrors in it, and then put in synthetic mixtures of hydrocarbons and simulated air with nitrogen oxides, which were the photo-initiator. The long path chamber would be irradiated by banks of lamps that had a spectral distribution similar to that of sunlight to create a simulation of atmospheric photochemistry. And so we sat down and put this proposal together, and, voila!, we got accepted.

Then I decided to pursue money rather than science—to take a flyer and go into business on my own, subsidized by some family members. I decided to start up a paint and adhesives business in Allentown, Pennsylvania. So I left the Franklin Institute, right after we had gotten the acceptance of our smog research proposal. I was in the paint business venture for about two to three years before I finally realized that I was not going to make my fortune in that venture, and that it was really a rather foolish thing for me to do. At that point, I decided to terminate that venture and look for another job. The closest job that I could find, living in Allentown, Pennsylvania, was a position in the central research labs at GAF located in Easton, Pennsylvania.

BOHNING: Okay.

ROTH: Then I got started there.

BOHNING: So the paint company was really your idea. Why paint?

ROTH: When I was living in Philadelphia, I had met someone, a neighbor, who claimed to have some fantastic knowledge of some very good paints and adhesives that could give us a pathway for starting an attractive business. Being young and innocent, I believed him. [laughter] It turned out to be somewhat of a hoax; it was grossly overstated. After about a year of learning about various aspects of the paint industry, I realized what I had gotten into, so I terminated my relationship with this other man.

It was a flyer, a business flyer, but in the course of doing that, I did get some interesting business experience. Since it was a very small operation, I did everything. I learned how to advertise products, how to create promotional literature, and how to deal and interact with commercial and business people. Anyhow, that was the nature of that two- to three-year diversion.

BOHNING: Then from there you went to GAF?

ROTH: I was at GAF for a little over two years.

BOHNING: You have described your experiences there in somewhat graphic detail.

ROTH: Where did you catch up with that? [laughter] Maybe in that paper I gave on receiving the first ACS Award in industrial chemistry, "Some Adventures in Industrial Chemistry" (3). That was the first opportunity I had to describe that extremely interesting experience at GAF.

BOHNING: They had reached the point of using acetylides as catalysts, which as you pointed out are highly explosive materials. They used acetylides until the plant had several disasters and sent it back for development.

ROTH: This technology had been developed during World War II by Reppe in Germany.

BOHNING: Okay.

ROTH: The Germans were looking for a totally synthetic route to rubber, because they could not get rubber from natural sources. So the brilliant chemist, Reppe, came up with the following scheme.

They had lots of calcium carbide from which they made acetylene. Then acetylene was reacted with formaldehyde to make 1,4-butanediol, which was then hydrogenated to 1,4-butanediol. This was dehydrated to butadiene. Once they had butadiene, they made synthetic rubber, and they actually practiced this complex scheme of reactions. That's how they got their rubber in World War II.

When the war was over, GAF, which had been seized as an alien property by the U.S. government, was used as a vehicle for transferring some of the captured German technology. There was a whole body of literature called the PB reports, which were reports of documents captured from the Germans. There is a fascinating cloak and dagger story about how the U.S. obtained the documents for the butanediol technology. Then GAF attempted to use this information, acquired from Germany, to develop and practice that technology in the United States. It uses a copper acetylide catalyst.

GAF had been researching the technology for well over five years, and they were at the point of starting up their commercial plant in Calvert City, Kentucky about the time I arrived at GAF. When the plant was started, it blew up. Subsequently they tried it a second time, and it blew up again when valves were turned. What was happening was that the copper acetylide, which was dispersed on a siliceous catalyst support, would get leached out and then redeposited on valve surfaces as pure unsupported copper acetylide.

In the pure unsupported state it was really a percussion material. You could just touch it slightly and it would detonate, but in the supported state, it was safe to use. You could take precautions, which the Germans had developed, for making it usable. Eventually, after experiencing a number of accidents and detonations themselves, the Germans had learned how to use it. The fact that the same technology has been used now for forty-plus years, even in the United States, shows that although it's hazardous, if you know what you're doing, you can control it and operate it safely.

When I arrived, after the second explosion, the manufacturing department decided to return the process to R&D. They said, "When you've solved the explosion problem, we'll consider reactivating the plant." As you might imagine, that was the biggest investment that GAF Corporation had made at that particular time, so this was a critical problem. I had the good fortune to be assigned to the problem as my initial assignment.

BOHNING: You described at least one explosion you had.

ROTH: I was trying to learn something about the nature of these deposits. We were able to find some conditions under which we could leach the copper acetylide out of the catalyst and then have it—a solution containing the dissolved copper—contact acetylene and form copper acetylide in an unsupported state. That eventually became the thesis of what went on. But what we found was that there were some conditions in which you could form a copper acetylide that was very sensitive, and a form that was less sensitive.

I wanted to research the problem, using advanced techniques of solid characterization and catalyst characterization, more advanced than any they had ever used at GAF, so I had to collect samples of these variable forms of copper acetylide deposits. One of them was of the sensitive type, but I guess we didn't realize it. My technician was carrying it down to the analytical department under water. If we touched this stuff with a glass rod under water, it would detonate, but we always dealt with very small quantities.

Then they took the material that they had very carefully put on a watch glass and they transferred it, in order to weigh it. When they transferred it, it detonated with a sound that was heard throughout a four-story building.

BOHNING: Was anyone injured?

ROTH: No. Fortunately, the people involved were wearing face masks, and nobody was injured.

I had a couple of sleepless nights while I was doing the research. I'd be at home thinking that so and so is going to be taking out samples. Do they realize that those might be the dangerous materials? I'd call people up in the middle of the night, just to double check and make sure they were taking the adequate precautions.

The GAF pilot plant had a one-inch tubular reactor, and when they decided to build the commercial plant they did not want to deviate at all from the pilot plant, so they built a commercial scale reactor having hundreds, if not thousands, of these one-inch reactors. Sometimes, unexpectedly one would get acetylene starvation in one of those tubes. Acetylene starvation would then cause leaching of the copper; the leached copper would come at the bottom of the tube, hit unreacted acetylene, and voila, we've got this unsupported explosive copper acetylide.

Sometimes, doing mechanistic studies doesn't always tell you what to do.

[END OF TAPE, SIDE 2]

ROTH: Am I giving you too much detail?

BOHNING: No, no absolutely. It's just perfect, it's really good.

ROTH: With the realization that this was what was happening, the engineers down at Calvert City took steps to design and install what was needed to insure that no condition of acetylene starvation occurred in any of these tubes, and to set up monitoring. Once that was done, the pilot plant was restarted, and to the best of my knowledge, has—oh God, [laughter] I can't believe it—in forty years continued to operate successfully.

But that was an exciting experience, and it gave me a real introduction to very applied catalytic science and technology, from a materials and process point of view.

One of the very interesting things in that GAF experience was that I found myself, when I joined the company, for the first time in my career, assigned to a department populated almost entirely by chemical engineers. I was virtually the only chemist in that department. I got to learn

more about the world of chemical engineering—the way engineers think about science and technology—than I ever had before, or that I would ever know in the future.

Ultimately, I think this had an important impact. Later on, when I went to Monsanto and was able to set up a corporate catalysis department, I was determined that it would not have the traditional organizational structure, which is when you have a bunch of chemists doing the exploratory work, in test tubes and flasks, and small equipment; and then when you get a lead, you pass it on to the engineers. They're in a different organization; they scale it up and they develop it.

I had the department structured so that there were research-minded chemical engineers working with the chemists at the very earliest exploratory stage. It was that organizational feature which led to one of my people, a chemical engineer, being the inventor of the process used to produce Roundup, Monsanto's highly successful herbicide.

BOHNING: During the early days at Dow Chemical, the physics lab had a similar arrangement. Within one group, they had a chemist, a chemical engineer, mechanical engineer, a physicist—this big mix of people—who all interacted together in a project. That's very interesting.

You said that there was one lesson you learned from that experience, and that was, “to elucidate any and all possible side reactions, their causes and consequences” (3).

ROTH: You ask yourself the question, “Well, gee, what did we learn from this?” Did anybody in all of the work that was done say, “What are the possible side reactions? What happens if you have an excess of one or the other, or one of your reactants gets starved?” No. But later on, when I got to be involved in the development of a very new process for Monsanto, the process for making acetic acid, where we were dealing with potentially hazardous materials like carbon monoxide, we did just that.

I had a very brilliant chemist, by the name of Denis Forster, do some research on the chemistry of side reactions. What would happen if you had too much of this or too little of this? In fact, he won the Ipatieff Prize of the ACS, as a result of his fundamental studies. He did such a thorough job of studying the conditions, we knew that you must never have a situation of CO starvation in the reactor, because if you did, you would precipitate rhodium metal or rhodium iodide all over the place, and you would have a disaster. With the price of rhodium, in that process, you had to have 99.9 percent recovery of rhodium at all stages.

As I have moved on to see research, like at Air Products—research that I was not in control of—others have failed to do that. In some of the consulting I've done for other companies, where they've called me in because they're having major problems at the multi-million dollar pilot plant stage, they have not done that. If you're going to do a first-class



research job on a new chemical process, you really have to address all the what ifs—and not just in a token way, but in a thorough way.

In the case of the acetic acid process, that was done so well, both by Forster and the process people at Texas City, that when that plant started up, it almost instantly produced design capacity—no problems!

BOHNING: Vladimir Haensel, who developed the platforming process for UOP, told me that once they had the process underway, they knew the right catalyst and so on, and he was not involved until they started the plant up (4). He was invited to the plant start up. I asked him if he ever felt apprehension anywhere along the way, and he said, “Not until we got to the day they started the plant up, and after ten minutes they shut it down.” He said, “That’s when I got scared, because I’ve been responsible for getting them to this point, and all of the sudden, they’re shutting everything down.” It turned out it was a problem that could be solved relatively easily, but he said that was probably his most worrisome moment, when they reached the production stage and it didn’t work right away.

Are you saying that the lesson you learned at GAF allowed you to avoid that?

ROTH: Yes. I think it instilled in me—and it could be used as a model for other people’s thinking—that if you’re developing a new process, you not only have to research the desired reaction, but think about and research the possibilities of undesired side reactions, and what could happen if certain upsets occurred in the process. The failure to do that has caused a number of disasters. For example, Oxirane’s attempted development of their ethylene glycol process. If you sit down and talk with the gentleman who invented that process, and talk to him about what he did at the research stage and what they did at the process development stage—which he didn’t participate in very much—you see that it’s another example of that sort of thing happening. But I thought I had gathered that particular lesson from that experience.

BOHNING: After three years at GAF, you ended up back at the Franklin Institute.

ROTH: Yes. As a result of the successful resolution of the butynediol process problem, GAF gave me an amazing award. They gave me a cash award of a thousand dollars, which, I was told at the time, was the largest award ever given for a research accomplishment in the history of that company. About a month later, I got a phone call from the director of the laboratories at Franklin Institute, telling me that my former boss, who was the manager of the chemistry division, had just left to go into business for himself, and they were offering me his position at a forty-percent increase in pay, with a private secretary and my own private parking space. [laughter] I thought about all of this, and it sounded pretty attractive. [laughter]

At the time, GAF was beset by a never-ending legal battle between the government ownership of GAF and certain other organizations. There was an international organization called Interhandel, which was fighting the U.S. government over the ownership of GAF, and as a result of that further progress in that company looked very, very grim. So I was receptive and took the job. I got back, and then after one week, I thought, “Oh my God, I completely forgot about all the unpleasant things here!”

This is really a contract research organization, and I realized that as manager, I would be spending very little time doing research per se—which is what I really wanted to do—but instead would become a salesman trying to get contracts. While I was willing to contribute to the process, I was not willing to let that be my main occupation. After a month, I decided to give this thing six months. “If I still feel the way I do now, I’ve made a terrible mistake and I’ve got to get out of this.” That’s exactly what happened. At that point, I decided that I was no longer going to stay in Allentown, Pennsylvania, where my family was very comfortable, but I was going to look for the best job I could find anywhere in the United States.

I started a search and ended up with Monsanto, which did not have a dedicated effort in catalysis research. I gave up my job as a manager of a twenty-person department, with all these perks, and became a bench chemist [laughter] at Monsanto.

BOHNING: Was there a cut in pay?

ROTH: Yes, as a matter of fact, there was a slight cut in pay.

BOHNING: That took some courage to do that then.

ROTH: Yes, that’s true. Had I been a little wiser in the ways of the world, I probably could have negotiated [laughter] for a better deal, but at the time that was the offer, and I took it and went to Monsanto.

BOHNING: I have a couple papers here that you published during that one year at Franklin Institute—one on solid propellants and another one on oxidation-reduction catalysis—primarily looking at combustion and pollution problems.

ROTH: Yes, but let me elaborate on that. Do you have the title there?

BOHNING: It was “Oxidation Reduction Catalysis,” (5) with Robert Doerr.

ROTH: Once I got involved in that project on trying to elucidate the nature of photochemical smog, I also became interested in the possibility of controlling emissions from automobiles. In particular, I was interested in the possibilities of eliminating NO<sub>x</sub> emissions from automobiles by catalytic reduction—with reductants such as carbon monoxide and hydrocarbons—and therefore I had done some preliminary work on that at the Franklin Institute.

BOHNING: This shows up later on with Monsanto as well.

ROTH: Yes. In fact, that may have been one of the things that attracted me to Monsanto, my background in this area, because my initial assignment at Monsanto was to work on auto exhaust catalysts.

BOHNING: Oh, okay. That was the question I wanted to ask you. You started there in 1960, with the title of Research Specialist in Heterogeneous Catalysis.

ROTH: Right.

BOHNING: You were primarily assigned to work on auto emissions. Who did you report to, and what kind of group was it?

ROTH: There was sort of a chemical process group, and the man who hired me and who was my boss there was a man by the name of Jim Fair.

BOHNING: I've done an interview with Jim Fair (6). He's at the University of Texas.

ROTH: That's right.

Jim hired me and was my boss. A year or two after I was there, he was moved from research into the chemical engineering unit of the company. They had set up a massive thousand-person chemical engineering unit at Monsanto, and he became the director of fundamental chemical engineering research. He was quite an expert in distillation theory and practice, but this project was of interest to the company, so I and a technician—I had a technician working for me in this department—started this research.

In the process of doing that, I played a major role in developing capabilities at Monsanto in the characterization of heterogeneous catalysts. I was probably one of the first users of a number of techniques and one of the first people to do in-situ X-ray diffraction studies of catalysts. I also did electron-probe microanalysis studies of catalysts, and developed these techniques. As it later turned out, these techniques and capabilities became generally available, and their use diffused throughout the corporation.

BOHNING: There's a series of four papers on copper oxide and alumina in the late sixties (7).

ROTH: Right.

BOHNING: In which you used ESR [electron paramagnetic resonance], ESCA [X-ray photoelectron spectroscopy], and so on.

ROTH: Right.

BOHNING: You were doing this when you first started there, but it didn't show up in the literature until much later.

ROTH: I probably did that during my first three or four years. A lot of the work I did when Wolberg and several other people came along I did on a collaborative basis later on. I would say most of that work was done probably during the first five or six years at Monsanto, irrespective of when it would actually get published.

BOHNING: Something else I wanted to also ask about in a general sense, and that is the company attitude towards publication of information of this kind. Some companies have a don't-publish-anything rule. What was Monsanto's attitude towards that?

ROTH: I found that Monsanto's attitude was sort of a mixed bag. Each business unit, each major profit center or division, passed on any application I might make for publication. This was somewhat frustrating, because the work that I did on the development of a process for making linear olefins for biodegradable detergents—which was my first major commercial success—was in a particular division that was extremely hard nosed and would not allow me to publish. Much of the work I did in that has never been published.

On the other hand, in the work I did in acetic acid, I dealt with some more liberal people. I would have to say that there was no singular company policy which dictated the situation everywhere; it varied. If we have time to discuss this when we get into the question of innovation, I would say that the attitudes and the culture regarding innovation varied in a major way, between one unit and another.

BOHNING: Okay.

ROTH: I think some of those differences appear in other companies as well.

BOHNING: It was around the time that you started there that the industry knew they were going to switch from branch chain to linear groups in detergents for pollution problems—to get better biodegradable material. How soon did you get involved in producing the olefins that were required?

ROTH: I came to Monsanto in 1960, and I must have worked on the auto exhaust program until about 1963. Then Monsanto formed a corporate task force, and appointed a man by the name of Dr. Leo Spillane from the Hydrocarbons Division as the czar, to develop a technology for producing linear olefins from linear paraffins. They were going to use the linear olefins to alkylate benzene, and they knew that they would produce detergent products with acceptable biodegradation properties.

Somehow or other, Leo had become acquainted with my work in catalysis, on the auto exhaust catalyst, and he succeeded in getting me reassigned to form a group that reported to him to develop catalysts for producing linear olefins. That then became my major project, and I had a group of about three professionals and three technicians—that's the first time I ever had a group of any kind there—and we worked on this.

Initially, we started to work on molybdena alumina catalysts, because some of them had been studied in a preliminary way in the Hydrocarbons Division. They had some positive leads and they wanted me to follow them up, and as it turns out, molybdena alumina catalysts for reforming and dehydrogenation are well known in the literature.

I must have worked on them for about a year, I'm going to say a year to a year-and-a-quarter, and early on we learned enough to convince me that these catalysts were never going to make it. I then asked them for permission to explore some other types of catalysts, specifically noble metal catalysts. They wouldn't allow me to do that.

I did that during the first year, and finally at the end of about a year or a year-and-a-quarter, the project moved along. There was certain technical goals in terms of conversion and

selectivity which were set, and which were necessary to achieve the requisite economics, and a decision was made to go to the Board to build the plant. The performance and economics, which were presented to the Board at that time, were not actually anything that had been achieved. They were just projected that they would be achieved, and I believed they weren't [laughter] achievable with the moly alumina type of catalyst.

It was at that point that I said, "We have a bad situation here, we've got to break out of this box and start in some new direction." They wouldn't let me do it, but then finally, when they got the approval from the Board to go ahead with the plan, I was unleashed. I immediately started working with platinum-based catalysts. In fact, when I tell people this they don't believe me, but from the day I performed the first experiment with a platinum-based catalyst to the day of the start up of a 150-million pound per year plant producing linear olefins was a total of fourteen months.

BOHNING: What was it that gave you the lead to move to platinum, or to noble metals in general?

ROTH: It was a question of understanding the nature of acid-catalyzed hydrocarbon reactions. When we looked at the performance of the molybdenum catalyst, it looked like the side reactions, which were killing our selectivity, were skeletal isomerization, cracking into smaller fragments, and high formation of aromatics—all of which were caused by acidity. We didn't want any of these things, so we decided that what was needed was a low acidity catalyst. But when we started working the platinum catalyst, we quickly came to the realization that—as in the case of reforming—when using a platinum catalyst you have got to get the maximum value out of every single platinum atom that you put in there. So we did a lot of work on controlling the dispersion of the platinum throughout the pellet, finding out if it was better to have all of it on the surface—as is in the case of reforming catalyst—or was it better to have it spread out, and how did we control the acidity to control side reactions. Also, what do we do when the catalyst cokes up? We had to develop a regeneration procedure that would not lead to significant deactivation of the catalyst. We went through all of this. Also, to repress coking, we had to have excess hydrogen present, so we had to study that phenomenon.

There was a key engineer down at Texas City, Al McFarlane was his name. Al worked very closely with us on the molybdenum, but after one month of working with a platinum catalysis, I called Al up in Texas City. I said, "Al, all these reams and design documents that you have on the molybdenum account, you're going to have to tear them all up. [laughter] We have now found out how to accomplish what is needed here."

It took a few months before they became convinced. Not much time, because then they went from there to scale it up. In fact, a number of things were remarkable, because while the company had manufactured sulfuric acid catalyst itself for many years, it had never manufactured a noble metal catalyst. The competition was so fierce and the need to move very rapidly so high

that a decision was made to manufacture the catalyst ourselves. We then had to work on developing a manufacturing process for making the alumina-supported platinum catalyst ourselves, which we did. So that was another exciting [laughter] facet for us. I think they've sold that technology. I don't know how the catalyst for that plant is now currently made.

[END OF TAPE, SIDE 3]

BOHNING: Were you involved at all in the development and scaling-up phases, or was that turned over to another group?

ROTH: That was basically turned over to another group. In the case of the dehydrogenation process, it was turned over to a group in Texas City, Texas. They had a process development department there with a couple of hundred people, who were very experienced and very knowledgeable in process development, and reactor design, and reactor engineering. In all fairness, I would have to say that while we had some interaction with them, it was modest.

BOHNING: You've made some comments about this that I'd like to repeat and have your reaction on. One was, and I quote, "Those were the days when quality performance and personal empowerment were not practiced as slogans but as quiet realities" (3).

ROTH: Oh boy! [laughter] We're digressing from the technical developments themselves. I would use the example of empowerment in the acetic acid case. My colleagues and I were allowed to study the fundamental aspects of side reactions because we made the judgment that it was an essential part of developing the technology. I would regard that as a form of self-empowerment.

Another thing is, how did we get into the acetic acid work to begin with? We were at a company conference reporting on some other work that we were doing on low pressure hydroformylation of olefins, and a research director, a gentleman by the name of Walt Knox, said, "Gee, you know the price of methanol is coming down so low because of the introduction of centrifugal compressors and radial flow reactors. If you could come up with a low pressure process for carbonylating it to acetic acid, it would be terrific."

Nobody said, "Go to work on that." We just thought about it, and thought about the possibility. It was not an assigned project; nobody did any arm twisting. It was just an intriguing outgrowth of our work in carbon monoxide and carbonylation chemistry. The first experiment we tried aimed at carbonylating methanol to acetic acid gave us zero yield, but when we added the iodide promoter, in eleven days we had discovered the rhodium carbonyl iodide catalyst.

I regard that as a form of self empowerment. In terms of quality, we had an opportunity to have more room in deciding on the nature and details of the research we were going to carry out than is the case today. As a post-retirement consultant I have gone to visit a large company that has commercialized a process, that is giving something like sixty percent yield of design production. I started asking the research staff about their efforts to elucidate the problems. I asked, “Did you do this?” “No, we wanted to, but we were told by the business people that we didn’t have time to do it.”

Those are basically technology decisions, and the people are disempowered of their ability to do the things that they should do in order to do a quality research job. I think that in the chemical industry today, to a large extent, the business sector of the company has assumed a much greater control over R&D operations—to such a degree that it compromises the ability to do quality R&D. Out of one side of their mouths, they talk about self-empowerment, but in reality, they are themselves the biggest enemies of self-empowerment.

BOHNING: That fits in with the next statement, which is, “Those were the days before our current afflictions of paralysis by analysis and decision making by committees that manage but do not lead” (3). One more quote.

ROTH: Okay. [laughter] You really discovered them all.

BOHNING: I’ve had a question I wanted to ask you, but it will follow from this quote. “Since Monsanto chose not to publicize its position and priority in the dehydrogenation of n-paraffins to linear olefins, neither the company nor its scientists have ever received much public recognition as the originators of this technology. Considering the large impact of the technology, this lack of recognition has continued to be a source of great disappointment” (3).

You said that GAF gave you a thousand dollars for your contribution there, at a time when that was really quite something. Did Monsanto recognize internally your contribution to this process in any way?

ROTH: The only monetary contribution I got was a thousand-dollar bonus for the biodegradable linear olefins. I did get promoted to the technical ladder, and was appointed a scientist. That gave me more status. Later on, when it came to the acetic acid, I think I got a seven-thousand-dollar bonus. But when you consider that the intellectual property alone from that development brought the company one to two hundred million dollars in royalties and license fees, I think they could have done better than that. [laughter]



BOHNING: That was one of the reasons I wanted to ask. I did want to ask you about this promotion to scientist, which was in 1964. You mentioned a technical ladder. Is this the first step on the technical side?

ROTH: Yes. I don't remember the titles, but the technical ladder at Monsanto was a three-tiered one. The first one was a scientist, the next one had some other title, and the top one was the title of "Distinguished Scientist." Monsanto was one of the earlier implementors of the technical ladder program, and they had a fairly large and, relatively speaking, pretty good technical ladder program.

BOHNING: Had you ever thought about leaving the technical side and going into the other side, the business side of the company?

ROTH: Yes, yes. As a matter of fact, when I reached the point in my career after the acetic acid, a man by the name of Bill Williams came in as director of corporate research, and we sat down. I had built up a thirty-man catalysis department, and he said I shouldn't be on the technical ladder anymore. He thought it was appropriate for me to make a choice between being a scientist or getting into management. I thought about it and decided, "Okay, I think I could possibly make a contribution in R&D management." At that point, I became director of first catalysis research and then process sciences, which encompassed electrochemistry and some other fields as well.

BOHNING: Okay.

ROTH: I thought that my track record warranted my being considered for director of corporate research, but when I brought up the question, I was advised that if that were my aspiration, I really had to move into one of the profit centers to gain experience in a business unit. I declined; I did not want to do that. My first love was research, and having developed some commercially used processes while in corporate research—where I spent my entire career—I felt as though that should have been demonstration enough that I was able to continue doing that. Therefore, I never became director of central research at Monsanto, and while I was not actively looking for another job, in 1980, when I was approached, I was highly vulnerable.

BOHNING: I'll come back to that.

The decision to move into homogeneous catalysis, did that occur right after the linear olefins work?

ROTH: Yes. Right after the linear olefin work, we had a new director of corporate research, a very flamboyant man by the name of Richard Gordon, who quickly assumed the identity of “Flash Gordon”—not only at Monsanto but throughout the industry. He came roaring into my laboratory a week or so after he had this appointment and said, “Where is this Jim Roth I’ve been hearing about?” I came and said hello to him, and he told me, “I hear that you’ve been doing some good things. What we need to do is have you do more of them. I want you to start up a group. Think about the scope and impact to the field that catalysis can have—think broadly about that—and come back and give us a proposal.” With that invitation, I decided that one of the major things I could do was expand into the field of homogeneous catalysis. That was not made to solve any specific problem; instead, it was a conscious decision to recognize that as one looked into the future, there might be some opportunities. That was it, and we went ahead and did it.

There was a case of self-empowerment, because I have seen what other companies would do. They’d let you take some people from other parts of the company, and retrain them, even though they had no experience or track record in the field. I was allowed to go out and say to myself, “I don’t know this field. Who is the best consultant? Who’s the best person in the world I can get for that?”

It was Professor Jack Halpern of the University of Chicago. I was allowed to go after him. I got him as our consultant and I began hiring two or three of his students. I had him come in one summer to give a week’s summer course at the place, and that’s where I personally learned about the role of iodide, which then became my major contribution to the development of the acetic acid catalyst. We then took off from there, so I think that represented a high degree of self-empowerment and opportunity to build a high class, world class operation.

Not many years after that, the Japanese were organizing massive multi-million dollar programs in  $C_1$  chemistry, because of this vast effort which Roth and his coworkers had. We didn’t have a vast effort; [laughter] we may have had ten people working in the field. [laughter]

BOHNING: You built a new laboratory too. Is that correct?

ROTH: Yes. We had a high pressure laboratory building, and we acquired part of the space in that building. We established a group of people, who were going to do nothing but operate high pressure reactors for the chemists, as part of the collaborative programs. We achieved a very high degree of efficiency and collaborative effort between the engineering and chemical aspects of the homogeneous catalysis work.

BOHNING: It sounds as if you were virtually given a blank check.

ROTH: It may sound that way, when you look at it today, but not if you look at it at that time. I look at other fields, at the field of polymers. I had a counterpart, a man by the name of Mel Hedrick, who was very successful in the field of polymer composites and was purely a technical man. Mel had a fairly similar charter. There weren't a lot of us. Then there was a man by the name of Bernie Wildi in the area of bioscience. There were three or four of us who were given charters to do that sort of thing.

BOHNING: Was this Gordon's doing, or was it higher up?

ROTH: No. Gordon answered to no one, and that eventually led to his undoing.

That's part of the success story. He created the new enterprise division, which Monsanto must have poured a hundred fifty million dollars in and which led to particularly nothing, was another side of the story. He had created some successes and some failures. Interestingly enough, when the decision came to split these, he went with the new enterprise division and left a little bit, mostly me, [laughter] behind in central research.

BOHNING: Yet, when you started in the homogeneous catalysis area, isn't it true that others were getting out?

ROTH: Yes. DuPont had had a very significant effort in homogeneous catalysis; Hercules had had a significant effort, and there was at least one or two other companies. Englehard had had an effort in that area, palladium-based oxymetalation chemistry. There were three or four major companies that had been in the field for a number of years. That was following the discovery and development of the Wacker process in Germany. Following that, there was an intensive effort for five years maybe, and then practically everybody was just bailing out. That was the time that we elected to get in.

BOHNING: But nobody had used rhodium catalyst before?

ROTH: Well, let me see. There was a limited amount of work that had been done with rhodium-catalyzed hydroformylation, primarily at Shell Development Company. The man who had done that work was a man by the name of Lynn Slaugh. I know Lynn quite well—I haven't seen him for many years—and he has just received next year's ACS Award in Industrial Chemistry.

He was one of the earliest people who did work in phosphine modified rhodium complexes for hydroformylation of olefins. He also used phosphine complexes of cobalt, and it turns out that his complexes were aliphatic phosphines. Shell actually commercialized aliphatic

cobalt complexes for hydroformylation. The patent literature, at least, does not reveal any work by him in the area of the arylphosphine complexes.

You know, I don't know your background.

BOHNING: I'm a physical chemist.

ROTH: The question you put to me was were we first in rhodium base carbonylation? I think the answer to that is no, but we were among the first in rhodium arylphosphine complexes for carbonylation chemistry.

BOHNING: Was this an outgrowth of your hiring Halpern's student, Paulik?

ROTH: Yes. When Paulik came to work for us, he had done a postdoc at Halpern's laboratory, and he had been studying oxidative addition reactions to iridium and rhodium complexes—not catalytic ones. We asked him, "Okay, you're here. What would you like to do?" He thought this was an interesting field, so he started studying some reactions which involved displacement of carbon monoxide and displacement of hydrogen. When he observed mutual displacements, I suggested to Frank these complexes may be hydroformylation catalysts. Initially Frank didn't know what hydroformylation chemistry was, because he hadn't worked in that area, and it was primarily an aspect of industrial chemistry. I don't fault him for it. He then began to work on hydroformylation and achieved promising results. We developed an intensive program in hydroformylation which was later pursued simultaneously with acetic acid work.

At the time our Organic Chemicals Division was interested in building a brand new plant, the world's largest oxo alcohols plant. We were interested in getting them to pick up and follow up on our hydroformylation, rhodium-phosphine catalyzed chemistry. That particular division, as I came to learn later on, was headed up by a group of people who were zero risk-takers. They started to do a little bit of work, the work that William Knowles did for them, which confirmed our results in rhodium-based hydroformylation—where you're reacting olefins with CO and hydrogen to produce oxo alcohols. Now it was up to them to decide what to do. What they did instead was to license twenty-year-old technology from a French company and built the world's largest plant based on proven, established, but old cobalt-based technology.

In the meantime, closely behind us in rhodium-based hydroformylation was Union Carbide. Union Carbide came along and elected to pursue this area themselves, which they did and now have licensed extensively. While the people in the hydrocarbons division were sufficiently entrepreneurial—willing to pursue, develop, and take the risks of new process developments, both in the linear olefins and in the acetic acid case—other people in the Organic Chemicals Division were not. In my opinion, had they had more entrepreneurial people running

that business, they would today have the position now held by Union Carbide, as the preeminent developer and licensor of rhodium-based hydroformylation technology.

BOHNING: Risk is one of the many pieces of a definition of innovation. As you put together how different people look at innovation, risk becomes part of the equation. That must have been quite a disappointment to you, when they turned that down.

ROTH: We actually started acetic acid later, in the latter stages of our work on hydroformylation. When they did that, the only thing that kept our sanity was the euphoria from the success of the acetic acid. It was not only that it kept us going technically, but we had filed some patents that had earlier filing dates than Union Carbide, on what was eventually to become their Pruitt-Smith patent, their primary patent on rhodium base hydroformylation. Had we been more diligent, had we had more company interest, I know we would have had a basis for challenging the Carbide patents, developing an interference, and in my opinion, winning that interference. However, there was no interest at all, in that particular division, in pursuing that and pursuing such an interference.

[END OF TAPE, SIDE 4]

BOHNING: Are these different kinds of decisions made, for lack of any other term, at the local level? How far up in the company does this kind of thing get bounced? Is the top management aware of what's happening at this level?

ROTH: I think the answer is sometimes yes, and sometimes no. Let me give you an example of each.

One of my colleagues at Monsanto was a man by the name of Manuel Baizer who developed an electrochemical process—electrohydrodimerization of acrylonitrile for producing adiponitrile—which became the world's largest organic electrochemical process developed by Monsanto, about the time I did the work on detergents, and they commercialized this. How did that happen?

Manuel Baizer did all of his work in a little beaker, and he captured the interest and attention of a man by the name of Carrol Hochwalt who was at that time the corporate vice president of the research. He reviewed all of this, and he made a decision; "We are going to commercialize this technology!" He then went down to the textiles division located in the laboratories at Durham, North Carolina, happily found a chemical engineer—his name was Don Danley—who was given the assignment to develop this and did a brilliant job because the field of electrochemical engineering was unknown to Monsanto. It was pursued, developed and the

plant was built in Europe. How did it happen? It happened because it got the interest and support of a senior management person who not only had the authority, but also the courage and leadership to say, “We will commercialize this.” I learned something from that experience.

In the case of the work with acetic acid, I made sure that we were able, before we proceeded too far, to get the support of a senior management person in the hydrocarbons division. It was with that support that we were successful.

In those instances, as in the organic chemicals or specialty organic chemicals division—the one that had responsibility for plasticizers at Monsanto—where we did not have that support, nothing happened.

This is nothing new. If you go to, as I had, the MIT Sloan School of Management, you take Ed Roberts’ course there, you learn one of the things that you need is a very proactive champion at the senior management level to make these things happen. What’s happening today is that the aversion to risk is so much greater, and the barriers to innovation so much greater, that without that, you’ve got no chance. You’ve got no chance at all.

BOHNING: When you were turned down on the oxo alcohol process, how long after that before Walter Knox talked about the possibility of making acetic acid from methanol?

ROTH: Not long at all. It happened when we were at a company-wide conference, and one of our people was giving a technical report on our work on rhodium-based hydroformylation—how with these rhodium phosphine complexes, we could hydroformylate at very low pressures. The catalyst was stable; we could use fractional distillation to remove the product and not decompose the rhodium catalyst. Knox asked if these catalysts would carbonylate methanol to acetic acid at low pressures.

Now, by the way, if I may interject this, after William Knowles finished confirming the work with the rhodium phosphine complexes for hydroformylation of olefins to aldehydes, he was casting about for a new project. It was then that he happened to read about the work of Professor Kurt Mislow at Princeton on an elegant generalized synthesis of optically active phosphines and the thought came to his mind, “Gee, this guy Wilkinson has done hydrogenations at very mild conditions with rhodium phosphine complexes. What if I made one of his optically active phosphines, put them on the rhodium?” So he shifted, and it was a very comfortable shift, because all he had to do leave out the carbon monoxide, and he was into hydrogenation catalysis.

So I take a little credit for [laughter] the fact that our effort to commit the company to homogeneous catalysis, which got us into rhodium hydroformylation, had a ripple effect in getting Bill Knowles to work first on hydroformylation, then on hydrogenation. Entirely on his own, he was responsible for developing asymmetric hydrogenation, and for the world’s first commercialization of optically active synthesis with a purely synthetic, optically active catalyst.

BOHNING: At that time, wasn't acetic acid was made from ethanol?

ROTH: No. The process at that time, in use throughout the world was the two step Wacker process, in which ethylene was oxidized with palladium catalyst to acetaldehyde.

BOHNING: Okay.

ROTH: Then the acetaldehyde would separately get oxidized to acetic acid. The Wacker process was itself a major advance in homogeneous catalysis. Our process used entirely different raw materials. Of course, one of the intriguing things is that as part of the concern about energy independence, people were increasingly desirous to get away from the use of intermediates and feedstocks like ethylene derived from petroleum.

Methanol and carbon monoxide can be made readily from coal, or natural gas, or petroleum, but it gives you a technology option of shifting your fundamental fossil fuel feedstock from petroleum.

BOHNING: The first experiments were negative, and there are a number of examples in the history of science, in which initial experiments are negative, and the crucial decision becomes, do you pull the plug or do you continue? What was your reaction at this point? I know it was only eleven days until you added the iodide, but with initially negative results, what was your attitude towards continuing?

ROTH: We asked ourselves the question, why didn't it work? Because rhodium is so facile in reversibly interacting with carbon monoxide, and undergoing insertion and reductive elimination reactions, so why didn't it work? The difficulty probably lay in forming a metal-carbon bond using methanol, instead of an olefin. But how could we do that?

I remembered the Halpern lecture where methyl iodide is a molecule that very prominently undergoes oxidative addition to d8 complexes, and we were dealing with d8 complexes. Maybe what we needed to do was add a little iodide so we could form some methyl iodide which, unlike methanol per se, could directly form a metal-carbon bond.

You can make twenty speculations of that kind, and maybe one out of twenty will work. It was basically asking that question, why didn't it work, when carbonylation of all these other species, the C<sub>2</sub> and higher hydrocarbons did work? It was the fact that methanol itself does not undergo oxidative addition, therefore there's not a pathway to form a methyl-carbon bond.

BOHNING: It must have been a very exciting result.

ROTH: Yes, it was. It was sort of astonishing. I don't think the first experiment, in which we produced twenty percent acetic acid, made it clear to us what we had accomplished. We had to repeat it. We had to demonstrate our ability to do this almost quantitatively, before the exhilaration set in.

BOHNING: Five months from the first experiment, and you were off to scaling up?

ROTH: We had developed a phenomenal body of data. By that time, we had evaluated all of the noble metals. We had evaluated [laughter] a third of the periodic chart for alternatives—for example, iridium worked almost as well—so we had the basis for a very strong patent position, covering catalyst compositions. Then we discovered some things related to the acidity—the medium must be acidic, in order to convert the methanol to methyl iodide. If we added iodide in the form of sodium iodide to an aqueous solution, with no acid, we get no reaction at all. So we defined some of these conditions.

In the meantime, we had one engineer who was studying the kinetics, and the kinetics were unbelievable. Here's a reaction between carbon monoxide and methanol that was zero order in both reactants. How can a reaction behave like this? Then Denis Forster was doing his studies of the elementary reactions in the sequence on the side. He actually captured an intermediate, and had a crystal structure done on one of these. I'd say the research that Forster did, and the kinetics work that Hirschman did, would compare favorably in quality with the best of an academic laboratory.

We had some very excellent quality chemists and scientists to work on this.

BOHNING: The side reactions—the elucidation of the kinetics, the mechanism and side reactions—shows up in almost every example we've talked about since your days back at GAF. I understand that facilitated the scaling up process tremendously.

ROTH: Oh, yes.

BOHNING: It took five months to scale up at the petrochemicals division. How long was it before the plant started operating after that?



ROTH: Didn't any of the papers say what year the acetic acid process started?

BOHNING: No. That's why I have that question here, because I didn't see the year.

ROTH: I think the plant started up in 1970.

BOHNING: There's a number of papers published about these rhodium catalysts between 1968 and 1975 (8). I've counted seven different papers that you wrote that were in the literature about these catalysts. Again, I come back to the question of was this the group that encouraged publication of papers? You said it varied from one group to another.

ROTH: We were largely dealing with the people in the hydrocarbons division. I would say that by and large, they had what I would, from a self-serving interest, describe as an enlightened attitude towards publication. They didn't ask us to publish, but they responded very carefully. By and large, what we did was publish things that were already disclosed in our issued patents. We took the position that our competition is not stupid, if there's information in the patent, they know about it. They've read about it; if they're interested they've probably even checked them out.

BOHNING: Okay. We've already talked about Bill Knowles and his work as an offshoot of your work in rhodium-based hydroformylation. I don't know if there's anything else you want to add there or not.

ROTH: If you're interested in innovation, I think there's an interesting feature—at least I find interesting—and that is that commercialization also occurred around 1973. In the meantime, there was a lot of research, including academic research, all over the world that followed in the footsteps of the Knowles work. It became a very popular field for academic research, and nobody ever came up with anything else commercial.

In the meantime, in the late eighties, almost fifteen years later, the Japanese came up with a major advance in the field. There's a Professor Nyori who has developed something called the BINAP type of phosphine ligand which he used to make rhodium and ruthenium catalysts. They got commercialized in Japan, and that has opened up a whole area of research. Now almost every pharmaceutical company in the world has an effort going on in asymmetric catalysis and products that have been introduced, or are about to be introduced. In the sense of trying to make more environmentally-friendly bioactive molecules, if that molecule is optically active, it's predicted that in the future it will be the desired optically antipode that will be made and manufactured. This is a big activity.

When I looked at what Knowles did, and the fact that it took about twenty years for this to go from some initial stage of introduction, to the point at which there is commercial proliferation of the technology, I'm puzzled. I would have thought that once the stone got turned over, it would have happened faster.

BOHNING: Was the rhodium readily available for the making of these catalysts?

ROTH: Oh yes. You can just buy ordinary rhodium salts from Englehard, add the phosphine ligands, which you can make yourself or buy somewhere, and throw them into a reactor. That's not difficult, it's more making the commitment to work in the area and putting some really creative people to work on them.

BOHNING: Another thing that grew out of the acetic acid work was the ability to make acetic anhydride, although Monsanto never pursued that. It took twenty-one years to get the final patent?

ROTH: Yes. What has happened in the meantime is that a number of people from Eastman Chemical, Halcon, Chevron, and other companies filed and obtained patents related to acetic anhydrides. Monsanto filed its patent on acetic anhydride as a CIP of its original acetic acid carbonylation case, but it ran into an almost endless stream of objections and difficulties. [laughter] I have the letter from a patent attorney stating that after twenty-one years of uninterrupted prosecution, the Monsanto basic patent has been allowed. If you trace that particular issued patent back to its initial filing date, it is the earliest patent in acetic anhydride, but since the company decided not to pursue it commercially, the amount of attention and resources which were devoted to the prosecution of that patent were modest, to say the least. Yet the catalyst used originally in Eastman Chemicals' five hundred million dollar complex that goes from coal to acetic anhydride is a rhodium iodide based carbonylation catalyst. I think it's fairly clear that it is a derivative of our work in rhodium-based carbonylation.

As I said in this paper, there were a couple of things Monsanto did not commercialize. They didn't commercialize rhodium based hydroformylation, and they didn't commercialize rhodium based carbonylation under anhydrous conditions to produce acetic anhydride. Business factors were behind the decision not to pursue those commercially.

BOHNING: By 1973 you were moving away from this research yourself, and becoming more of a manager. Once you had made that conscious decision, how did you feel about it?

ROTH: There were times at first when I had some doubt in my mind as to whether I'd made the right decision or not. But as I reflected over my own scientific career, I thought, "Gee, I've really been very lucky." How many people who get into the business of chemical research ever have the good fortune to be the inventor, or senior inventor, of a major new chemical process? I had the good fortune to have that happen to me, not once, but twice. How many people who have invented a heterogeneous catalytic process have then had a homogeneous process commercialized? The fields are so vastly different that I don't know of anybody. [laughter] I had been pretty lucky, so I thought I could try my hand at something else in the area of research management.

When I went to Air Products, I was hired to start up, from ground zero, a new long range exploratory research unit. I was given a fairly liberal charter to do that, but one of the things that I was determined to do was build what I would call a world-class organization. This was a company that had no strong heritage at all in chemical research, but I think I've succeeded. I think they now have a world-class organization in a number of fields.

When I went there, they had done no work at all in the field of zeolite materials. I think their capabilities in zeolite materials research now rank among the top throughout the world. They are beginning to exploit this, in terms of commercial applications. They had never attempted to synthesize an organic polymer as a membrane, and they're now in the forefront of that field.

In another field, the area of molecular complexes, my colleague Guido Pez has just successfully developed the world's first family of stable, reversibly binding metal complexes that will bind oxygen.

I've enjoyed a certain amount of satisfaction, but on the other hand, I'm very frustrated that the commercial benefit to the company from this technical capability has been very modest.

[END OF TAPE, SIDE 5]

BOHNING: When you set out, as you did at Air Products, to build a world-class laboratory, it takes more than just money. Obviously, you have to have the financial resources, but what else is there?

It's a pretty challenging directive, to take a company that's got a strong financial track record but a very modest history in developing new products and all of the sudden say, "We're going to be involved in chemical innovation, and we're going to do great new things." How do you do that?

ROTH: Part of it was done by the man who hired me, Bob Lovett, because he had decided that Air Products really needed some centers of expertise. There were certain technology underpinnings to most of our strategic businesses. For example, organic synthesis and polymer synthesis was a major underpinning of the chemical units of the company, which comprised about forty percent of the company. Advanced gas separation technology was another, and catalysis was another.

When I took the job, he had defined five areas. Of those five areas, I embraced about four of them. The fifth one, which was cryogenic technology, is largely mechanical engineering oriented, and I did not feel qualified to head-up and lead such an effort. I sort of defaulted on that one.

I thought, “We’re going to operate in this. We have to have somebody heading up each of these areas who has an established track record of achievement and creativity in each of these fields.” I was empowered to go out and hire each of them at what we called our chief scientist level, a level so high that there was no one in such a position in the company.

I went out and tried to do that. I could identify the people, but every one of these people were so well-recognized within their own organizations that I had no chance whatsoever of recruiting them. There was no way I could offer them the incentives. They had the golden handcuffs because of their years in the retirement plan and so forth.

The next best thing was to try to find people who were at an intermediate level, who in my judgment had the potential for developing. That’s what I did. I went out to hire some people. I hired Pez from Allied; I hired John Armor from Allied; I hired Steve Auvil from Monsanto; and I hired Lloyd Robeson from Union Carbide. These are all people who are moving rapidly into national and international recognition on their own right.

I then worked with them to try to instill in them the importance of—when they were building their staffs—looking for, and not compromising on anything less than first-class people. They may know nothing about industrial research, but they should have somewhere demonstrated creative abilities in chemical research in their respective fields.

In fact, the first exercise in recruiting that I had there got me in a little bit of trouble. I worked with the personnel department, and we identified one candidate, a Ph.D. in inorganic chemistry. We set a date and Personnel went ahead and set up an interview panel. Who did they have on the interview panel? They had all managers and directors of research.

I read the interview evaluations, and they were ridiculous. They were entirely a personality evaluation. There was no critical evaluation of these people’s technical ability, so I went back to them and said, “I’m sorry. We can’t continue like this.” Henceforth we created panels that could evaluate critically the candidate’s technical abilities. For the first fifty people that we hired, I was personally involved in recruiting every one of them.

Technical ability and creativity became the standard, once we got the senior people in place.

BOHNING: Most of your experiences that we have talked about, have been with teamwork. What do you do when you have somebody who may be extremely creative but prefers to work alone?

ROTH: That was a very difficult one, because at first I believed that my organization should have sufficient flexibility, that we should be able to accommodate people who were loners or eccentric in such a way that they really could work by themselves. But after having gone through a number of experiences, I rejected that. I found that even with the senior people who run the departments, while it's absolutely essential that they be able to satisfy the technical qualifications, they must also have interactive abilities which enable them to work effectively with other people who are needed in order to fill out the full complement of skills and capabilities needed to get the job done. I would say, particularly in the last five years of my ten-year stay at Air Products, that was one of the essential qualifications, and there were a number of people who were rejected as potential candidates because the judgment was made that they would not be able to work well with other people.

I'm sure that people would debate that, give episodic incidents that run counter to that, but that was my experience.

BOHNING: I have a couple things here that were left over from Monsanto. Let me back up, if I could, to Monsanto again.

When was it that the company decided to get out of catalysts for auto emissions? Was the decision to get out of that business made rather abruptly?

ROTH: Yes, it was made very abruptly. Our program was going along, and we were doing very well. We were working with General Motors, in collaboration with them, and were doing very well. We were either number one or number two among a half a dozen people they were working with in terms of performance. Then something happened at Monsanto. It turns out they went through a period of financial stress. The man who was president of the company resigned rather suddenly, and the chairman took over, and within a day or two after he came in, he had the view that you cannot work with the automotive companies and make any money. He made a singular, unilateral decision that that program would be terminated. It had nothing to do with our performance, but with a decision that the program could only benefit through a business venture with the automotive companies, and he decided that we could not do that profitably. Therefore, stop it.

BOHNING: That kind of decision, at that level, didn't happen too often though, did it?

ROTH: No. Not at that level, but decisions of that kind were made at somewhat lower levels. Maybe a division vice president or someone like that has on occasion made decisions like that, but not the president or chairman of the company.

BOHNING: In that line, I wanted to ask you, how much of an imprint does a new CEO leave on the company at the research level?

ROTH: In some cases, it can leave major imprints, and sometimes in surprising ways. Let me give you an example. When I came to Air Products, in order to get involved in a major way with any major research project, we had to get the blessing of the most closely related profit center. Our entire program, more or less, required that we gather an assortment of blessings. In some cases, that meant we could not work on some things which in our judgment should have been worked on.

This went on for many years; then all of the sudden we had a change when Dexter Baker came in. When he came in, the vice president of research was eventually able to negotiate with him and change that premise. We were no longer required—we were encouraged to do it, and try to do as much of it as we could—but if we had a program that we had very strong conviction about, and which the vice president of research would support, we could proceed. Our ability to select our programs was influenced by who was running the company, and how they were going to control the funding of our programs.

BOHNING: We mentioned earlier your involvement with Roundup. I think at that point you were already director of process sciences, were you not?

ROTH: It was a little bit ironic, because when I became the director of process sciences I acquired responsibilities for the electrochemistry group under Baizer. Just before that happened, the people in the agriculture chemical unit of the company discovered the compound Roundup, had field tested it, and found it had extremely interesting properties, but the few pounds that they needed had been made by brute force methods that would not at all be suitable for a commercial process. It was going ahead, but now they had to develop a commercial process. There's a key oxidative cleavage step involved in making it, and they developed an electrochemical process for doing this. They were about ready to start building the pilot plant when a number of us got together and looked at the chemistry and said, "Gee, we think you might be able to do that catalytically." So we went ahead and began, without much approval or anything, to start some research in the area. Then we reported to the people in agrochemical that we were working on it.

They were kind of skeptical but didn't say stop. Then all of the sudden we had a team, a chemist and an engineer, working on this. The engineer on the program did an extremely insightful analysis of a body of data that he had collected. He had been looking at platinum on carbon catalysts and come to the conclusion that the platinum was contributing very little—it was just the support itself. This resulted from very incisive data analysis. He went ahead and tried it, and voila, the carbon itself was the catalyst which was commercialized, and a determination was made that this engineer was the sole inventor on that particular process.

The economics and everything else about it looked so much more attractive than the electrochemical process that it was dropped, and the catalytic process developed in our department was the one selected for rapid commercialization. The bottom line is, we developed the process for producing glycoside, which is the active ingredient in Roundup, and it was essentially us who made the determination that we had a good chance of being able to perform the reaction catalytically and develop a commercially acceptable catalyst.

It's interesting because that reaction is not conducted commercially for any other process. It involves selective oxidative cleavage of a carbon-nitrogen bond, an aliphatic carbon-nitrogen bond.

I felt good about that, because we had contributed to that wonderful, very profitable product. It also was a further justification of the concept of involving engineers, not in the development and scale up, but at the very exploratory stage. It was the engineer, in this case, who was responsible for a key invention, namely, discovery of a suitable catalyst.

BOHNING: Up to that point, you had to sell people on your ideas. Now, you have people selling you on their ideas. You're on the other side of the fence, as it were. Is that true? How did you react to that?

ROTH: No, I don't think that was really true, because the interaction at Monsanto was what I call a push/pull interaction. Sometimes we were asked to assist in something. Sometimes it was in a very subtle way, like, "Gee, if you could do this?" Nobody said, "Do it." Nobody asked us to do it; they planted a seed, but we took the initiative to do it.

In another case that I haven't mentioned, my group developed another process that was commercialized. One of the divisions, the specialty chemicals divisions at Monsanto, was interested in getting into the business of making diphenylether by the coupling of a phenol, so they asked us to work on it. We worked on a collaborative program, and we developed the catalytic technology that was used and commercialized for producing it at a significant commercial level.

In the case of the linear olefins, there's a case where we were specifically asked. If viewed impartially, the work on acetic acid would probably be viewed as one where we provided

the initiative. L-DoPA was a case where the researchers provided the initiative, but the diphenylether was the other way. It was kind of a mixed bag.

BOHNING: Let me return, again, to some general comments that I pulled out of some of the things you wrote.

“If you wish to do some trail blazing and create technology leadership positions, don’t follow the bandwagons” (9).

ROTH: This gets into an aspect of innovation development that I’ve spoken about when I talked about the nature of the S curve. Have you read any of those papers that discuss the S curves (10)?

BOHNING: No.

ROTH: I have written that any given technology goes through an S curve. This is the nature of the learning process. When a technology is mature, you have to spend a lot of time and money to make a very small further advance. At that point, a discontinuity occurs and someone comes along. You start out by making acrylonitrile from acetylene, and you do that as well as you can, continuously making small improvements. Then—voila—somebody comes along at Sohio and makes it from propylene. This is a technology discontinuity.

The maturity of chemical technology today is such that most of them are on the upper portion of this S curve, and what is needed is a technology discontinuity. If you want to make a discontinuity, you’re not going to do so by trying to incrementally evolve yourself there, by continuing to follow and doing what other people are doing.

Pez started out with a few of the known complexes, but he quickly gave them up. When the answer is a new composition of matter, that’s usually a discontinuity, and that’s not something that you’re going to make. That did not evolve from following the bandwagon, [laughter] or from trying to incrementally expand that which we already know. It takes very creative people. Guido Pez is probably the most creative chemist I have seen in American industry. When I’ve gone to Guido, who worked for me at Air Products for ten years, and tried to get him to work on a problem that represented an incremental advance, I could never get him to work on it. Even if he had an idea, he wouldn’t want to work on it. He wasn’t sufficiently challenged.

BOHNING: Is the ability to see these discontinuities a function of age? Do you see that age has anything to do with it?



ROTH: I wonder. When Bill Knowles did his pioneering work on asymmetric catalysis, he was well into his fifties. Pez has been working on his new complex for ten years, and he just turned fifty. I don't know whether you regard that as young or old. On the other hand, some of the other things that I've seen have been done by younger people who may be less encumbered by conformity and more willing to engage in free thinking.

BOHNING: One of the reasons I asked that was because if you spend a long period of time in a single area, there's a danger of not being able to see beyond.

ROTH: In the case of Pez, he did not make the breakthrough himself. I think you have a good point here. He worked with a series of young people. They would come to work for him for two or three years, and then they'd move on to something else. The person mostly responsible for this major breakthrough is a new person who came to work for him, Dorai Ramprasad, and he is a young scientist. He is the one who came up with the new approach based on cyanocobaltate type complexes. But Pez has had a phenomenal record of finding the problems, hiring these people, motivating them, and getting remarkable results out of them.

[END OF TAPE, SIDE 6]

BOHNING: A few other quotes. "The career of an industrial chemist is often erratic. It shuttles between occasional moments of success and euphoria that are all too infrequent, and many interludes of frustration, disappointment, and even disenchantment" (3). You say that you experienced every single one of those categories that you mentioned.

ROTH: Oh, absolutely. Many people who work in exploratory research in industry never achieve a major commercial success. That has got to be frustrating. I know my wife, who's not a scientist, says she could never do long-range research because the personal/emotional need that she has for seeing the results of her endeavor, in some reasonably short time, is too strong. She wants to get closure and completion with regard to her efforts.

Frustration occurs when you come up with something new, but because of weak commercial development, or business reasons, or something else, the company makes a decision not to follow up on that. That can be enormously disappointing. I know of a man by the name of Albert Chan who I think left Monsanto for that reason. He had come up with a new synthetic route to a high valued product, and the company decided not to pursue it.

It takes a long time, and it's very difficult to achieve success. As I look about the world of chemical technology today, the weak response of corporations in general—in terms of supporting the commercialization of new opportunities, investing in them, and exhibiting the patience and financial investment that's required—is very discouraging.

BOHNING: Is there much of a reward system? As you said, many people go through their whole career and don't have a major breakthrough in commercialization. Most people seem to like some kind of reward system along the way, to have some kind of recognition, even if it's only within the company. How much does the chemical industry today, as opposed to your time, have a reward system for its research people?

ROTH: It's getting better. It has created technical ladders. Many companies have technical ladders, which assign special recognition, and some increased financial recognition and reward, to those people who are the top performers. In terms of other things, like special awards, I know Monsanto has established Edgar Queenie and Hockwalt/Thomas Awards, and Air Products now has awards of either twenty-five or fifty-thousand dollars, that can be given at the chairman's discretion. People can and do get those rewards.

I think that's a step in the right direction. Because having operated myself now for a number of years in the executive branch of Air Products, I'm fully aware of the compensation opportunities that exist for business executives or management executives in the company—even the technology management executives—in terms of bonuses and stock options that are open only to a limited extent to scientists on the technical ladder.

To a large extent, scientists have historically and even today continue to get as one of their rewards, the self satisfaction that of recognizing their own accomplishments in a scientific or technologic context, whether or not they're roaring commercial successes. That's manifested itself in a different way. For example, presenting papers at national and international meetings is a perk that many scientists enjoy. Receiving awards, those that are good enough to achieve those forms of recognition; memberships in honorary societies, like the National Academy of Sciences, National Academy of Engineering. All of these are forms of reward and recognition that are given outside the confines of the company. But I think as society becomes increasingly focused on financial rewards, this is going to become more and more an issue for scientists.

BOHNING: Another quote. “In today's environment, translating new knowledge into a profitable commercial product is not easy. The barriers that one encounters are, at times, overwhelming. There are many commercial, financial, and cultural hurdles to overcome” (3).

I was struck particularly by the inclusion of a cultural hurdle. I can see commercial and financial, but cultural was surprising.

ROTH: I would include under the nomenclature cultural such things as aversion to risks, a lack of self empowerment, and other things of that sort. One of the things that I said to one of my colleagues at Air Products who was always looking for ways to achieve more technology breakthroughs is that I'm now convinced, as I sit back in the wisdom of my years of retirement, is that if somebody came along and presented an opportunity for a Roundup, or an acetic acid to Air Products or many other companies in today's environment, they would fail to commercialize it and convert it into the commercial success that each one of them achieved in the early 1970s.

That's a pretty powerful statement, and I believe that to be the case.

BOHNING: You're saying there's a mindset there?

ROTH: Yes.

BOHNING: Okay.

I'm almost through quoting your statements for you to comment on. One last one is, "Conventional wisdom is rarely the pathway to technical leadership" (3).

ROTH: That's kind of a restatement of this biz of following the bandwagons not being the path to a major novel innovations.

BOHNING: You have this list of agenda questions that we've pretty well covered at this point (11). Let's take a look at number 15, the last two.

What is important for the future of R&D? You've said to me before, as we were off the record, that you felt most innovations would be coming from outside the United States. Could you elaborate on what you think is important for the future of R&D in this country?

ROTH: I think that most of the research, by far the great majority of research in this country, is aimed at incremental improvements in products or processes. The breakthroughs in the future and the opportunities will likely come from the outside, because they're not being worked on here. What I have tried to do, in the field of catalysis, which I know quite well, is to identify major, unsolved problems that, if solved, would provide innovations and breakthroughs.

The highest volume product produced in the United States is sulfuric acid. The technology for producing that is quite old. It hasn't changed in maybe fifty or more years. Yet, if

you look at that, and you ask yourself the questions, what are the limits of this technology and what are its characteristics, it turns out to be a fairly capital-intensive process. One reason is that the present catalysts require that the oxidation of  $\text{SO}_2$  to  $\text{SO}_3$  occur at fairly high temperatures, like 500+ degrees Centigrade, at which point the thermodynamics only allow substantial but incomplete conversion. You then have to go through multi-stage reactors, and that represents a lot of money. There are also people who would like to decrease even further existing  $\text{SO}_2$  emissions, so that they don't photooxidize in the atmosphere to contribute to acid rain.

When you look at all of that you say, "What could we do about that?" One of the things we could do about that, is develop a low temperature catalyst. If we had a catalyst that, for example, oxidized  $\text{SO}_2$  to  $\text{SO}_3$  at 200 degrees Centigrade, rather than 500 degrees Centigrade, it would have an enormous difference, reducing the size of the reactors that are needed. You might do it in one reactor. Reduce the capital, reduce the energy intensity, improve the environmental situation.

The question is so obvious, and who's been working on that problem in the last thirty years? I know of no one. Someone will come along. Maybe someone will read one of my papers, somewhere in Poland, or in the Soviet Union, or in Korea, and they'll work on that problem, and come up with a low temperature catalyst. When they come up with that low catalyst, they will have a technology that will dominate the future process technology for making sulfuric acid.

I can go down the list of the fifty top chemicals produced in the United States, and describing the limits of existing technology, and the reasons for those limits, project one or more potential scenarios for a new process innovation. I can't tell you how to do it, but pathways exist for major innovations in chemical process technology. What I say for the field of catalysis, which is very important and very big, could also be said for polymers or other established fields, but the pursuit of new discontinuities is not very active in the U.S.

So if you ask what could be done, what could be done is to pursue what I'm suggesting, which is to look at this so-called mature chemical industry and ask ourselves incisively, what possibilities are there for drastically changing the processes and in some cases the products that we presently make, and displacing them. I'm not saying all the research, but that would create an enormous body of research which is not now being pursued at all, in deference to small incremental improvements.

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

ROTH: I thought a little bit about that. I have a small answer to that question, and that is, I think it gave me a sense of increased self worth. Presentation of the Perkin Medal occurs at a formal, elaborate dinner attended by four to five hundred people, executives from all over the country coming to help celebrate the scientific and technical accomplishments of the medalist.

Before winning the Perkin Medal, I didn't feel as though my accomplishments were perhaps as important as I now feel they are, [laughter] or as widely recognized. The value of what I've accomplished in my life, has been made to look better, to me, at least, as a result of having won the Perkin Medal.

BOHNING: Okay. Is there anything you'd like to add that I haven't covered?

ROTH: In order to improve the prospects for the future of chemical R&D, we need to pursue these unsolved problems of chemistry, but not only creatively in the R&D sector, but also creatively in the commercial and business development sector. I feel that the latter has very often been the reason for lack of success. Somebody's come up with the technical lead, which has gone nowhere.

I do firmly believe that these things are not insolvable, and that some of what I'm suggesting here [laughter] may have some merit in creating a conceptual design for addressing the future. The mistake that many people make is that they look at the way it was done before, forty years ago, thirty years ago. The academics have always enjoyed ability to do untethered research, just generate new knowledge. That was wonderful forty years ago, but today, as the science and technology has matured, I personally think they should be working more—not entirely—but more in focused fundamental research than they are. I think the approaches we need, even in terms of organizational design and approaches to face the future, have to be different from what we've been using.

BOHNING: On that note, I thank you very much for spending the afternoon with me. I thoroughly enjoyed it, and you've certainly given us some very good information. Thank you, again.

[END OF TAPE, SIDE 7]

## NOTES

1. W.J. Svirbely and James F. Roth, "Carbonyl Reactions I. The Kinetics of Cyanohydrin Formation in Aqueous Solution," *Journal of the American Chemical Society*, 75, (1953): 3106-11.
2. G.P. Wachtell, H. Bickford, L. Conant, and James F. Roth, "Ignition of Solid Propellants by Natural Convection and Radiation," *Bulletin of the First Symposium on Solid Propellant Ignition*, Silver Spring, Maryland (1953).
3. James F. Roth, "Some Adventures and Innovations in Industrial Catalysis," *Catalysis Today*, 13(1) (11 March 1992): 1-12.
4. Vladimir Haensel, interview by James J. Bohning in Amherst, Massachusetts, 2 November 1994 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0115).
5. James F. Roth and Robert C. Doerr, "Air Pollution Studies: Oxidation-Reduction Catalysis," *Industrial Engineering Chemistry*, 53, (April 1961): 293-6.
6. Jim Fair, interview by James J. Bohning in Austin, Texas, 19 February 1992 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0102).
7. A. Wolberg, J.L. Ogilvie, and J.F. Roth, "Copper Oxide Supported on Alumina IV. ESCA Studies," *Journal of Catalysis*, 19(1) (October 1970): 86-9.  
  
A. Wolberg and J.F. Roth, "Copper Oxide Supported on Alumina III. X-Ray K-Absorption Edge Studies on the  $\text{Cu}^{+2}$  Species," *Journal of Catalysis*, 15(3) (November 1969): 250-5.  
  
Pierre A. Berger and James F. Roth, "Copper Oxide on Alumina II. ESR Studies of Highly Dispersed Phases," *Journal of Physical Chemistry*, 71, (1967): 4307-15.  
  
E. D. Pierron, J. A. Rashkin, and J.F. Roth, "Copper Oxide on Alumina I. XRD Studies of Catalyst Composition During Air Oxidation of Carbon Monoxide," *Journal of Catalysis*, 9(1) (1967): 38-44.
8. Peter R. Rony and James F. Roth, "Supported Metal Complex Catalysts," *Journal of Mol. Catalysis*, 1(1) (September 1975): 13-25.  
  
Arnold Hershman, K. K. Robinson, J. H. Craddock, and J.F. Roth, "Continuous Propylene Hydroformylation in a Gas Sparged Reactor," *Ind. Eng. Chem. Prod. Res. Develop.*, 8(4) (1969): 372-5.

- K.K. Robinson, F.E. Paulik, A. Hershman, and J.F. Roth, "Catalytic Vapor Phase Hydroformylation of Propylene Over Supported Rhodium Complexes," *Journal of Catalysis*, 15(3) (November 1969): 245-9.
- J.H. Craddock, A. Hershman, F.E. Paulik, and J.F. Roth, "Hydroformylation Catalysis by Arylphosphine Complexes of Rhodium," *Industrial Eng. Chem. Prod. Res. Develop.*, 8, (1969): 291-7.
9. James F. Roth, "Perkin Medal Address," *Chemistry and Industry*, October 3, 1988.
  10. James F. Roth, "Evolving Nature of Industrial Catalysis," *Applied Catalysis: A: General* 133, (1994): 131-140. Reprint. Amsterdam, The Netherlands: Elsevier Science Publishers B.V.
  11. James F. Roth, interview by James J. Bohning in Sarasota, Florida. Research file. (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0128).

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