

CHEMICAL HERITAGE FOUNDATION

FREDERICK J. KAROL

Transcript of an Interview
Conducted by

James J. Bohning

in

Bound Brook, New Jersey

on

10 January 1995

(With Subsequent Corrections and Additions)

ACKNOWLEDGEMENT

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FREDERICK J. KAROL

1933 Born in Norton, Massachusetts, on 28 February

Education

1949 B.S., chemistry, Boston University
1962 Ph.D., organic chemistry, Massachusetts Institute of Technology

Professional Experience

Union Carbide Corporation

1956-1959 Chemist, Chemical and Plastics Group
1962-1965 Chemist, Chemical and Plastics Group
1965-1967 Project Scientist
1967-1969 Research Scientist
1969-1978 Group Leader, Chemical and Plastics Group
1978-1981 Research Associate and Group Supervisor
1981-1984 Corporate Fellow
1984- Senior Corporate Fellow

Honors

1982 Thomas Edison Patent Award, R&D Council of New Jersey
1987 Excellence in Catalysis Award, Catalysis Society of Metropolitan New York
1988 Chemical Pioneer Award, American Institute of Chemists
1989 Perkin Medal, Society of Chemical Industry
1989 Conley Award for Plastics/Engineering Technology, Society of Plastics Engineers
1990 International Award, Society of Plastics Engineers
1990 Collegium of Distinguished Alumni, Boston University
1991 Award for Creative Invention, American Chemical Society
1991 50th Anniversary Recognition Award, Society of Plastics Engineers (Newark)
1992 New Jersey Inventors Hall of Fame
1992 Outstanding Presentation Award, American Institute of Chemical Engineers Meeting, New Orleans

ABSTRACT

This interview with Frederick J. Karol begins with a short discussion of Karol's family background and childhood near Boston, Massachusetts. Following an early interest in chemistry, Karol in 1946 enrolled at Boston University and graduated with a B.S. in chemistry before enlisting for two years of military service. He worked for Union Carbide from 1956 to 1959, began a family, and then entered a graduate program at MIT, studying statistical thermodynamics and organic chemistry under Gardner Swain and conducting thesis research on isotope effects. He continued catalysis research upon his return to Carbide in 1962, eventually developing a variety of proprietary catalysts for use with a high density polyethylene gas phase process. Karol's contributions to the development of a gas phase process for making polyethylene products under low pressure helped to revolutionize the industry, as Union Carbide next developed this technology to commercial operations. The interview describes the worldwide licensing of the linear low density polyethylene process, its economic and environmental advantages, and the extension of this technology into synthetic rubbers; also discussed are the technical and management necessities for such innovative developments. Karol contributed to Carbide's collaboration with Shell Chemical Company, which produced polypropylene, improved the catalytic system to make a wider spectrum of polypropylenes, and eventually led to process licensing. Here Karol discusses kinetic and analytic studies to understand the fundamental principles and mechanisms of polymerization; catalyst requirements and testing involving screening of reactions, analysis of property indicators, and use of pilot plants for testing; and his role in guiding development. After describing Karol's education and subsequent research, the interview focuses on Union Carbide's history and work environment, support for R&D and publishing, and Karol's career progress and professional philosophies on management and scientific innovation. Karol describes the history of linear low density polyethylene, the development of both the Ziegler-Natta process and the UNIPOL process, and Union Carbide's licenses and worldwide ventures. The interview closes with a discussion of the future of R&D and the chemical industry, and the significance of the Perkin Medal.

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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INTERVIEWEE: Frederick J. Karol
INTERVIEWER: James J. Bohning
LOCATION: Bound Brook, New Jersey
DATE: 10 January 1995

BOHNING: I know you were born on February 28, 1933, in Norton, Massachusetts. Could you tell me a little bit about your father and mother and your family background?

KAROL: Okay. We lived in that area for a short time, and then we moved from there to Lowell, Massachusetts. I don't recall much of anything in Norton, because I was just a few years old when we moved to Lowell.

Both my mother and father were Polish, although they were born here in the United States. They suffered through the Depression. My father worked for a gas company. He had a high school education, but he was in charge of gas manufacturing—from coal, making gas. I remember his operating this whole panel of equipment for that process. My father had lost his job near Norton, Massachusetts, so we moved to Lowell. That's what got us to Lowell, because there was a job there.

My mother's background was, she had a high school education. She was one of about seven or eight in the family, and my father was one of ten or twelve—large families in both cases.

I grew up in Lowell, which is north of Boston, and went through grammar school and high school there. I had one sister who was about four years younger than I was, and all of her bringing up was also there in Lowell. I'm not sure there are any great events from those days—except that when we lived in Lowell, we got dumped on with a lot of snow. The only real significant event was, I got into high school. I decided, "I ought to do something significant in high school." I said, "Why don't I graduate first in my class; it'd be a good thing to do." I was rather well organized, so I set out to do that. I was in a class of about six hundred and fifty, so that wasn't too bad.

I played musical instruments; I played in the band. I had a few other things in school—the science clubs and things. Also noteworthy was, my first exposure to chemistry was in high school. I had, probably, one of the most outstanding teachers in that area—very challenging and motivating. Since I did well at it, I figured, "That's something that I might want to continue."

BOHNING: Is this teacher Richard Conway?

KAROL: Yes, that's Conway. You heard that from someplace?

BOHNING: You filled out a form for us in which you mentioned his name.

KAROL: Oh, yes. Good guy. He was really very challenging, an unusual person. It was a good experience for me.

BOHNING: Did you have a good lab experience with that as well?

KAROL: I found that I was better out of the lab than I was in the lab, in terms of manipulative skills. [laughter]

I ended up graduating from high school in 1950. When I went to college, I went to Boston University.

BOHNING: Why did you select BU, as opposed to Northeastern University or any of the other schools in the Boston area?

KAROL: I don't know. When I finished high school, I wasn't sure what I wanted to do. Even though I had graduated very high in my class, I actually didn't go to college right away. I worked for about six months.

It's tough, in the middle of a semester year, to get in, and then I had an opportunity at Boston University. I thought, "That's a good place, go there." I was at loose ends for a while. I did go back, and I did all right in college. I didn't have the same motivation as I did in high school. I was going through a period where I wasn't sure what I really wanted to do—an identity crisis, maybe. I ended up four years at Boston University. I did okay, but there was nothing outstanding.

BOHNING: You majored in chemistry?

KAROL: I majored in chemistry. The thing I remember most was that all four summers, I worked up at a Boston University location. They had a summer camp up in Peterborough, New Hampshire. I don't really know that area. But there was a part of Boston University which was called Sargent College, a women's college. They had summer camp up there. My thrill was working up there in the summer, four summers.

When I finished college in 1954, this was the end of the Korean War. A whole bunch of us who knew each other enlisted in the service. You'd go in, you had to stay only two years. Rather than wait to get called—they were calling people—we just went and enlisted. We figured we'd get it over with.

I spent two years in the service, from 1954 until 1956. I was over in Germany during that time. I recall that four or five months before I was getting out of the service, I said, "I've got to figure out what the heck I'm going to do with my life." I had inquired to a number of companies to see if they had openings.

I interviewed here at Carbide in 1956—not at this location; we had another facility in Bloomfield, New Jersey, which is north of here, thirty-some miles. I interviewed at several companies, and I got a very nice offer—I thought it was nice at the time—from Carbide.

It certainly wasn't the facilities, because they had the worst facilities of the whole bunch. It was really an old, old building. But I liked the people I met. I recall that the salary offer I got was four hundred and thirty-five dollars a month. I said, "That's kind of nice," and I took it.

It was very significant because I worked for a fellow, when I first came in, whom I really liked and who was just beginning in a whole new area of chemistry. It's really launched off where I ultimately ended up, because it's the area that's known in the industry as Ziegler-Natta catalysts. Discoveries were just being made when I was beginning. I did some work in the laboratories, and I got very interested in the area. I had just a bachelor's degree at that time.

I worked there for a couple of years, and then I got married in 1958. My wife is the same wife I have today, from Massachusetts. Then I thought, "Maybe I ought to go back to graduate school." I thought the opportunities would be better, and I felt I needed some additional background in science. We had one child. So we decided in 1959 to go to graduate school.

I left Carbide in 1959 and got a position as a graduate student up at MIT. It was a good spot for us, because we were from Boston. My wife could be close to her family, and my child. I worked up there in physical organic chemistry for a fellow named Swain, Dr. C. Gardner Swain, who was at that time doing some very important physical organic chemistry.

When I first got the assignment, we talked about it. It was in the area of statistical thermodynamics, which is really in the physical chemistry department. I was in the organic

chemistry department. Swain said, “Well, you don’t really have the background to do this project.” I said, “I’ll complete this project. Okay?” [laughter]

I took a project on isotope effects in graduate school, doing a lot of theoretical calculations and calculating partition functions which, at that time, I knew nothing about. I worked extremely hard in getting through the organic chemistry as well as the physical chemistry. Plus, I had been out of school for a number of years, because I had worked and been in the service. So I was in a little bit of a shock when I saw the challenge that was there. I got very interested in the particular area. I learned a lot from other graduate students working in the same discipline.

I had a wife and one child, and another child when we were at MIT, so it was some motivation to not linger around too long in graduate school. One of my motivations was not only to get my project done, but to also get out of there, get a degree, learn what I could. I got out from MIT in two years and nine months, which was reasonably fast. I graduated from there in 1962. I did all of my work in the areas of statistical thermodynamics and organic chemistry.

I decided where I was going to come to work. Since I’d worked previously at Carbide, I interviewed a number of other companies at the same time, but I did decide to come back to Carbide. The main reasons were that I liked the kind of work I had been doing, I liked the fellow I was working with, and it looked like there’d be some nice opportunities there. So I came back in 1962. I worked in some areas that were a little bit unrelated to where I ultimately ended up—but I was doing work in polymerization catalysis.

BOHNING: Were you in catalysis the first three years you were there?

KAROL: Yes. I’ve been in catalysis of one type or another during my whole career at Carbide.

I worked on a number of areas in catalysis—but not in polyolefin catalysis, which is what I had worked with before I went back to school. Carbide was a big producer of polyethylene. We were one of the ones running high pressure routes to make polyethylene, and we’d gotten involved in low pressure routes. We were originally a licensee of Phillips Petroleum, about the time that I was coming back into the program. Carbide was interested in low cost processes and new routes for making polyethylene. We had some engineering people who had been fooling around with gas phase processes.

We started out to develop proprietary catalyst technology. Initially, there was a certain amount of resistance. We had gotten a licensing from somebody else—a certain amount of comfort index there. To come in and bring new systems in was a challenge.

I worked in 1964 and 1965 on some proprietary systems. We had made some progress in that area. Then I got transferred into another area of catalysis because they needed somebody over there. Here I was, back in the area—and then I got transferred out. I worked in some other areas of free radical chemistry. I assumed that I would not ever be back in polyolefins. But we had some reorganizations about a year or two later, and the fellow I had originally worked with wanted me back into that area. He said, “You’ve got to come back in this area.”

I went back with a small group of people. This was now 1967, 1968. We launched a series of catalytic programs to develop a variety of proprietary catalysts that could be run on our units. The process we used was a solvent process. With the emerging gas phase process coming up, what we did was to merge the catalytic technology we had with this new process we had. It was translating catalysts into a process for making what was known at that time as high density polyethylene.

BOHNING: Could I ask a question about the gas phase process? If I’m correct, up to this point almost all of the processes were solution and slurry processes.

KAROL: Solution and slurry processes, yes.

BOHNING: Was it Carbide who made the decision to look at the gas phase process?

KAROL: Yes. There was a fellow whom I personally had not met, but who said that Carbide ought to somehow get involved in the lowest cost process. High pressure processes run at twenty thousand to thirty thousand PSI and two to three hundred degrees centigrade, very expensive to build. He said, “Why don’t we get rid of all that and look at that gas phase?” It was a business challenge.

BOHNING: Okay.

KAROL: That’s the setting. The important feature was, there was a receptivity at a level to do some of these things that were radical.

We tested out varying catalysts. We tried to find out what would make them work in the gas phase. No solvent here—we had a heterogenous catalyst making polymer. The process consisted of taking solid catalyst particles, putting them in a reactor, and growing polyethylene on these catalyst particles up to a certain size. I’ll talk more about that. I just want to capture

the challenge of saying, “Toss it in the gas phase.” It sounds simple, but there were lots of challenges.

We developed several catalysts for high density polyethylene. We were able to demonstrate that they could run under commercial conditions. Commercial conditions meant the catalyst residues were so low in the product, we never even took the catalyst residues out. Productivity had to be close to one million pounds of polymer per pound of transition metal. A typical transition metal at that time was chromium. Subsequently, there were others which worked here.

We’re talking about the period of 1968 to about 1971. We were doing this work for high density polyethylene, and we were beginning to look at licensing this technology to other people. We had an affiliate in Sweden and we had an affiliate in Australia, whom we licensed. So what we saw emerging was a licensing concept, as opposed to our doing everything ourselves.

BOHNING: Was this licensing new for Carbide, or had they done this before?

KAROL: Oh no, it wasn’t. In the past, there was the concept that if you got something like that, you kept it to yourself. You just don’t go out and license it. There was an interplay going on at this point, “Is this something we should or shouldn’t do?” So the first entree was to do a little bit with affiliates at this time.

All of the focus up to this point was dealing with what we called high density polyethylene. It was not attacking the major area of high pressure polyethylene, which was known as low density polyethylene—billions of pounds of material. But what we did establish was that a gas phase process could be credible for making some polyethylene.

Now, the challenges in just making polyethylene were, polyethylene isn’t like acetone or a discrete molecule. It can be a whole variety of things—it has a molecular weight; it has a molecular weight distribution. We had to make the right molecular weight and molecular weight distribution. Normally, we copolymerized ethylene with alpha olefins such as propylene or butene to get some branching along the chain, but only a little bit. All of these have to be dialed into the catalyst, so that the catalyst does all of these things.

There were a whole variety of challenges to make that happen. When we ran in the gas phase, we were normally running about eighty to one hundred degrees centigrade. If we were up much higher than one hundred degrees, the whole bed would gum up. Even when we were running, we had to be careful that the reaction didn’t run away on us, because we ended up with an agglomerate that could be twenty thousand pounds to one hundred thousand pounds. There

were some challenges in getting it out of the reactor. There were a lot of other challenges of that type.

All of this was dealing with an arena where we got credibility from the gas phase, but we hadn't applied the gas phase to go after the high pressure materials, which had more branching in the polymer chains. There's a set of properties associated with high pressure as opposed to low pressure.

We started a program in 1971-1972. We said, "We know gas phase works. Let's find out what it takes to do this under low pressure and attack this huge market." It was an interesting thing for Carbide. We were a leading producer of high pressure polyethylene, but we were saying, "Hey, we're going to come in and replace all this technology." Which I think, in the history of science, is an interesting perspective. Normally, you have another company that might attack your technology and other things. Here's a case where we were actually going after and attacking our own high pressure technology.

We had facilities all over the United States, many, many reactors—some in Puerto Rico, here in the U.S., and so forth. Some people were watching us all along because they wanted to figure out, "Is this really going to happen?" There were some doubts as to whether this was really going to happen. Even if we made it, would the properties be satisfactory?

BOHNING: Who was involved in making these decisions?

KAROL: Which decisions?

BOHNING: These were research decisions, business decisions. At what level were they being made? Who was the champion of the cause?

KAROL: Clearly, in an effort of this type, in order to make anything happen, we had to have people at the top of Carbide. I particularly remember John Luchsinger, who was at that time an operations manager. Back then, at about 1968, we were putting in our first gas phase reactor as a commercial unit. I recall John as being the business operations manager and spokesman for that area. He certainly championed this cause to the hilt. It was a significant risk at that point, just demonstrating the credibility of the process. Then the second part was, "What are you going to do with this process, in terms of the kinds of product opportunity?" He was involved, and there were several other people in the organization at the operations manager level. But the one I interfaced with, in terms of providing the business perspective, from my point of view, was John Luchsinger.

His challenge to us was, “This is what we want to do.” Then we figured out how to do it. “What is the mechanism which we’re going to use?” Because in this kind of program, where we have lab people, we have product people, we have process people, and we have plant people; the secret of this thing is to integrate all of these things so that they all kind of work together. One of the things I’ve always felt very strongly about is that, in the research that we do, we need to constantly interface and be part of the process, part of the product.

Part of my role was in the discovery and invention of these catalytic systems. But also, it was working with the process and product and manufacturing people, in terms of establishing credibility for this and translating the technology from the lab to a commercial unit. When new people came in, one of the things they didn’t have is an exposure to this kind of thing. Part of my role was to be sure that people had this kind of education.

[END OF TAPE, SIDE 1]

KAROL: It’s interesting. You have a variety of different people working, and with some people there is an attitude, “If we do the basic research, then implementation is trivial.” They need remedial training. We have to teach them that that isn’t the case—that a significant part of the success of any project is not that they understand the detailed chemistry, but they can take care of that in terms of translating that into process, into commercial operations. That’s what I call innovation—translating the inventions that are being made into commercial reality.

What I’m struck with over the years is how we do this, I think, quite well now. But at the beginning, there was a period of having to do this and getting manufacturing even to talk to R&D people. I think that those hurdles—in this area at least—are not a problem today. They were something that had to be incorporated.

We had a business climate that said, “We want to do this kind of thing.” My role was to work with the catalyst people in terms of developing the technology, then to interface with the product and process people, in terms of being able to show them how to use this and translate that. Clearly, there were lots of people involved in this—engineering, product. We looked at it as though the reactor provided the economics and the catalyst provided the product opportunities. Do you understand?

BOHNING: Yes.

KAROL: Our philosophy has been to look at a variety of different catalysts. Any one catalyst can take an awful lot of time to develop and study, but if you get hung up on any one system, you find that you don’t have as much flexibility as you like. So, our philosophy was, if we run

into a stalemate with any one piece of catalyst technology, we are going to look at another one. We've always carried several catalyst systems too, because they provide flexibility. Their structure and formulation play an important role in commercial operations.

Anyway, we set off, in the 1970s, to try to make products as replacements for high pressure polyethylene. We worked with certain chromium catalysts. We were able to identify a chromium-based catalyst that made some attractive substitutes for high pressure polyethylenes. But the materials that we made were satisfactory for special applications, in what we called the wire and cable area. The big thing in high pressure polyethylenes is having the capability of making products that would fall into the film arena. The catalysts that we had were not satisfactory for doing that, although we did develop this chromium-based technology. We did show that we could make, in the gas phase reactor, some attractive products. So we made a step forward. We went from high density polyethylene into low density polyethylene, but we hadn't quite gotten where we wanted to be. We wanted catalyst technology that would make product replacements for high pressure. If we used the gas phase process, our economics were so much better.

We set out on another research program, looking for another catalytic system that would do that. We demonstrated we were successful with this new system in about 1977. We developed some technology that allowed us to make products that looked as though they would be very competitive with high pressure. We made announcements—going all the way up to the Chairman of the Board—that we had been successful in demonstrating, in a gas phase process, that we could make products equivalent to available materials. Some of the properties of this material were actually superior to the high pressure materials that were there.

These discoveries create a huge new revolution, because you have high pressure reactors, all over the world, making these materials, and Carbide coming along and saying, in 1977, "We can do this by a low pressure process, with better economics, in terms of investment and operating costs." At the same time, we made a decision to license this technology in a serious way around the world.

I mentioned earlier that the person I interfaced most with was John Luchsinger. The person who was involved, after John, was a fellow named Bill [William] Joyce. I became aware of Bill and his whole business involvement in this area around 1975, 1976. He has been the major spearhead, business champion of this technology from that time on. Bill, at that time, was an operations manager. Today he is the chief executive officer of Union Carbide.

Bill was able to see that this technology had vast potential, in terms of being able to replace a significant amount of high pressure. He recognized that Carbide didn't have the financial resources to put in all this investment in these reactors. So he spearheaded a major licensing effort at Union Carbide, to license this technology. The first licensee for this was Exxon, which was very significant. Subsequently, there was Mobil.

Today, there are licensees all over the world. There are many, many reactors. It is the major process in the world today for making linear low density polyethylene. The process is licensed in all parts of the world. The way I look at it is, the sun never sets on these reactors. [laughter] We were in Australia. Today we're in China, Japan, Saudi Arabia, Russia, South America, Canada, and a number of places in Europe.

The advantage of this was, this process has such improved economics. It's easy to run. It's environmentally friendly: there are no solvents, and it runs under very mild conditions. Instead of twenty to thirty thousand PSI, we're running this at one hundred to two hundred PSI. Instead of two hundred fifty to three hundred degrees, we're running this at eighty to ninety degrees. There's about twenty billion pounds of this—somewhere in that vicinity—being produced around the world today.

There's been a huge development since the discoveries in 1977 that we could make these products. The market share, if you look at high pressure polyethylene, hasn't grown much at all—it's shrunk a bit. But in the United States today, the market share of linear low density polyethylene is nearly fifty percent of the polyethylene market.

The detailed structure of these products that we made had some advantages versus high pressure, but there are still some improvements we can make to capture more and more markets. So there's a fair amount of effort to develop and improve on this process. I guess it almost starts from way back with the original discoveries; our continuous involvement in this area is for a lot of years.

It's involved a significant management commitment, at the top, to let us go and do the things we wanted to do. There's no way we're going to do this without support. There's been some unusual leadership, particularly with Bill Joyce in 1977. I don't know whether I mentioned it to you, but about a year and a half ago, he got the National Medal of Technology for business leadership.

I've been involved for a lot of years in this area. I look at several things. One is training people how to do catalysis work: how to interface, discover new systems, learn how to translate them as quickly as possible. I think our cycle is for evolving technology from the lab—process, product, going around that cycle, identifying what's important, bringing it to management's attention, and a receptivity from management to do that. I think it's the unique combination of things that come together for these kind of things.

In all the pieces, there's lots of challenging technical features to this kind of thing. Certainly we could talk forever on that. But that's a general flow of what's occurred. I might mention that a recent extension of this technology has been the ability for us to take this technology, and not only make these types of materials, but also now enter into the rubber area, making rubber such as EPR and EPDM. If you look at the significant challenges of materials, such as sticky types of material with lots of the agglomeration issues, there was a belief for a

long time that that couldn't occur, but that is an extension of this technology that we're now pursuing, and it looks promising.

BOHNING: There's an interesting chart here.

KAROL: Yes, it is. [indicates chart]

BOHNING: This is Figure 29 in your ACS Award address (1).

KAROL: It's very good. It captures a lot.

BOHNING: It summarizes things in a very interesting manner. It's almost as if you're using a catalyst to dial in the set of properties you want.

KAROL: Right. The molecular configurations, we think we know as well as anybody today how to dial these in. What's needed when we make high density polyethylene? What are the kinds of structures we need? What are the catalyst requirements? It's an entree into linear low density polyethylene where, in essence, what we're doing is we're putting in more branching. Branching means that we add an alpha olefin—such as butene or hexene—to the system. There's a linear low density area. Then there's a flexomer in the area. As we drive the density down, we get materials that have unusual flexibility, used in hosing or tubing, things of that nature, where flexibility is particularly good.

Then what we did was, while we were working in this area, we wanted to get involved in polypropylene by a gas phase. We'd made high density and linear low, so we wanted to get into polypropylene. Since we hadn't been in polypropylene—but we knew we had a process that would make attractive polypropylene in a gas phase—we had a collaborative effort with Shell Chemical, joining our resources in this area. They would provide a certain perspective in certain areas. We formed a joint venture and demonstrated that polypropylene could be produced.

Originally, Shell brought the catalyst and we brought the process. Then we jointly worked on an improved catalytic system that made a wider spectrum of polypropylenes. There's a unit that makes around two hundred million pounds of polypropylene down at Seadrift. We elected to license this technology as well. Just like we have some licensees in high density and some in linear low, there's another licensing program that deals with producing polypropylene.

So here we were at this end of the spectrum. Then we felt that we ought to learn the requirements to make EPR and EPDM. We were at the challenges issue from a process point of view—how did we run a viable process, when a resin is intrinsically sticky at a low temperature? A unit is in the process of being built, sometime for 1996, where we think we'll have attractive products for EPR and EPDM. A diene is used to provide cross-linking, which is important in a lot of rubber applications.

We see ourselves as having the capability to do this. We found ways to adjust the process. Then we've dialed in with the catalyst. I'd like to say a little more about the catalyst. There's a set of requirements for the catalysts that we have to meet. The first one is, it's got to be high productivity. All the catalysts have to be high productivity. In the polyethylene area and the polypropylene area, as I said earlier, that's down to a few parts per million or less of a transition metal. Transition metals are things like titanium, chromium, and sometimes vanadium. Second, we have to make the right molecular weight. So we have to have—dialed in the catalyst—the capability of adjusting molecular weight. The catalyst also has to make the right kind of molecular weight distribution.

Some applications for polyethylene and polypropylene demand a narrow molecular weight distribution. Other applications demand a broadened molecular weight distribution. Generally, the nature of the catalysts that you produce provides the capability of making narrow or broad distribution, and we have the technologies to do those.

The catalyst also has to be able to introduce a comonomer, because frequently you need catalysts that put in branching like butene and hexene, and various catalysts, depending on their structure, can determine how much branching you can put into the polymer. Some catalysts are good at it, some are not as good. We know what factors control that.

The catalyst morphology is important. Typically, you have catalyst particles that might be around thirty to fifty microns. We have a catalytic species put on a substrate. The substrates that people use are substrates like silica. There's a way of formulating the catalysts on silica. These particles are around thirty to forty microns, during the polymerization converting to a polymer. The polymer particle is five hundred to one thousand microns in size. That's important, because we're fluidizing this bed in a gas phase fluidized reactor. The size of this particle and shape are very important, because they influence the dynamics of fluidization. So it's not a trivial factor. Usually, the polymer particle is fifteen to twenty times larger than the catalyst particle.

In addition, the catalyst particle at the end has to be fragmented. During the process, we literally blow apart the catalyst particles. Polymerization takes place in the particles. Then we fracture these thirty-micron particles into fragments less than one micron. Now, that's very important for us, because if these catalyst particles didn't fragment, first, they wouldn't be very active. Second, these fragments would appear in the polymer as discrete gels—you could visually see them.

So there's a whole technology of using the right supports to get this factor to blow these particles apart. There are literally many, many man-years that go into the science of doing this kind of thing. That's very important in order to be competitive.

The end result is, you need to do all those things—high productivity, molecular weight, molecular weight distribution, morphology—and you need to do it with catalysts that are not very expensive. If the catalyst were to contribute a few cents per pound to the cost of polyethylene, it is too expensive. You have to be down around a penny or less per pound of polyethylene. So we had to do all of those things with something that was not only simple, but reproducible. In catalyst systems, sometimes they want to misbehave on you, so we have to learn to be conscious of the economics with this kind of thing, at the same time. That's important.

BOHNING: When you started doing catalysis work in the 1960s, how much of it was on a firm scientific basis? How much of it was more the art of screening large numbers of materials to see if they would work, to get the properties you wanted in the polymer?

KAROL: This whole area was so new then. We had to go back to the excitement of the original discoveries—a surprise to everyone that any of this change even occurred at room temperature and atmospheric pressure. We didn't have any background on that. So there was a huge amount of just running polymerizations, playing it out, to see where this was applicable. Out of that, there started to emerge a picture of what was important. We did a lot of kinetic studies; we did a lot of analytical studies to try to understand what were the fundamental principles and mechanism of polymerization.

The mechanism of polymerization is still debated today when people hold symposiums. I think there has emerged through all of this period of time an understanding of some of the principles of what's going on. This technology is called coordination polymerization. It was recognized that certain oxidation states of a metal were important. A lot of this came out of empirical work. Then we had, starting to emerge, some organometallic chemistry to support these principles. So it was a matter of proposing, "This is what I think is going on," based on a lot of empirical information, checking that mechanism out with various studies, and building an evolving model.

Looking at where the state of the art was in 1994-1995, and where some of the newer systems are going, there has emerged a lot of computational chemistry. There was lots of computer modeling going on around the site. There was a feeling that a lot of the things done empirically through the organometallic chemistry are much better understood today. Clearly, there is a bank of technology.

A lot of the catalysts—in fact, the majority of them—are heterogeneous. There was a reluctance for a lot of academic people to learn about heterogeneous catalysts as opposed to homogeneous. I think what has evolved over a period of time is a lot of support of the organometallic chemistry that provides an interpretive perspective on a lot of what is going on.

Clearly, when we were doing this in the 1960s, there was a lot of empirical work. I think the thing we did was, we said, “This is our model. Let’s check this out against some things and have that guide our experiments, and refine the model.” It was the philosophical approach that we took during that time.

[END OF TAPE, SIDE 2]

BOHNING: Again, going back to the early days, how did you go about testing the catalysts?

KAROL: How? In credibility?

BOHNING: Yes, its effectiveness.

KAROL: We have one-liter laboratory reactors here. Typically, we’d use something like hexane as a diluent. We would prepare the catalysts and react them in this one-liter autoclave at a few hundred pounds pressure, at around eighty to one hundred degrees. We would make one hundred to two hundred grams of polymer.

What we would do is, we would screen a lot of reaction—variable effects of supports, catalysts loading, promoters. We had certain property indicators we would look at. There’s a term, melt index, which is an indication of molecular weight. We’d measure the melt index under several different conditions, and we’d get a feel for what the molecular weight distribution was. We’d measure the density.

The key thing we were looking for was, what kind of molecular weight distribution did it make? What kind of melt index did it make? Was it highly active? The battle that took place in the laboratory—a significant one—was impurities. We were dealing with ethylene and trace amounts of catalyst. Frequently, we’d make runs and nothing would happen. We’d make a run with a trace more catalyst, and the thing would roar away on us and we’d agglomerate.

One of the findings in this area was that the impurities are critical. In production facilities today, as a result of a lot of this work, we have rigorously controlled feedstream purity.

The levels of impurities are down to parts per million. So there's a constant battle in the laboratory during runs.

When we had something credible—which meant we thought it was fairly active in the laboratory and it looked as though it met some of the needs—we would take it to our pilot plant. We would produce large quantities of catalyst. A pilot plant, to us, meant that we were making twenty to fifty or sixty pounds an hour of polymer in a reactor. We started off with just one little reactor. Today we have seven or eight pilot plant reactors that run around the clock, almost every day of the year.

We would test under steady-state conditions, control conditions, and make pounds of material. We would make on the order of one hundred pounds of material, and we'd study the process features of the polymerization. The facility we were doing this in was Charleston, West Virginia. Typically the chemists would go and work with the process engineers. The product would come back here to Bound Brook, which is where our product testing is, and we do product evaluation. They would say, "Yes, some things are good, but some things aren't good;" or, "Nothing's good and bad, you need us to go back and go through this process cycle again."

One thing I found, which I think was important, is our philosophy that certain things you could learn in the laboratory, but you need to go to the biggest scale unit to learn what other problems there were. The point I'm trying to make is, you want to work on the right problems; but the right problems, in a lot of cases, aren't all that obvious, based on just plain laboratory work. Over the years, we've learned that some things are better done in the pilot plant reactor—studying factors such as agglomeration, operability and so forth.

So our philosophy is to move things quickly through the units to establish just how the overall operations perform. We started off with just a couple of leftover autoclaves in the laboratory. Now we have more than twenty batch reactors here, and we have our pilot plants down in West Virginia. We also have catalyst manufacturing facilities here. If we want to make larger batches of catalysts, we can make them here and test them out in our laboratory to be sure they're okay. Then they can get shipped down to Charleston or to the plant at Seadrift, Texas.

A protocol has evolved out of this. But the main initial evaluating tool for a catalyst is these batch reactors. We use them both to support our pilot activities as well as to do basic research on new catalysts that we think would be promising in the future.

BOHNING: Your primary function was developing the catalysts. Is that correct?

KAROL: My responsibility was to define the catalysts, and to work collaboratively with the pilot plant and product people, to be sure the technology we were aware of was effectively done

in the pilot plant—also, that we understood what the deficiencies were in the product evaluations so that we could correct them through changes in the catalyst.

The key thing was this interplay. If we discovered something, and we waited for somebody else to ask us about it, we'd never get anywhere. So part of my role was to create awareness and to guide the technology as it went along—as well as to create the motivational environment today and perspective on what should be done. So many things in catalysis could be studied that we had to be selective, doing things that had relevant meaning.

BOHNING: I know of several examples in which once the basic discovery was made, the individual responsible for it was no longer involved. It fell to a whole new team to work up the technology in going from bench scale to commercialization.

KAROL: There's an overlap. Let's take a situation. We have a catalyst here that we want to scale up in the pilot plant. What do we do? We get together with the pilot people. We talk to them about this, and tell them what we've observed. Then we have one or two of our people participate in pilot trials where the run is done by the pilot plant people, but with the perspective of our knowledge.

We have to now find out whether the product has any value. The catalyst people are involved to the extent that they need to understand what the deficiencies are, because the product people might say, "This is not right for this or that." There's an iteration between the product people and process. There's also an iteration between the product people and the catalyst people, so that they can go and change things around. That is an important iteration for the success of our development. If each of these units were working independently, we wouldn't get anywhere. We look at it as a collaborative effort.

The business setting is such that this is actively encouraged. The idea of continuing—what I call rolling over resources—is built into our whole business strategy. When we moved the resources from high density into linear low, then moved them from linear low into polypropylene and then EPR, there was a whole support basis for doing this. That's where we are.

When people take things into the plant, there are run teams and technology managers. A whole variety of new people come into this kind of thing. But we don't abrogate our responsibility that, if there's a catalyst problem that exists in the plant, we shouldn't worry about it. We have run teams that do this under business sponsorship. There are business managers, operations managers, a whole folio. The technical responsibility—when we hand something to the pilot plant—doesn't end.

BOHNING: I'm wondering if we could go back a little, to get some dates and times in a framework. I'm also not quite sure about some of the places you mentioned. But I have one question about Boston University, if I could go back that far. Was Newman there when you were there?

KAROL: There's a building named after him.

BOHNING: He was involved in the history of chemistry. One of the rooms there had a history of chemistry collection.

KAROL: Yes. The name I recall.

BOHNING: As an aside, he was very heavily involved in the history of chemistry and had quite a collection then. We're trying to determine what's happened to it.

KAROL: I was on the alumni board in the chemistry department. The chairman of the department, at least a year or so ago, was Guilford Jones.

BOHNING: Oh, yes.

KAROL: He rediscovered me, in a way. When I won the Perkin Medal, he asked me to come to Boston University and talk with them. I gave lectures and ended up renewing some acquaintances from way back. Arno Heyn was there. I don't know whether you know Arno. He was in the analytical end; he'd been there a lot of years. Also Gil Jones and Norman Lichtin. So I have renewed acquaintances up there, but I think it's through the library that I know the name, Newman.

BOHNING: Apparently, much of it was put in boxes someplace. We're not sure what's happened beyond that.

Did they have a history of chemistry course when you were there?

KAROL: I don't recall that.

BOHNING: Okay. You had two papers published with Wayne [L.] Carrick when you were at MIT (2). I had assumed those came out of the work you had done in that first three-year period at Union Carbide. Also, you indicated that Carrick was another influential person in your life.

KAROL: Yes. He was my first supervisor. I had just come out of the service. I had graduated from Boston University. It was the beginning of Ziegler-Natta catalysis. He gave me an initial assignment to look at something. I got fascinated with copolymerization, the capabilities of that. So I worked in studying copolymerization and studying a lot of the rules of the game in that regard. Wayne was very supportive of the work we were doing at that time.

We were very interested in what made these catalysts work, what were the factors? A lot of the publications focused on methods to probe the nature of the active site. It was a very stimulating environment. It was an interesting thing; it was an old, dilapidated building, really not much of a facility. No air conditioning, two- to three-man laboratories, not very much room, no elaborate cafeteria or anything like that. But there was an environment of excitement because there was really an emergence of organometallic chemistry. That's what drove a number of us to work. We thought we were finding out a lot of things. It looked at that time as though we might even have had a catalyst of our own.

For me, just starting, it was a nice environment. I had a lot of freedom to do some things and work very hard. I remember coming in evenings and working. Wayne was there, and we'd have discussions. So it was a very stimulating environment. I'm certain that's the reason I came back to Carbide, because I got better offers at other places. He and I worked well together.

BOHNING: It almost sounds like an academic environment.

KAROL: It had a lot of that. We weren't worrying very much about the business end. It was more, "Gee, this is interesting stuff. Let's do it." [laughter] It was just very open at that time.

Carbide was already in high pressure polyethylene, from way back. Anything we were doing in polyethylene was okay. The company had a lot of money; things were going well. We had freedom to do research. We never dwelt much on budgets, as I recall. At least, I wasn't exposed to much of that. We were pretty excited about what we were learning.

I hated to leave. I was doing this work, and I thought it would be very productive. I decided to leave in 1959 to go back to graduate school. I hated to leave the research area. Then when I came back, I was asked whether I wanted to work in several other areas perceived by management, perhaps, as more important than what I'd been doing before. I said, "No, I don't want to do that." I figured I'd get back to my first job. Again, a relationship did that.

BOHNING: In 1972, you had two more papers on chromocene catalysts (3). I wanted to ask about the attitude of Carbide towards publishing papers at that time. Did that attitude change in later years?

KAROL: Well, there's the whole issue of proprietary technology. We had developed the chromocene catalysts. We had received some U.S. patents on them. We thought we had some very important things to say about them. What we did was, we wrote the things up, submitted them, and gave justification for why we wanted to publish. It was received favorably.

If something is protected with patents, and it looks like an important contribution—to go through putting it together and submitting it, I think a lot of times people don't want to go through that effort. But if you go through that effort and people can see what the document is, as opposed to some vague proposal, it helps. I found it's a fantastic learning tool for me personally to do that, because I find out what we really know. Even if it didn't get published, I've had an important learning experience in that regard.

I think part of my role has been to be a spokesman for technology at Union Carbide of the catalysis that we're very proud of. This the most important arena in all of Union Carbide. We could license the technology. It's important for people involved in the technology to represent these. It's good for licensing; it's good for the individual personally.

So, my philosophy again regarding this is, there are various attitudes about, "Should we or shouldn't we publish?" What I have done is, when I think there's something that ought to be said, I put it together and submit it. I'd say, "This went okay." This is covered under these things. We go through a dialogue as to how much proprietary stuff is in there. I, over the years, have given a significant number of presentations around the world at various meetings.

I think it's a good thing to do. But it's a privilege I don't want to abuse, in the sense of getting into situations where we disclose it prematurely. That was the attitude in 1972. "Here are some catalyst results. We think it's good; we're proud of it. Here, this is what the papers are."

Most of my writing of papers I never do here at work, anyway. I do it at home. There are long, sustained periods of quiet time. This is the only way I can write a paper. I don't like to spend a half hour on it. I'd never get anything done or get into a train of thought. So that's been my attitude: put it together, let somebody take a look at it.

BOHNING: You've followed the research ladder at Carbide. I have a few questions about that. However, before I move to that, did you ever think of moving into the management side of the company?

KAROL: Yes, sure. I looked at that. I was concerned that I might get removed from technology, which is, I think, an important strength I have. I've had a group working with me since 1968. It's varied in size. I've had a few people, as many as fifteen or so—Ph.D.s, something of that order. It varied from one time to another. So I'm not sure whether I'm a traditional scientist or a manager with a scientist's hat. But right now, a number of people work with me.

There's an administrative organization. Then there's a functional organization. I have a lot of interface with regard to the programs that go through the pilot plant, product, as well as some line responsibility. So I'm a scientist-manager or manager-scientist, depending on how you look at it. I'd always had, at least since 1968, people working with me—I don't know what you want to call it. As opposed to a manager doing budgets and personnel, without having a significant technical component, I've not been in that role. But I have had people working with me.

BOHNING: You started out simply as a chemist. Then you became, successively, a project scientist, a research scientist, and a group leader. Did each of those changes involve more responsibility?

KAROL: Yes. What happens is, there's a set of grade levels. The titles refer to a certain grade level with a certain aspect of responsibility. In general, as we progress up the ladder, our span of responsibility increases, and also the credibility we've established in the organization. So as we move up, the freedom to do things increases. So, yes—it's credibility; it's responsibility; it's independence as we go up the ladder.

BOHNING: In 1978, you were research associate and group supervisor. I would like to know what happened beyond that time.

KAROL: In 1980, I got promoted to corporate fellow. The top ranks in Carbide are corporate fellow and senior corporate fellow. A corporate fellow is equivalent to, say, an associate director. Then in 1984, I got promoted to the highest technical rank at Carbide, senior corporate fellow. That's equivalent to a major director.

BOHNING: Again, I would like to fix some dates in mind. The first pilot plant in Texas was in 1965.

KAROL: You're talking Texas, there's no pilot plant there. The pilot plants are in South Charleston.

BOHNING: That's right. The prototype reactor was 1968. That was in Texas.

KAROL: That's in Texas. That was the first Carbide fluid bed commercial reactor. In 1968.

BOHNING: Was that still primarily HDPE?

KAROL: It was just HDPE at that time.

BOHNING: Oh, just HDPE.

KAROL: In 1968, that was HDPE. In 1977 was our announcement of linear low density polyethylene. We were actually manufacturing it a few years before, but not the really preferred film grade. In the 1970s, that would have been. The dates that the public is aware of are 1968, then 1977 for linear low-density polyethylene.

BOHNING: The pilot plant for LLDPE was made operational in the time period 1972 to 1974.

KAROL: Yes, the pilot facilities.

BOHNING: The first commercial production occurred in 1975.

KAROL: Yes. Then the announcement was in 1977.

BOHNING: At the same time, in one of your papers, you discussed first-generation catalysts (4). Later in the same paper, you then discussed second- and third-generation catalysts. Were these first-generation catalysts the titanium-magnesium catalysts, or were they chromium?

KAROL: In 1970 to 1974 were the first-generation chromium-based catalysts.

BOHNING: Those were chromium.

KAROL: They were for low density polyethylene. In 1976 to 1980, the second generation for LLDPE were the titanium-based catalysts.

BOHNING: Where did the name UNIPOL come from? Union Carbide registered it as a trademark name.

KAROL: Yes. It came out of our headquarters. I did not coin that name, but it's the combination of Union Carbide with polyolefin. I got involved in naming all of the catalysts—we have our own way of describing them—but the description for this was done at headquarters.

[END OF TAPE, SIDE 3]

BOHNING: The original Zeigler-Natta catalysts were in the early 1950s.

KAROL: In 1953, 1954.

BOHNING: Was catalytic research going on in different petroleum companies in this country before that time?

KAROL: Clearly, there was not necessarily any research to make polyethylene. No one would believe it. But there was work going on in the very early 1950s at Standard Oil of Indiana and at Phillips Petroleum.

BOHNING: Okay.

KAROL: Phillips discovered a chromium oxide catalyst at about the same time, or maybe a little bit earlier than, Ziegler and Natta had discovered polyethylene—1953, 1954. The whole Ziegler-Natta area merged. Phillips concentrated on that catalyst and was the main licensor of the catalyst throughout the 1950s. We were a licensee of Phillips.

[J. Paul] Hogan and [Robert L.] Banks got the Perkin Medal a few years before we did, for their initial entree into this area back in that period of time. It was an impressive operation, in terms of being able to take a composition and then translate that into process. The process that they sold was a combination of a solution process and a slurry process, using their catalyst. In the United States, for a lot of years, the Phillips process was the dominant process for making polyethylene. In Europe, the Ziegler-Natta process was the major one.

Now, as time went on, we established our own identity, in terms of our own proprietary catalysts. One of the problems with the chromox [chromium oxide] catalyst, when you run it in a gas phase, is you couldn't make a whole range of products in terms of molecular weight, because the catalyst was one that basically did not have any chain transfer to hydrogen; so you couldn't control molecular weight. Phillips spent an incredible amount of time trying to understand this behavior—a phenomenal amount of effort.

One of the things we discovered was—if we went away from the chromium oxide catalyst and used our chromocene catalyst—by converting from one chromium compound to another, it allowed us to control molecular weight very easily, because our catalyst had a very good hydrogen response. It was one of the main reasons we were able to make a variety of products back in 1970. We would not have been able to run chromium oxide catalysts in the gas phase to make a whole range of products for high density, if we didn't, at that time, have a chromocene catalyst. So we were in a sense competing with what was going on with chromox. Phillips continued to study their system.

I began to recognize and have a greater appreciation that the way to change the capabilities of catalytic systems was not just to concentrate on one, but to look at a variety of systems with different ligand structures. This approach was a very important contribution back in 1968. One of the driving forces for why we published that paper in 1972 was because it showed that ligand design around catalysts is very important (5). Today this perspective is very important with other systems. There's a whole family of new catalysts where people are using the ligand design—what we were using with chromium. It was very important. It created a sense of departure for us.

Also, with the Phillips slurry process, when Phillips tried to make linear low density polyethylenes, the solvent gets to be a complication because it tends to swell the polymer, so you couldn't drive the density down too far. Although, as I said, Phillips was a very important contributor to early technology, from 1951 on, for many years.

Have you talked to Hogan and Banks?

BOHNING: I'm supposed to see them in late February (6). I have a trip planned to the Southwest.

KAROL: I think they're both retired. I know Hogan is retired.

BOHNING: As an aside, I've been at Phillips several times, and I know their archivist. Phillips has a very good archival collection of material.

KAROL: They seem to be conscious of history a bit.

BOHNING: Yes.

KAROL: I've been trying to capture that sense here at Carbide, on the history. My experience with the Phillips people was that they're very conscious of that.

BOHNING: Their corporate headquarters in Bartlesville has that. The second floor is totally a museum.

KAROL: Oh, really?

BOHNING: It's very, very nice. It captures their history all the way from the first filling station to the North Sea drilling platforms. They've done a very nice job. They have a beautiful photographic collection, and they have a full-time archivist who does nothing but maintain it. It's a good research facility. I've gotten nice documents on some World War II synthetic rubbers, another interest of mine. Their photograph collection is enormous.

KAROL: I was trying, a few years back, to take our reception area and use it to show some of Carbide's history. I thought we ought to make it more the kind of thing you were talking about. I think we need to do something in that regard. We're renovating some of the buildings, and we need to capture the sense of history, more than we have in the past.

BOHNING: The American Chemical Society runs what's called the National Historical Chemical Landmarks program. This is meant to be a grassroots movement on which nominations come in regarding collections, sites, and products. We recognize these as being historical chemical landmarks. We've put a plaque on the Chandler Laboratory at Lehigh and a plaque on the Priestley House at Northumberland.

Yesterday I was at Dupont's first nylon plant in Seaford, Delaware. They're talking about building a little museum at the plant site, open to the public, which would capture the original production of nylon in 1938. It's going to be very nice when it's done. A lot of companies don't do that.

KAROL: Have you noticed Leo [H.] Baekeland, who started things here many, many years ago—also a Perkin medalist, deceased. They put the Bakelite autoclave, which was his first reactor, in the Smithsonian Museum in a special new branch they have.

BOHNING: Yes. That was the first Historical Chemical Landmark, actually.

KAROL: Yes, yes. Right.

BOHNING: That was to get the program kicked off. Now we're working with the Sun people on the Houdry process. Part of the problem with an industrial process is that it's hard to know where to put the plaque. It's meant to be educational for the public, but most plant sites are not available to the public.

KAROL: Oh, I see. Certainly the UNIPOL reactor, in terms of the history of chemistry in polyolefins, is a major landmark. But again, it sits at a plant site.

BOHNING: To go back to that ACS talk (1), in table 18, you listed the factors that were present in the climate in the UNIPOL world. I'd like to list them and have you respond to them in some specific way.

KAROL: Yes. Frequently, we get caught up in all of the chemistry without an appreciation of the atmosphere. My intent was to try and capture some of that.

BOHNING: You did it very well.

KAROL: Here we are. [indicates table]

BOHNING: I was wondering if you could comment on some specifics for each of these. We've talked a little bit about "Leadership."

KAROL: In the context that I was looking at leadership, there was leadership at the very top of the organization relative to this. There was a plan. There was motivation provided by the leadership: "We're going to do this. We're going to give you the financial support for your R&D, and it's very important." I've mentioned to you two people in the very early days—John Luchsinger and then Bill Joyce, who has had a very significant impact not only in this area, but in all of Union Carbide.

But there was leadership at lower levels, guiding components of the program along the line. They created an enabling environment, a financial environment to do things. "We're going to spend the money and do it, and we're going to get the bureaucracy out of the way." When we needed something, we had an ad hoc group who made sure that, if there was something we needed, we'd get it without going through all the bureaucracy. That's what I meant.

You come to "Vision." We knew, way back, that the industry should move in this direction. We knew we had the lowest cost process. We had the licensing vision; we were going to be a major licensor of this technology around the world. We knew we wanted to do that. We set up an organization to do it, and then we basically did it. So it's a worldwide process, and UNIPOL is a word that's used and known by anyone in the industry. Coming with that goal, there was a commitment of dollars and resources to do it. It wasn't that we got it for three to six months. It was a longer term commitment to provide the funding to do it.

BOHNING: Was there ever any danger of the plug being pulled?

KAROL: In the early 1970s, when we were trying to establish credibility for the technology and we weren't being successful, we had one interesting situation. We were up for budget for the coming year. The question was, "What did you do in the past year?" I'm talking 1972 or so. We hadn't seen much happening. So I got together with a couple of people, because we had a very small group. I said, "We have to establish some credibility for our catalyst program. Even if it's not all finished, we're going to do something." I felt that there could have been a period

where, if we weren't going to demonstrate something soon, we might lose resources. They'd think this wasn't viable.

I think once we got beyond 1977, nobody was going to pull the plug. We knew we had what we wanted. Before that, we didn't. But we had what amounted to a very specialty business. After that, 1977, it was just a question of putting in the manpower to develop the technology.

BOHNING: Do you think that people like Joyce served to insulate you from those above them? You say they were the leaders, but they had to account to somebody else.

KAROL: I'm sure, for a period of time. There was always this question of whether we should be moving more towards specialties, as opposed to commodities. There was a mindset, "Are we ever going to make any money on polyethylene?" We used to have a ranking of businesses. For awhile, polyethylene wasn't at the top by any matter or means, because it was viewed as cyclical. You made money for one year, and then you lost money for a bunch of years, and then you made money. How can you run a business like that?

When we put the licensing business into our portfolio, we had a source of more steady income. Then we could ride the cycles, using licensing income. That changed the perspective on the polyolefin business. But yes, I think that if we hadn't done what we did with UNIPOL, we would not even be in polyolefins today. I think that a lot of the business aspects of this, relative to commitment—selling the board of directors alone—had to be very difficult.

Tom Tomfohrde, Joyce's boss, was also, I think, important in terms of being supportive and committed to the UNIPOL process and licensing. He was on the same wavelength that Joyce was, in terms of direction. We had people for licensing versus not licensing. "Why would you give this away to anybody? What will you gain by doing that?" There was a mindset that had to be changed. No doubt there was resistance.

BOHNING: At a pre-World War II styrene development project, Willard [H.] Dow said, "It was an example of patient money and a prayerful attitude" (7). That seems to sum up a lot of things.

KAROL: It came out so many different times, in a technical setting. You're at a point where you need to do something to demonstrate credibility, so you go take more risks than you might want, because you need to demonstrate a point. If you look at just the lab date that we had at the time we went and scaled up to the pilot plant, just looking at the raw data, there was minimal justification to do the run at all. What tended to happen was, in a gas phase setting, we got

improved performance over the laboratory. Now, it turned out that those improved performances were due to some factors that we hadn't recognized at the time. We kind of had luck.

I have at the bottom, the last item here, "Luck." We were working in an environment, and as a result of that, we wanted certain things to happen. We look back in hindsight and say, "If this had happened and that had happened in the reverse way, we may not have had this." Even in a commercial setting, if we hadn't done things in a certain sequence, it's unlikely that we would have had the credibility to do this.

I guess luck comes to people who are out there, willing to take chances, and can do some things. But we've had several circumstances where we were very lucky, in just how things fell into play.

I guess I've covered this "Can-Do-Attitude." My feeling, and the feeling of the people involved was, "Yes, we can do this. It's a lot of work, but it can be done."

We certainly were focused. When you come up with a new area, there's about twenty different ways you can go. I think it was important, at all levels, to focus in on exactly what we wanted to achieve. We put the resources on it; we moved the resources. Here's a whole existing business of high pressure polyethylene. We took people out of high pressure and put them into low pressure—even at the risk that maybe this business might not run quite as well. We said, "If there's an emergency, we'll have them there. But we're moving out of that area, so let's move these people." A number of the people who ended up working in the low pressure area had worked with the old technology before.

I think we had some good people, outstanding people. There was a lot of creativity in terms of people being allowed to use their imaginations. As in any large industrial organization, there was a lot of teamwork going on.

Everyone wanted to make the thing work. It's fun to be in a situation where you're going to change the industry around. This industry is so huge. We're not dealing with fifty or one hundred pounds, we're dealing with billions of pounds of materials—the world's largest thermoplastic—and we're making it by an entirely new process. It's kind of different. You had a sense that there's some history; you'll remember this when you get old.

I understand, in some of the courses in business technology at some of the big business schools, UNIPOL is cited as an example of new technology.

The fellow at Kinsey Associates, Foster, used to give seminars about technology development. His attitude was that generally, when there's new technology coming out, it comes out by someone else attacking the people trying to preserve it. It was an interesting case.

We were attacking our own technology, which seems to be a little bit different than the normal situation.

BOHNING: You changed the whole economics situation at Union Carbide so drastically.

KAROL: Oh, yes, very significant—just the cost of equipment to run under high pressure conditions, and the maintenance. A number of the products, they're better. Some products were still better by the high pressure route. We are still evolving with low pressure technology. We think that, as time goes on, some of the remaining high pressure products will give way as well—it will be more and more low pressure. I don't know how much construction of high pressure equipment has taken place since 1977, but it isn't very much.

That covers the list, I guess.

[END OF TAPE, SIDE 4]

BOHNING: I want to look at table 19 (1). But before we do that, I want to turn to the whole concept of teamwork. Some people prefer to be loners working in a research environment. How do you identify team players?

KAROL: When I have new people come in, I make a judgment just from talking to them: "This person would be good in doing research in the laboratory." Then I had a fellow come in, looking back some years ago, where I said, "He would be very good in an interface role. We'll get him involved in our pilot scale activities in the plant." So in my own case, I make some judgments by seeing persons. We try them out and see how well they work.

As far as the loners are concerned, if a person functions in that category, that's fine. I want them to have an appreciation for the fact that just because they're doing research in the laboratory, doesn't mean that their research is a thousand times more important than anything else. There's a mindset. You need to understand what other people are doing. Even though you're doing research alone—being an individual contributor—you ought to understand the context of the work you're doing, because ultimately, the work you're going to do must have some impact in the organization. You've got to understand what the rest of the organization is about.

Some people do better doing laboratory work, although a lot of people like the interfaces that occur—particularly if they've got their own thing they're doing. They want to carry it with them so that everybody does it right. [laughter] We don't have much of a problem with people

interfacing with each other. Now, some people do it better than others, and some people are more conscious of this.

It's built into what we're doing. We've operated in such a way that when people come in, they just get involved in the culture. If a person is a loner and works alone, that's okay—provided he has appropriate perspective as to the context of the work.

BOHNING: Let's go to table 19. Here are some other generalities about the UNIPOL process. The first phrase is, "The Wedding of Many Technologies."

KAROL: Catalysts, process, fabrication, stabilization—all of these things need to be integrated. UNIPOL is a combination of disciplines: the purification of the monomer, the control of reaction rates, the construction of the facilities, the ways of making catalysts. There's a whole string of pieces, all of which must work together in order to make the product. An incredible amount of work went on among engineering, manufacturing, R&D. If we don't talk to each other, there's no way we're going to get this thing to go—that's what that means.

With many inventions here, we set out to cover the aspects of the catalyst, the aspects of its manufacture, the operation of the fluid bed. We have patents and know-how on a lot of these aspects. The bottom one, "Important Contributions By Many Investigators," is a lot of creative people doing things. You always run into the issue of individual recognition of visible group efforts, in terms of the balance of this.

I wanted to acknowledge that here I am, I'm given this award by the ACS. I didn't want to leave the message that, "I'm doing all of this by myself," because that is not the case. I spent a little time emphasizing that from the management point of view, we give awards to an individual—but in an industrial context, this is a group effort. That's what that was about.

BOHNING: Whose names were on some of the crucial catalyst patents? Was it your name and many others, or just yours alone?

KAROL: It's a spectrum. If you look at the key patents, there are some with me and maybe one other person, some with five or six names on them, and some where I've just got my name on the patent. The names are scrambled. Some of them, another person's name is first. There seems to be concern sometimes about first names on patents. We scramble them. We tried alphabetical for a while, and we've ended up with just a whole spectrum of different approaches. I told the attorneys—in a lot of cases I gave them guidance as to how to put the names—that this was not created as a one-man show. We've reversed names on many of the patents.

So the answer is that some have just one or two, or they could be five or six—particularly for ones that involved the interface between the laboratory and the pilot plant, where we contributed a piece and the pilot plant contributed a piece, and there was lots of discussion going on. In those cases there could be a number of people, because each one is contributing some aspect of it. It varies.

BOHNING: What was the approximate size of the UNIPOL team?

KAROL: At what time?

BOHNING: At the time when it was ready to go in 1977.

KAROL: Well, let's go back a little bit before that. In 1973 and 1974, the catalyst effort involved one fellow and me in the laboratory. There was maybe one process person and a supervisor down in Charleston, and maybe a couple of product people. We're talking anywhere from two to six people, depending on what particular year, 1973 or 1974. But it was just one person in the lab. From that effort to where we are today, where we have lots and lots of people—I don't have a count, but it's certainly huge compared to where we started off in 1973, 1974.

We looked for new catalysts because we felt that was key, to get to these new materials. We did that in 1975, 1976. We still weren't sure whether they were going to work until we went to the pilot plant. In 1976, I guess it was, when we made those runs and evaluated the product—we knew as soon as we saw the product that we had what we wanted—the effort went up very rapidly, just a few people to a huge effort. We have hundreds of people in R&D today who associate, one way or another, with UNIPOL. It was a big, big change.

BOHNING: Would you say that UNIPOL is the business that keeps the company afloat?

KAROL: Probably, yes. I think it's the most significant business in Union Carbide today. It certainly is a worldwide business. We have a number of joint ventures—one that we're just trying to complete now with Enichem in Italy, and one with Atochem in France. We have a huge complex in Kuwait. The intent there is to put an ethylene cracker in to get the ethylene, then to use that ethylene in the UNIPOL process for polyethylene, as well as to make ethylene glycol. Glycol and polyethylene are two large businesses. Polyethylene is doing very well in licensing.

One licensee has large facilities in Saudi Arabia. We're having an entree into Europe now. In Union Carbide, we went—at the same time—from almost one hundred twenty thousand employees down to less than twelve thousand employees, so the whole nature of the company has changed. Lots of business areas that Carbide was in, they've been sold or spun off. Praxair was spun off. Some of them were sold. We've gone out of some businesses. So we're truly a chemicals and plastics company now. In the past, we were involved in all kinds of things—carbon products and consumer products. UNIPOL technology is a major business today for Union Carbide.

It's a huge change. It's the reason why we want to have credible technology where we have the leading technology. We certainly believe in this area. We're going to do whatever we can to continue to reinforce that.

BOHNING: The next thing I'd like to look at is your Perkin address, which you've entitled, "The Roots of Innovation" (8). You've already talked about how you've defined innovation. You said it depends on teamwork, and we've already talked about teamwork. You also said that innovation depends on a strong marketing network.

KAROL: We'd established that we had a process that could make a product. These products were different from high pressure ones. They happened to be better in a number of applications. But we had to educate people that different wasn't a deficiency but an asset. These materials fabricated somewhat differently from high pressure material. We needed to convince people that changing the extrusion behavior would be a benefit to them. So we addressed the status quo of, "This is what we have today. Why are you coming in with something like this?"

How do you educate people, as you create what we call a polyethylene revolution? Since it's made by a different process—even though it's an improvement—the public had to be receptive. I think that's characteristic when you come up with something different. The initial reaction is, "Who needs you? You're creating problems for me," as opposed to an opportunity. That's what I meant by the marketing aspect.

BOHNING: Another aspect is related to that: learning to listen to what the customer wants. I think the chemical industry has changed a lot from the old days, when their attitude was, "We are making all this chlorine, come and get it." Now I think they realize that they have to develop something for the customer's needs, instead of finding a customer to buy their products.

KAROL: I think the whole industry has changed, in terms of customer focus and understanding what are they trying to tell us—to listen to that, as you have mentioned. I thought it was important to capture that aspect in the presentation.

BOHNING: You also discussed your ideas about innovation being motivated by what you called fear, or competition.

KAROL: In this industry, there are a lot of players. Everybody's working to develop something new and different. The last thing you can do is be complacent—assume you're sitting at the peak. In fact, if you think you're at the peak, you probably ought to be running as hard as all, because there's a bunch of guys who have you targeted to knock you off the peak. [laughter]

So it's an important concept, to recognize that you're not alone in the world. People are going to compete with you. If you stand still, you won't do anything. You want to get out there. When you think things are going well, you probably ought to run with as much fear as any, as opposed to polishing the sails on the sailing ships. No one's going to have a better idea—and then you find out somebody who's got something entirely different.

BOHNING: As part of that, the increase in competition comes from both World War II activities and now global industry—as opposed to industry concentrated in a few countries.

KAROL: Recently, I had an opportunity to be in a number of different countries—Europe, Asia, Japan, China. There are big things happening there, the competition you see emerging. I was struck by China, seeing the amount of construction going on. The people seem to be very active in terms of motivation, from what I saw. We have to recognize what's happening in those countries.

We have a number of licensee projects in China today. They're building new UNIPOL process plants. But understanding the people, the culture, what they've trying to do—how do you adjust to their desire to be fully competitive? It's very different. Whole different kinds of people need to be out there understanding that—not only from a management point of view, but what about the technology things that are happening there?

When we were putting units over there, we were struck by the dedication some of the people were putting into getting those units built up and running quickly. Many of them did a very fine job. But as time goes on, they'll be competition. There are lots of things happening around the world.

BOHNING: You said there's another side of the coin. In addition to motivation by fear, another motivation was the fact that it was fun.

KAROL: Oh, yes. I think people, when they're working on something they think the company cares about, are willing to put resources in. You can take your own approach and tinker with the whole business arena. People like that. The discouraging thing is when you've started working on a project, and there's no funding six months later—just as you think you're getting started. The threat of pulling the plug within six months is not as much fun as working in an area where you're doing something that's important, management cares about it, and it's scientifically interesting. It's a big combination. People like that. It's fun playing around with that.

BOHNING: As long as it's fun for the people who are doing it, you get a lot more productivity at the same time.

KAROL: Yes. It's a fragile thing. You need to be sure that those individuals who are involved in the specifics are having the same kind of fun you're having. [laughter] That's something you have to watch out for.

I think climate, atmosphere, and an enabling environment are critical. Monitoring just what that environment is, is important. You think, "I created this environment." Then you find that it isn't really that environment. [laughter] It's a touchy issue. It affects motivation. It needs to be monitored. It's a very intense level of motivation, this work. We've gone into phases where we've consolidated our gains before we took the next plateau. So we go through plateaus of demonstrating, consolidating, reinventing.

BOHNING: You listed a few factors that would help you identify potential innovators. One of these was people who were in a hurry.

KAROL: Yes, a sense of urgency. I think that's important. There are so many things we could do. We could study something forever with the sense that, "If I do this and maybe publish a number of papers in this area—if I get it done this week or next week, it doesn't matter." We're in a race with other countries, but they're not taking that attitude. They're out there, they want to make something happen. So we have to have a sense of urgency. Time is very important, between first identifying a major discovery, then taking advantage of it. We ought to treat it as urgent. Why does it take seven to ten years to commercialize something? Why can't we do it sooner? It creates a sense of urgency.

BOHNING: Another factor you listed in identifying potential innovators was an independent spirit.

KAROL: Just because I go to somebody and say, “You want to do this? You might want to look at it this way,” doesn’t mean they have to do it that way to get an answer. Frequently, people say, “He didn’t know how to get there. I know how to do it.” We’ve got to create that atmosphere so that we all have the same objective, but we know there are many pathways to creating something. The idea is, “You’re smart enough to figure out some other things.”

You want to be sure they have the opportunity to look at some other things. If we say, “We believe those things have better ways than what you thought about,” they have longer to think about it. That’s what that is about—as opposed to saying, “I’ll do it. How do you want me to do it?”

BOHNING: The last one was something I liked. You said that potential innovators see difficulty as a challenge, not as a roadblock.

KAROL: Yes. I think it speaks for itself. Good people like significant challenges, and they want to go and do it. They look at obstacles as something to push out of the way—as opposed to, “This is too difficult; I don’t want to get involved in this.”

In terms of people’s accomplishments, people can list a lot of things they’ve done at the end of the year. The question we’re always faced with is, “Was it very difficult?” We can get somebody who’s listed ten accomplishments during the year, and we say, “Why did it take you so long to do those ten?” This other guy has overcome only one, but that one was so significant compared to the others. That’s important in looking at people. What are we asking them to do? Are we giving them an easy job, or is it something really significant?

BOHNING: You also said that innovation can be messy and disorderly.

KAROL: There’s no structure in the early stages of this. My people are groping: they have some ideas, they want to do it this way. They try it; it doesn’t work. A couple of other guys think they have better ways to do it. They need to probe it. So we go through a period where the initial demonstration of credibility involves the creative interplay of one or several people. To have a person fail once or twice—or not have rational explanations for everything he’s doing—is part of how this happens. There’s a risk in creating too much structure too early. That’s not the nature of research.

I worry a bit about that one, because I think there is frequently an attempt to put lots of structure and order into a process that’s inherently disorderly. I’ve seen it a number of times.

I've seen how it works. Preserving that perspective on being messy and disorderly, not for the sake of being messy and disorderly—it's just the nature of it. I'm not sure how much you want to systematize this kind of thing.

I constantly have interactions with my engineering friends who tend to be more structured and orderly than I might be. In fact, the reason we're here, rather than in my office, is that my office is disorderly. I don't have a lot of room for us to talk. I don't know whether that's a good sign or a bad sign.

There is a process here. The people who have made inventions are now trying to go through the innovation process. It's amorphous; it's squishy; and there's not much structure. But the thing that drives that is motivation. It's the perception that we need to get there. We can't endorse chaos, but I've said to all the people who worked with me, "Never let ignorance prevent you from making progress."

Sometimes we don't have all the answers—because we don't even know the right questions to ask—but we have to try this or that. Don't let the fact that you don't know everything about it disturb you from going to the next step, because, in the process of getting all the information, the parameter you're playing with is time. It can take you an awful lot of time to do all that, and there's a balance between having a certain amount of information.

My philosophy is not universally acceptable, but that's how I see the thing and how I've operated in the past, at least with some success.

[END OF TAPE, SIDE 5]

BOHNING: You might want to inject some additional comments. One of the other things you said about innovation was that it involves risk.

KAROL: If you're going to do something different, you're sticking your neck out to a certain extent. You don't know how things are going to happen. You're going to find that things didn't work out. You have to be able to accept that. If somebody asks you, "How come this isn't working? Why are you spending so much time?" you have to be prepared to do that.

Most people aren't risk takers, not really. You have to do that, to do something new and different. You're going to stub your toe. You have to be prepared to do that. If there's an enabling environment at the top of the organization that says, "This is important. We recognize that there's going to be risk associated with this project," it's okay. Otherwise, you're just making small incremental changes, and you always stay in the same box. If you want to get out

of the box, you have to take risks. Sometimes you get put in the penalty box, sometimes you don't—depending on what happens.

BOHNING: Essentially, you spent your entire career working on catalysts. Did you ever get tired of that? Did you ever wish that you could go off to a totally new area and start something else?

KAROL: That is true. But when you look at it from an R&D point of view, what we're doing in catalysis involves a lot of things. It involves the synthesis of exotic organometallics. It involves interfaces with a lot of different people. It involves worldwide interactions with people in this process, as a business. I interface with a lot of different people in a lot of different areas.

I don't look at it as, "You're working in catalysis, in a laboratory with the door closed every day." That's not what I do. I happen to play around a lot with catalysts, but the exposure I've gotten makes the job worthwhile.

BOHNING: Did you have any memorable frustrations, setbacks, or failures along the way?

KAROL: Oh, yes. You go back and try to establish credibility for these kind of things. A number of times we've done things and they haven't worked. There's been frustration along that line.

BOHNING: How have you dealt with that?

KAROL: My own dealing with this is to continue to plow ahead. People have made those comments—but at the same time, they haven't. We haven't gone the process of, "We need somebody else to do this." We're allowed to redeem ourselves.

There are always organizational changes that occur at any big organization. Some are good; some you think should not have occurred. You have to grapple with that over a period of time, in terms of what you would have expected. Always the biggest issue is where you don't feel you're in control of your own fate. That's the worst thing, to me, that can happen—not control your own fate.

Things occur in organizations where you feel you should be part of the process. Things occur where you're not part of the process and you have to take the outcome. They affect what you do. Those circumstances were always difficult for me to deal with. When I'm in control of

my own fate, I can overcome things. But in big organizations, there have always been situations where there's frustration. There're so many people involved, and so many different parts that impact on you, such as, "That's not the way that should be done, and you can't control it." That's bad. I think organizations need to grapple with that—as opposed to individuals with sensitivities of that type.

The human relations aspects always get to be part of any organization when you're there for a while—as to what you perceive as the best things to do, and what the organization perceives as the best things. Sometimes they don't match.

Some of those are difficult for me. What saved me is that I could get involved enough in the details of the work, that I could ride out some of those. If I hadn't had something that was challenging and interesting, that would have been more difficult. Those are always, always difficult ones with organization. How do we best do this? As organizations change and new people get involved, it's always a dinosaur complex. They say, "You never did it that way before. Why are we doing it that way now? It worked this way. What's your experience that says it's going to be any better?"

There are two parts to that. Just because they did it that way before, doesn't mean it's right now. On the other hand, part of the history in chemistry is, "How does an organization preserve what it knows are good things to do?" A way to operate, a way to think, a way to do things. How do you transfer that to people in an organization? Are they going to be receptive to that, because they see things differently. That's the big challenge in an organization—to preserve its creativity and identity as the organization changes. This could be a whole study in its own right.

BOHNING: I think personality has a lot to do with that. It's innate. If a company has the right mix of personalities, it can get tremendous things done. If it doesn't have the right mix, even if people are competent, it isn't as effective.

KAROL: Absolutely. Over time, I've seen that. I've seen how things have changed. I've been at Carbide a good many years, and I think I understand what makes things work to be effective. But—as I see other people coming in and looking at it differently—there's always tension that gets created. I have to balance that with, "Am I always right?"

BOHNING: That's a hard question to answer.

KAROL: My wife knows I'm not. [laughter]

BOHNING: We've covered almost everything on the agenda I sent you (9). I still have a few loose ends, however. One is, how much interaction did you have with peers outside of the company?

KAROL: I know most of the people who work in the area. I attend major international symposia, which involve academics. Also, for a number of years now, I've given a presentation on some aspect on UNIPOL technology.

We fund research at a number of universities, not only U.S. We gave some money this year to a fellow in southern Poland. I've been over there and visited with him. He's doing work I think is very interesting. The money we gave him was not a tremendous amount, but he had some funding. He will love it, and I'll have a good relationship.

I interface with people in Korea and all over the world. I paid a visit to Novosibirsk, which is in Siberia. There is a large catalysis laboratory at this location.

So my attitude is that it's very important. I try to go to meetings each year. Generally, they're international meetings. I think I know all of the major players in the world personally. I work at it: I do go there, and we do have discussions. In addition, the UNIPOL process creates an environment for people to come and talk, where they might not otherwise. It's a combination of what's happened and being present at the meetings. I've found it to be good, very informative.

BOHNING: What changes have there been at Carbide over the years you've been here? Have you seen any changes in the company's attitude towards supporting R&D, or towards R&D in general, in terms of their expectations of it?

KAROL: I can't speak for all of Carbide. Certainly, the support for our things has been fantastic. It really has been. Some years ago, we requested an increase in the number of people. We might have asked for two or three additional people. The question was, "How come you're not asking for five or six?" Unbelievable, is all I can say.

We were looking at getting some additional help in computational chemistry. I had a budget for this year. I already had the manpower in there. I said, "We'll look at one of these fellows, but if we hire them, we'll go over budget. Is that okay?" "Yes, that's okay."

It's truly a unique environment. A lot of the rules in textbooks don't even apply. I tell that to people. People say, "I really have to sell this project to my management. I have to be optimistic." We almost have a reverse situation. The management has already said that they're

going to accept it, so we'd better be sure that we're responsible in what we're recommending. It's interesting from what historically you might read, as to what's really happening in our area. It's supposed to be, "Why aren't you helping the business? Why aren't you doing more new things? Why aren't you requesting funds to do more of that? Where are the proposals?"

I think it's attributed to environment. Bill Joyce, in particular, is a unique individual who encourages a lot of that. He just touches base with the organization. He's certainly been a true supporter of R&D, to put funding—at times—even beyond what we might have requested. That's very good.

BOHNING: In your opinion, what is important for the future of R&D and the chemical industry?

KAROL: Certainly, the recognition—by management—that R&D is a vital link to the future is the key to the whole thing. Having a vision of where they want to go that they can convey to R&D. Where do you try to be ten years from now? What kind of businesses do they see themselves wanting to be in? Which areas do they want to be number one in? What's their strategy? There have been lots of discussions in the last few years about strategy. What is it? Does everyone have a strategy? Recognize that R&D requires certain kinds of tools, facilities, and capabilities, and be able to provide those things.

It's strategy. It's focus, vision, and trust in R&D, that they'll be able to do things for you—as opposed to, "We can never trust you guys to do anything." Everything flows on that. I think there are high levels of credibility—and then you go through areas of less than high credibility. That's the nature of what we're doing now. I think the management that recognizes that, and is willing to do that, is the management that's going to succeed.

Be sure that, as people leave organizations and new people come in, to have a conscious way of maintaining what things work well or not, we need to do some more things so that people understand we can change the way we do R&D. You need to recognize what did work and what doesn't work. You've got to avoid bureaucracy as much as you can. There are always attempts to systematize and organize. You can't be against that, but you have to recognize the environment.

I've been to Italy and made a lot of friends in some of the companies there. There's not a lot of organization that I can see over there. People are immensely creative in terms of things they come up with. Yet they've asked me what's the right way of doing things here.

Joint ventures teach you a lot, because you start working with other cultures. How do they do things? The challenge for us is, we're going into joint ventures with lots of people. We're going to be working with this company, that company. How do you take what's good in

what they do, and use it? How do you carry out joint research programs? That's a big challenge for us.

BOHNING: I have one last question. What did it mean to you to win the Perkin Medal?

KAROL: I've gotten a lot of awards over a period of time, but it's a special award, the Perkin Medal. First, the ceremony really is an individual ceremony. It's done with great class. I certainly appreciated it. It made me feel good, that this is an important award in the United States. It's the reason why I'm here. For the industry to say, "Yes. You're really special, in regard to what you've done"—it's about as good a feeling as you can have, working in the area we do. I have a very warm spot in my heart for this particular award.

BOHNING: Is there anything that I haven't covered that you'd like to add, at this point?

KAROL: Not that I can think of. I've rambled on. I'm not clear how this all gets consolidated.

I felt good. You like to feel you've accomplished something, somebody cares about it, you had an impact. I just appreciated the opportunity to be in an environment where we could do that and do something that for me is important, that somebody cares about the industry and the challenge.

Having an enormous challenge to try to go and do something, then seeing an organization and working as part of that organization and seeing it happen, is why I came to work in the first place. It's satisfying.

BOHNING: With that I'll thank you again, very much, for spending the morning with me. I've enjoyed it.

[END OF TAPE, SIDE 6]

[END OF INTERVIEW]

NOTES

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