CHEMICAL HERITAGE FOUNDATION

DELBERT H. MEYER

Transcript of an Interview
Conducted by

James G. Traynham

at

Naperville, Illinois

on

20 January 1997

(With subsequent corrections and additions)

ACKNOWLEDGEMENT

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Oral History Program

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

DELBERT H. MEYER

1926	Born in Maynard, Iowa, on 28 August

Education

1949	B.A., chemistry, Wartburg College
1953	Ph.D., chemistry, University of Iowa

Professional Experience

	Amoco Corporation/Amoco Chemical Company
1953-1961	Research Chemist, Standard Oil Company, Whiting, Indiana
1961-1967	Research Chemist, Amoco Chemical Company, Whiting, Indiana
1967-1977	Research Supervisor, Naperville, Illinois
1977-1989	Director, Exploratory Research Division, Naperville, Illinois
1989-1992	Research Consultant, Naperville, Illinois
1992	Retired

Honors

1983	Alumni Citation Award, Wartburg College
1989	William M. Burton Award, Amoco Chemical Company
1992	U.S. Medal of Technology
1993	Honorary D.Sc., Wartburg College
1995	Perkin Medal, Society of Chemical Industry (American Section)

ABSTRACT

Delbert Meyer begins his oral history with a description of his family life as a youth in Maynard, Iowa. He was uncertain of his future career choice and served for two years in the U. S. Navy. Influential professors at Wartburg College and later at the University of Iowa fueled his interest in chemistry. Meyer spent thirty-nine years with Amoco, beginning as an exploratory researcher for Standard Oil Company in 1953 and later becoming a research consultant at Amoco in 1992. During his career at Amoco Corporation, Meyer developed a faster and more economical method for producing purified terephthalic acid (PTA), the major material used to make polyester. He eventually moved into research management and product development. Meyer concluded with a discussion scientific innovation as a result of need for products in the marketplace; speculation on the future of research and development management in the chemical sciences; and reflections on winning the 1995 Perkin Medal.

INTERVIEWER

James G. Traynham is a Professor of Chemistry at Louisiana State University, Baton Rouge. He holds a Ph.D. in organic chemistry from Northwestern University. He joined Louisiana State University in 1963 and served as chemistry department chairperson from 1968 to 1973. He was chairman of the American Chemical Society's Division of the History of Chemistry in 1988 and is currently councilor of the Baton Rouge section of the American Chemical Society. He was a member of the American Chemical Society's Joint-Board Council on Chemistry and Public Affairs, as well as a member of the Society's Committees on Science, Chemical Education, and Organic Chemistry Nomenclature. He has written over ninety publications, including a book on organic nomenclature and a book on the history of organic chemistry.

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INTERVIEWER: James G. Traynham

LOCATION: Naperville, Illinois

DATE: 20 January 1997

TRAYNHAM: Dr. Meyer, I know that you were born August 28, 1926. Would you tell me something about your parents and your childhood?

MEYER: Let's see. My parents both had eighth-grade educations. I was born on a farm in northeast Iowa. The farm had no running water, no electricity, and we did our farming—through most of the years that I was with my parents—by horses.

TRAYNHAM: What were your father's and mother's names?

MEYER: My father's name was Henry, and my mother's name was Frieda.

TRAYNHAM: Had they always lived in Iowa, or had they moved here from somewhere else?

MEYER: They had always lived in Iowa.

TRAYNHAM: They must have given you considerable encouragement to proceed with education since, as you indicated, both of them only went through the eighth grade.

MEYER: Well, not really. As a matter of fact, when I was going through high school, I didn't intend to do anything but farming. Then, when I got out of high school, I was not quite seventeen yet, and I wanted to join the Navy. My parents didn't want me to join the Navy, so they said, "Why don't you go to college for a year before you do that?" That's how I got my start in college, and they did pay for my first year of college. I think the total bill was four hundred and fifty dollars for board, room, tuition, and all the fees, and spending money and everything.

TRAYNHAM: You say you went one year to college. Did you then go into the Navy?

MEYER: Yes, then I joined the Navy, spent two years in the Navy. When I got out of the Navy, I still had intended to farm, but by the time I got out—I was one of the last ones to be let out of the Navy—all the farms were either rented or sold by that time. I happened to be visiting my sister, who was in the same town that I'd gone to college in, and I went up there. The person in charge of recruiting students met me in the hall and said, "Aren't you coming back to college?" I said, "Well, it's too late to decide now, isn't it?" This was like the second week in August or something like that. He said, "Well, not if you've got a place to stay." I was told my sister this after I got back, and she said, "Well, you can stay with us." That's how I went back to college on the GI Bill.

TRAYNHAM: What was it in college that turned your interest from farming to chemistry?

MEYER: Well, when I went to college—you know, I hadn't really planned on going—I saw the high school superintendent and asked him, "What do you think I could do?" He looked at my records and said, "Well, one of the things you could do is engineering." My math background was strong in high school. I started out in the electrical engineering curriculum at Wartburg. When I got back from Service, I had to take chemistry. I'd never had any chemistry before, never even thought about it. I took this course in which Professor [A. W.] Swenson taught and he made the course very challenging, and I was really doing badly in it after the first six weeks. Then I finally caught on, caught up with all the kids who had had high school chemistry, and wound up getting an A for the semester.

Plus, it seemed like it was interesting. The way he taught it was to solve problems, and I enjoyed solving problems. I talked to him after that first semester and said, "Do you think I can still get a major in chemistry?" He said, "Yes, we'll work it out." That's how I got into chemistry.

TRAYNHAM: How large was your class?

MEYER: In high school?

TRAYNHAM: No, in that college chemistry class.

MEYER: Oh, I think there actually were two sections, so there were probably twenty-five to thirty students.

TRAYNHAM: You had the opportunity to have a good bit of interaction with the professor then?

MEYER: Oh, yes.

TRAYNHAM: Did you go to high school in Maynard?

MEYER: Yes.

TRAYNHAM: Do you feel that they gave you a good math preparation there?

MEYER: Yes, we had a particular teacher whom, as I look back on it, I wonder how he did all the things he did, because he was really the band instructor. He had the junior high band, the senior high band. He taught instruments to something like seventy or eighty students, and then he taught three math classes on top of that.

In our senior year, three of us decided we would like to have something a little more challenging in math than what they offered, so we went down and talked to the superintendent about getting geometry. He said, "Well, we'll set it up as a correspondence course, and then we'll just have Mr. Ellingson give you guidance in it." Well, the word got out, and we wound up with fifteen students, so Mr. Ellingson took on another class. His classes were a lot like his band. They would get noisy up until the point where he thought it was too loud, and then he'd just kind of say something, and they'd quiet back down. He made learning such a fun experience that I think you just couldn't help but do well. As a matter of fact, in that course in geometry, nobody got less than a C, and everybody really deserved it.

TRAYNHAM: It sounds as though you were unusually fortunate in having inspiring teachers, both in high school and at Wartburg College.

MEYER: Yes, I would say so. We also had a coach at Maynard who taught history, and of course that was right just during the period when World War II was starting up. We'd always start with maybe ten or fifteen minutes' discussion of the headlines of the day. He had a way of taking that and bringing it into his history class, and making you think about what history was about and so on. I think it was just very good. Then our school superintendent taught agriculture class. I don't know that I learned much about agriculture, but I learned a lot about the organization of thinking.

I had really some excellent teachers, even though Maynard was a very small school. I think we had sixteen teachers for the whole high school.

TRAYNHAM: You were, in effect, committed to a chemistry major after your first course in chemistry, is that correct?

MEYER: Oh, yes.

TRAYNHAM: You proceeded to major in chemistry. When did you make the decision to go to graduate school?

MEYER: Well, when I was getting my bachelor's degree, which was in 1949, there had been so many of the veterans who had gone to school on the GI Bill and who were graduating about the same time, so the field of chemistry was really full in industry. There just weren't many jobs available, so I decided, with the guidance of Professor Swenson, that I should go on and get a graduate degree in chemistry. That's how I made my decision to go on to graduate school.

TRAYNHAM: Where did you go to graduate school then?

MEYER: I went to the University of Iowa, which was the recommendation of Dr. Swenson, who had also gone there. He knew some of the professors, which I think was of some help to me later on in choosing whom I worked for.

TRAYNHAM: Did you consider any other universities for graduate study, or did you just center in on Iowa?

MEYER: I just centered on it and went there.

TRAYNHAM: Tell me something about your graduate school experiences.

MEYER: Well again, I really had no intention of going on for a doctor's degree. I was just going to get a master's degree and go work. My wife and I had gotten married after I'd gotten the bachelor's degree, and she'd gotten her degree at the same time, but by the time I was going to get my master's degree, things still hadn't opened up in the job field. I was talking to my advisor about what I could do. He said, "Well, why don't you go on and just forget getting a master's. Just go on and get your doctor's degree." I said, "Well, do you think I've got enough on the ball to do that?" He said, "Well, what are your grades?" I told him. He said, "Yes, I think you do." That's how I decided to go right on for a Ph.D. from the bachelor's degree.

TRAYNHAM: When did you begin your doctoral research?

MEYER: I'm not sure, but I think it was the second semester of my first year.

TRAYNHAM: With whom did you work?

MEYER: Well, I had two choices in organic chemistry that Professor Swenson had suggested. The one was Dr. Ralph L. Shriner, and the other was Stanley Wawzonek. Dr. Shriner limited the number of graduate students he had. He was up to his limit, so I went to work for Dr. Wawzonek.

TRAYNHAM: What was the area of your research with him?

MEYER: Well, when I started out, I was worked on theophylline derivatives, which are central nervous system stimulants. He had some association with Smith Kline & French at that time and was interested in synthesizing new drug materials, so that's where I started. The project had been worked on by a couple of other people, and it was one of those projects where you bumped your head against the stone wall. That's where I was at the end of about the first year of research.

TRAYNHAM: You changed the topic then?

MEYER: Yes. We went to a seminar one day, and somebody was talking about sulfilimines, phosphinimides, and arsinimides. Dr. Wawzonek said after we went back to our offices, "You know, why don't you think about trying to make an aminimide," which contains a nitrogen-nitrogen bond with a positive charge on one nitrogen and a negative charge on the other. I went to the literature, and didn't really find a lot. As a matter of fact, from <u>my</u> searching, I never did find that anybody had ever made a stable aminimide. It's like the isomer of hydrazine, so I went to work and tried to synthesize one of those. That's what I did for my doctoral thesis (1). I synthesized one compound and characterized it, and that was it.

No one really was able to use that technique to synthesize another analog. You know, in organic chemistry, once you synthesized one compound you synthesized a whole family of them. Well, I never was able to do that. Later on somebody else synthesized another aminimide with a different technique, and finally the third student found a more general synthetic method, and it grew to a pretty good-sized field. In 1980, Dr. Wawzonek wrote a review article in which there were one hundred and eighty papers and patents that had been issued on that topic (2). I guess I feel it was kind of neat that I at least opened up another field of chemistry for other people to explore.

TRAYNHAM: Yes. I'm sure it was interesting and satisfying to have done that. Once you finished that dissertation and received your doctoral degree, what did you do then?

MEYER: Well, by the time I was starting to look for a position, the industrial field had opened up, and I had opportunities at that time to interview twenty-six companies. I narrowed it down to ten, and then I added two more to it. One, because the one company had given me a fellowship, and I figured I just had to give them the right to take a look at me, and me at them. Then the other one, the interviewer came through, and he made his company sound so outstanding that you felt you just had to go and see whether it was really hot air or whether he really knew what he was talking about. I went out an interviewed these twelve companies and eventually elected to work for Standard Oil of Indiana, as it was called at that time.

TRAYNHAM: What was it that persuaded you to choose that one among all the others?

MEYER: Well, they had been in the process of starting a new group in petrochemical research, and I thought that sounded like it would be pretty interesting. They were more in the Midwest. I had spent one summer working for DuPont, while I was at the University of Iowa, in their Wilmington research area. Although I very much enjoyed working for DuPont, I didn't care for the Wilmington area at all. They did make me an offer for Waynesboro, Virginia, but I eventually decided that Standard Oil was the place I wanted to go to work.

TRAYNHAM: Was it a combination of the work that you were going to be associated with and the geographical location that persuaded your decision that Standard Oil of Indiana was the place for you?

MEYER: Well, it was at least the work I <u>perceived</u> I was going to be doing. I thought at that time that I wanted to do exploratory-type research, and I thought that I'd have that kind of opportunity working for Standard Oil. It turned out that I really wasn't prepared to do that kind of totally independent exploratory research at that point in my career.

TRAYNHAM: Well, what did you move into then?

MEYER: Well, I actually did work in exploratory research for the first two years I worked for Standard. Then they had come to an agreement with Scientific Design to look at oxidation technology to make aromatic carboxylic acids with the cobalt, manganese, and bromine catalyzed system. At that point everything was clouded in secrecy. They needed more people to work on that project, and they asked me if I would work on it. I had realized that I wasn't making very much progress working on the projects I'd had, so I transferred over to that project—probably 1955. I'd worked about two years in what I called probing or exploratory kind of research, and then moved to doing development research.

TRAYNHAM: Well, apparently it turned out to be a very astute move.

MEYER: Well, it turned out good for a couple of reasons. I think, as I look at it, I'm not sure just how much native talent I had. I needed a lot of help, and my first supervisor I'd had at Standard Oil was not the kind of person I needed to supervise me. I moved from him to another supervisor [Dr. Donald Barney] who wasn't afraid to tell the story like it was. He called me in, closed the door, and said, "Well, this is your last chance." I'd had no idea I was in that kind of situation. Then he went on and said, "Now, this is what I want you to do." He gave me a lot of guidance. The real problem was, I wasn't very productive, and I hadn't done nearly as many experiments as I should have. He just kept throwing me stuff to do and encouraging me to keep up with him.

One day I was getting frustrated because I thought I just had too many things to do. I sat down and counted them up, and I had ten projects to work on. I walked into his office and said, "Which six of the ten would you like me to really work on?" [laughter] I think what that convinced me of was that I'd finally gotten confidence.

A lot of the stuff I was doing was analytical research, physical chemical research. I had an analytical minor at Iowa when I was there, so that was kind of natural, but physical chemistry was my weakest field. I did quite a bit of physical property measuring—things like vapor pressure and solubilities. Then in the early stages, part of what I was doing was looking at how to separate components. We were oxidizing the mixed xylenes and separating all the acids, so I spent a lot of time on trying ways to separate them and ways to analyze the materials that we were separating, how to get rid of the impurities.

TRAYNHAM: You explored previously untried ways of separating the isomers that were obtained from the oxidation?

MEYER: Yes, but most turned out to be not commercially acceptable. Isophthalic and terephthalic acid were the two that were the most difficult to separate. Just about in any solvent tested, they would dissolve in at about a twelve-to-one ratio. Then you just couldn't separate them using two solvents. There were a couple of ways you could get them at ninety-six-to-four or something like that, but they weren't really clean-cut methods. Jim [James O.] Knobloch and I found that you could actually separate them using pyridine as a solvent, in which the isophthalic acid was less soluble than the terephthalic acid in that particular situation, so you could then separate them by going back and forth between solvents.

One of our engineers [Russel Malo] found out that the isophthalic acid would grow larger-size crystals than terephthalic acid during crystallization, but you had to crystallize it in the right way. You had to nucleate and then grow the crystals fairly carefully. Then if you did that, you could get a ninety-five, ninety-six percent separation by just separating them on the basis of particle size. That's how we actually eventually separated the isophthalic acid from terephthalic acid in our first commercial plant. My contribution was defining the nucleation and crystallization parameters.

TRAYNHAM: How did you separate the particles by particle size? Surely you didn't have to use tweezers, as Pasteur did.

MEYER: No, we used a Dorclone. You put it through the device, and it separated them on the basis of centrifugal force. The interesting thing, in order to test this, you had to actually have the Dorclone the same size as the commercial device. Five hundred gallons of slurry were enough for a thirty-second test, but it did work. It wasn't as good a commercial process as we would've liked, because to get the best kind of crystal growth, you had to remove some of the impurities by carbon treatment first. The nucleation period was long for a commercial process—it just took too long—but eventually we did make a product that was about ninety-six percent pure and used it to develop the market.

TRAYNHAM: Nearly all the accounts of your research career emphasize your discovery of a particularly good method for purifying terephthalic acid. Was this the method that you are famous for discovering, or was there a later development?

MEYER: No. That was certainly the most commercially significant discovery I made. I also invented Amoco's process to make dimethyl terephthalate [DMT]. I made that discovery first, because that was our entry into the polyester business. That was the only material that was used for making polyester at the time. The way I invented that process was kind of an interesting thing. We were having difficulty really developing any kind of commercial technique for making DMT on the basis of the research we were doing. We were wondering one day what happened to the catalyst in the system. We had been using a sealed-tube solubility technique for determining solubility of the various acids in acetic acid and water and all those mixtures, and my boss said, "Well, why don't we see if we can use that technique to find out if the catalyst is soluble?" We did. The glassblower told me that the sealed tubes that I was using were good for five thousand pounds per square inch pressure. It turned out later, I found out they were maybe good for one hundred psi, but they did last to much higher pressure most of the time. Eight out of nine would withstand the two thousand pounds or whatever it was that we had in the experiment.

Anyhow, once we got to looking at that, we said, “Boy, that would be a good technique to study the esterification,” because you could get them up to temperature in two minutes, then run them for whatever period of time you wanted, and cool them off in two minutes. You could actually get kinetic rate studies. Whereas in an autoclave, it took us half an hour to heat up the autoclave and a half an hour to cool it down, you just couldn’t do that and readily obtain rates. In doing this, I found out that the esterification didn’t take place at all temperature if you went through the critical zone too quickly. Methanol’s critical temperature was at 240 degrees Celsius, and we were trying to run the esterification at 260 degrees Celsius. If you went through that too fast and didn’t make enough, dimethyl terephthalic to provide a liquid phase, you got no esterification. That was really the basis of the invention: that you had to have some DMT in the process stream to make dimethyl terephthalate when operating above the critical temperature of methanol.

That’s how we actually got into the polyester feedstock business. You know, even in those days, in 1956, it just seemed foolish to go through all that process if we could purify TA (terephthalic acid) directly. The terephthalic acid that we made by our oxidation process was a lot more pure than that that was made by the nitric acid process, the original way TA was made. The material that I got from nitric acid oxidation was little better than ninety percent pure. It was aqua-colored. It had nitro compounds in it and oxidation intermediates, whereas ours was just mainly oxidation intermediates and a little bit of tannish color to it. That gave us hope maybe that we could purify it. Using DMT as a feed stock, you had to make the ester, send the ester to the polymer people, and they polymerized it with ethylene glycol and got impure methanol as a co-product. They shipped this impure methanol back to us and we had to purify it. This was a lot of steps, whereas if we could purify the TA directly by some simple way, and have our customers polymerize the purified acid with ethylene glycol, which looked like a lot better way to go.

TRAYNHAM: The pyridine solubility was your way of purifying it?

MEYER: Well, to start with, no. One of the real problems, of course, with terephthalic acid is that its good property in polyesters is, its high-temperature properties. It gave fibers the ability to make permanent press garments in a natural way. It had a high enough melting point so that you wouldn’t melt it with an iron and yet it retained its shape after washing. This high-temperature property was a negative from the standpoint of processing; terephthalic acid didn’t melt, and it had very little solubility. At 200 degrees Celsius, it still was only one percent soluble in solvents like water or acetic acid, which we felt we had to be able to use in order to be economical. So our first experiments were actually done with sodium hydroxide.

We had at Amoco the ability to do what we called ten percent-time experiments, so I guess somewhat due to my encouragement, a lot of people were doing ten percent-time experiments on how to purify terephthalic acid. One of my colleagues actually found that with dissolving it in base first, and then treating it with chlorine—which is really hypochlorite—and then carbon treating it, you could make a nice-looking terephthalic acid that was also very pure.

TRAYNHAM: Dr. Meyer, what do you mean by a ten percent-time experiment?

MEYER: Well, that meant that we could use ten percent of our time, in theory, to do any experiment on anything we wanted to. It was our choice. If you wanted to get credit for that, of course, you wanted to choose something that hopefully the company was interested in or would be interested in. I had quite a few ten percent-time experiments that didn't work out very well, and they stayed in the notebook, but I also had a lot of other ten percent-time experiments that did get known by the company.

Anyhow, Al Hensley was able to purify terephthalic acid. We had said that we had to have a material that was as pure as DMT dimethyl terephthalate, and that was 99.97 percent by cryoscopic purity measurements. Since you couldn't do that—you couldn't melt terephthalic acid, you couldn't get purity in that way—we analyzed for all the impurities and subtracted the percent of impurities from one hundred, and hoped that we had analyzed for enough to get an accurate purity measurement. Anyhow, this material that we made by this salt-chlorine treating process gave us material that was in that range of purity (99.97 percent).

TRAYNHAM: That was satisfactory for direct use in the polymer production?

MEYER: Well, we thought it was good enough, so we made a thousand pounds of material and sent it to our DMT customers. They looked at it, and came back to us, and said that it just didn't make a high enough molecular weight polymer. I talked with Paul [J.] Mehalso, who had the lab next to mine. He was our polymer expert. He made the polyethylene terephthalate from the DMT to end-measure our use quality. He knew how to make polymer from DMT, and I knew how to esterify TA from my work on making DMT. I figured out what kind of conditions we needed to run to make ester from terephthalic acid and ethylene glycol, and then he took it over from there.

For the first experiment, I made the pre-polymer and handed it over to him. We had decided to really load up the material with the best catalyst we could think of in quite a large amount, in order to get the molecular weight up. Well, we ran the experiment. These experiments were run in glass tubes, and you broke the glass tube after it cooled down. As a matter of fact, when the polyester cooled off, it just pulled the glass away from the inside of the glass tube we had run the experiment in. Then we'd drill the polymer to get a piece of material so that we could measure the molecular weight by intrinsic viscosity. On that particular sample, we couldn't even drill it, it was so hard, so we never found out what the molecular weight was.

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MEYER: On our second experiment, we backed off to normal catalyst, normal amounts, and we made material that was the same molecular weight as we made from DMT. Then we went out and got a one hundred percent polyester shirt, a Dacron shirt. They were pretty awful things to wear because they were slimy and they got gray looking when you wore them and put them through a number of washings, but we thought, "At least we've got one hundred percent commercial polyester for comparison." We compared the color of that polyester with the color of the polymer we made. As near as we could tell, it was the same color, so we concluded then that that was indeed a good material, that you could indeed make polyester from terephthalic acid. My colleague in this was Paul Mehalso.

On the basis of those two experiments, we got a patent on how to make polyester from PTA (3), and one of the claims that the attorney put in it was a composition of matter claim on that intermediate I gave to Paul Mehalso. I'm not certain, but I think just about anybody who makes polyester with PTA goes through that intermediate. Nobody ever sued for infringement—none of our customers sued the others for infringement of their processes. I think at least this muddied the waters enough so that they wouldn't do that. I think that in addition to proving to ourselves that polyester could be made this way, it also decreased the possibility that our customers would sue one another for infringement of their processes.

TRAYNHAM: The process became a major operation for Amoco, didn't it?

MEYER: Yes, but not right away. We ran that experiment in 1958, and not much really happened for the next three years, except for an occasional ten percent-experiment when somebody got an idea for how they thought they could purify terephthalic acid. Then in 1961, actually, a couple of customers came back to our marketing people and said they wanted to go into the polyester business <u>big</u>, but they wanted to do it differently from DuPont, and they wanted to do it with PTA. We were sitting there with that salt process, which number one, was about equivalent in economics, or worse, than DMT; and number two, we'd make a whole bunch of salt we had to dispose of. The only way of getting rid of the salt was to pour it down a deep well, and even in those days, for environmental reasons, we didn't want to do that.

We then set up a group of three of us to look for a commercially acceptable way of making purified terephthalic acid. Of course, the tendency always is to go with what you know. We divided up the areas, and one of my colleagues thought the best way would be to just improve our oxidation process—which was a very good idea—but he also had the responsibility for looking at hydrogen treatment. I looked at, "How could you recycle the salt in the hypochlorite carbon treating process?" Although there were some ideas on how to do it, none of them really worked very well. We could do it, but it wasn't very amenable to commercial techniques.

I looked at lots of other kinds of things. Another colleague had to make a million and a half pounds of this salt process PTA for our customers to help them develop their polymerization techniques, and he had the responsibility for that, plus some other new approaches. Well, after I had looked at lots of different approaches and everything was failing and not doing very well, I went to the other colleague in the group and said, "How about hydrogen treatment? Would you mind if I looked at that?" He said, "No, not at all, but it won't work." He said, "It won't work because you'll reduce the benzene ring." That set a requirement on the system then.

The major impurity we had to get rid of was the aldehyde acid, which was an oxidation intermediate. The problem with it was that it formed a solid solution with terephthalic acid, so you couldn't separate it by crystallization. You had to convert it to something else in order to make the separation. I'd always thought that the easiest way to do it was by oxidation, but in either acetic acid or water, you just could not oxidize that aldehyde to low levels. It would oxidize extremely slowly at best, and it seemed to me it was almost like it was a stoichiometric reaction or worse with the material you thought was your catalyst.

Anyhow, when you looked at the materials that were best catalysts for hydrogenation of aldehydes and did not reduce benzene rings, nickel on kieselguhr looked like the best catalyst, so we got some nickel on kieselguhr and set up and ran the first experiment. My technician, John Banns, was very good at scrounging equipment, and he got a big rocking autoclave, and we set up and ran the experiment. It had no cooling coil in it, so we had to wait overnight for it to cool down. We opened it up the next morning, and, boy, were we disappointed. Everything was green and just didn't look very nice, but John was pretty stubborn, and he said, "Let's analyze the solids."

We had analytical tools to analyze for aldehyde acid, and boy, the aldehyde acid was gone, and there wasn't much cyclohexane product. Therefore, the benzene ring wasn't reduced. He said, "Well, why don't we get rid of that green stuff," which was the nickel, of course, which had dissolved from the kieselguhr. We washed it with nitric acid and some acetic acid to clean it up, and it was nice white-colored-looking material. Although this experiment showed that we had been chemically successful, we actually set another requirement, and that was that the catalyst had to be resistant to the corrosive nature of the hot terephthalic acid solution, which really was quite corrosive when you got up to the temperatures we were using, 240 degrees Celsius. At 240 degrees Celsius, you've got a ten percent solution of TA in water, and our design and economics people thought maybe working with ten percent solution would be all right from a cost standpoint.

The next best catalyst in the literature was palladium on carbon, and Hans Leipold, who was next door to me, had been working on decarbonylanation of aldehyde ester for purifying DMT, so he had palladium on carbon catalyst. He loaned me some catalyst, and John repeated the experiment. The next morning when we opened the reactor, the crystals that we saw must have been half an inch to an inch long, nice white-color. We analyzed those crystals, and again, the aldehyde acid was gone, and there was very little cyclohexane formed. When we measured the color of the terephthalic acid, it was in the right range, and we said, “Now, that’s pretty good.”

About three days later the head of the research department, Dr. Carl Johnson, walked by, and we called him in and said, “We’ve discovered the PTA process.” You know, we were joking a little bit about it, but actually two months after we ran the first experiment, we decided that was the process we were going with commercially. We got together with the Amoco oil engineers, and my friend, George Olsen, who had been working on making the million and a half pounds by the salt process set the design basis for the new commercial PTA process, and by June the commercial process was designed. We went in to Amoco management, and asked for the money for the first plant, a forty-million-pounds-a-year plant. John Swearingen said yes, after he’d asked, “Could you sell the product?” The marketing vice president, Jack Lambertson, said, “Well, if manufacturing can make, I can sell it.” [laughter]

In 1965, the first commercial plant started. It was forty million pounds per year. By 1970, we had four hundred millions pounds per year capacity. Then during the 1970s it continued to grow at an extreme rate. It grew for ten years at thirty-five percent a year, so it was just unbelievable growth. It turns out that we gave some benefits to our customers. We sold them the terephthalic acid at the same price per pound that they were paying for the DMT, which meant they got seventeen percent more active material for the same amount of money. An area that we didn’t really know about is that the polymerization took about fifteen or twenty percent less time when you started with the acid than with the ester, because with DMT you never quite got rid of all the ester groups during the trans-esterification. At the end, when you’ve finished off a polymerization with PTA, the rate was faster and this increased rate reduced the size of the polymerization reaction and the amount of energy required, both of which reduced the cost of producing polyester.

TRAYNHAM: From that hydrogen treatment process came the very successful Amoco process for producing purified terephthalic acid?

MEYER: Yes.

TRAYNHAM: Is that development the one that in your career was largely responsible for your receiving the Perkin Medal?

MEYER: As a matter of fact, almost entirely, I think. I think it’s important to say that there were some other things that made the PTA process really work well, too. When we first started commercially in 1958, we had moved so fast. Everything was batch technology. In 1962, we finally got to the point where we could oxidize paraxylene continuously to terephthalic acid. The continuous oxidation process was invented by Don Hannemann and Wil Zimmerschied. As finalized, the process was simple. P-xylene, catalyst, and solvent were fed continuously into a stirred tank through one pipe and air through another pipe. The terephthalic acid (TA) formed by the oxidation precipitated in the reactor producing a slurry. This slurry was removed continuously and the TA separated from solvent by centrifuging. After washing and drying the crude TA, water and hydrogen were fed through a catalyst bed continuously, then crystallized, centrifuged, washed, and dried. Thus, we had developed one smooth, continuous process, except that we did have a separation step in the middle. It is an extremely simple process in the end, which means good economics.

The other thing that I think is important to point out is that none of the oxidation technology would probably have been successful—at least as successful commercially—if it hadn't been for the development of rocket science. The need in the rocket industry for titanium metallurgy was very important, and our oxidation technology required corrosive-resistant materials. It turned out that although Hastelloy C was fairly good, it developed leaks frequently. As a matter of fact, our Joliet plant mechanics group got so good, they could change out a reactor in twenty-four hours and have it operating again, so you know we changed out a lot of reactors.

Titanium came along, and it resisted the corrosion, so it's been very important. We don't have to change out reactors. The titanium reactors last a very, very long time. It was in large respect, I think, due to the development of titanium metallurgy for the rocket science that our technology has been as commercially successful as it is. As a matter of fact, the first eight commercial chemical reactors ever made with titanium were made for our oxidation process.

TRAYNHAM: Amoco was not involved in the titanium research itself, was it? It just used it?

MEYER: I don't know just how much our metallurgists were involved. We certainly set the requirements for what a reactor would have to be, but it was the metallurgy people themselves that eventually developed the technique. They actually put titanium in a stainless steel shell. In this way, they were able to reduce the amount of titanium that was needed, and you also got the strength of the stainless steel. At the temperatures we were operating, titanium might not have been quite strong enough.

TRAYNHAM: I'm glad we're dealing with the mixture of experiences you had in the research you were doing, because that's the way, of course, research really goes. Let me ask you about your movement, during this time, into research management. You have indicated a little bit about it, but can you give me more details about what appealed to you, your management style?

MEYER: Well, I guess I was never sure whether I was really management-qualified or not. I liked certain aspects of management, but obviously I enjoyed the research end. I thought I really wanted to give management a try. In 1966, I got that opportunity, so I took it and I managed the group, actually, that I had been working in. I was responsible for the tech service of the PTA. The R&D did the tech service on PTA, the quality problems, and if there were any manufacturing problems, we worked with our manufacturing people, so I was responsible for that. It was a great experience, because I went along with the marketing people to our customers. R&D and marketing sat down and tried to resolve whatever problems came up, and we were always very honest with our customers. Even though sometimes our customers didn't believe us, we would try to work to solve the problems. I think in the end, working with our customers that closely gave R&D a very good picture of what our customers needed in the way of product quality.

It turns out it isn't absolute quality that's as important as having the same quality all the time, because the real problem with polyester quality was color variation. Even with a white shirt, the eye can perceive fairly small changes going from white to a little bit yellowish and back, but what was really dramatic in some of our customers' cases was when they put dye into the material. They could dye red at times the same material, the same threads, and it would come out looking light pink, and sometimes red. Constant quality between both the PTA quality and the way that the polyester manufacturer made his material were really very important in producing quality fabric.

I think by being honest with our customers, we developed a high degree of credibility and mostly a very good working relationship with our customers. As a matter of fact, we spent a lot of time working on setting the specifications for PTA when we started. One of the people we dealt with was a technical specialist of one of our customers, who was very firm. My marketing colleague told me one day, "To know him is to hate him." Well, I got to know him later on, and it turns out that he really was a pretty nice guy, but he was just being tough on what he believed the quality needed to be. When we set those requirements and the specifications in the mid-1960s, they stayed until just a few years ago, when we got both better analytical techniques and techniques for making the polymer and finding out really what chemistry was important, what impurities were important in making polymer and so on. Those specifications were derived in what I would call struggling negotiations. Specifications were created, which I think were very good for us and for the industry. They held up for a very long time, and mostly they're still in place. Let's see, I got diverted back in there.

TRAYNHAM: I was just asking about your management experiences, and that's part of it. Do you have anything else to add to the portion of your career when you were in management?

MEYER: I had been in this field for about—I guess it was 1971, so that would be six years. I had spent from 1955 to 1971 essentially in the same field. I'd also worked with development of our trimellitic anhydride technology, and I'd done some with our isophthalic technology, but mostly it was with terephthalic acid and trimellitic anhydride. I decided maybe it was time to do something different, so I asked my boss if there was an opening in an area where they were working on developmental products. He said, “Yes.” I said, “Would you like to consider me for that position?” It was a sideways move, and he was very anxious to do it, so we made that move. Then I spent most of the rest of my career just working on new product development, exploratory research, that kind of thing.

TRAYNHAM: Tell us something about that. What particular product development were you most associated with?

MEYER: Well, nothing that I worked with actually ever became commercialized. I think partly it was because Amoco was growing so fast with the products they had that nobody was very interested in taking the risks of going with new products. I think the other thing was that there was a problem with the relationship between the marketing people who were supposed to be supporting us.

I guess I shouldn’t really talk too much about that. It just was that we never got teamwork going to determine what we ought to be doing.

Now, our management, I think, on the other hand, upper management, really wanted us to come up with new products. They kept giving us money to spend as long as we came up with ideas. R&D was coming up with lots of ideas, but the business end and the marketing aspects of them had not been tested fully enough. They didn’t say no, so management kept giving us more opportunity to do things. Actually, we had a lot of fun, and we had a lot of successful research that was unsuccessful from the commercial standpoint, which is one thing that just drives you nuts when you’re in that business, because you don’t just want to have successful technical research. You want your research to go commercial. That’s really the reason you’re in business in the industrial atmosphere.

After I had spent a fair number of years there—as a matter of fact, I became a director—we grew. I think when we started out we had about four people, and we probably grew to one hundred and twenty professionals doing that kind of work. I had decided that I really wasn’t cut out to manage that kind of work the way they wanted it managed, so I asked to get back into the technical ladder.

They moved me back to the technical ladder for my last two and a half years, which was a lot of fun, really, because I could talk to scientists and advise them from the technical standpoint. Once they realized I wasn’t part of management anymore, then they finally would start talking to me. You know, not everybody would open up, but there were enough that opened up, so it was a very enjoyable experience. In addition, I looked at a number of technologies from outside Amoco and evaluated them for the company to see whether we ought to pick them up or not—working with government labs, universities, and private investigators.

TRAYNHAM: Was there any particular project during that last two and a half years that you enjoyed above others?

MEYER: Well, yes. We had a project to find a process to make paramethyl styrene. One of the problems in dehydrogenation was that you could get isopropenyl styrene, which was really bad. It caused unwanted cross-linking. I found a way of selectively hydrogenating that material and getting rid of it. It gave me the sense that after twenty-some years in management, I could still go back to R&D and do it, so from that standpoint it was very desirable, enjoyable.

TRAYNHAM: Well, Amoco chose to recognize and honor your career with an award, I believe. Tell us something about that.

MEYER: Well, in 1989 the vice-president of research invited me out to lunch one day. I had a meeting that afternoon that I had to make a presentation at. He had asked me if I wanted a drink with the meal, and I said, "No, I've got to give a talk this afternoon." He sat down and told me that the company was going to honor me with an award. I said, "Well, that's nice." Then he told me that they were also going to give me one hundred and fifty thousand dollars, and Amoco had never given anybody that I knew of that kind of money for anything. He looked at me, and he said, "I thought I should have encouraged you to have that drink." [laughter] They did; they had a nice award party. It was a fun bash.

TRAYNHAM: I'm sure it was. What was the name of the award?

MEYER: The William M. Burton Award. Mr. Burton was the person responsible for developing thermal cracking of hydrocarbons to gasoline and kerosene. He and Robert E. Humphreys, whether they were co-inventors or what, they had been responsible for this cracking process. They were the last people who had gotten an award like this from the company. That was probably in, I don't know, in the teens or twenties or something like that, so I'm really the only person in the company who's gotten this award so far. They're looking for other people now who are deserving of the award.

TRAYNHAM: You received another major recognition from President George [H. W.] Bush, I believe.

MEYER: Yes. I got the National Medal of Technology from the Department of Commerce, and it was handed out by President Bush in the Rose Garden; I think that was 1992. That was an experience. That was a three-day bash, and they allowed us only two guests. We have five children, so it was kind of difficult to make a choice on which two of the five we should invite along, but we invited the two boys who are kind of in the chemistry field, and we all had a good time in Washington.

TRAYNHAM: You spent your entire career with one company, a very happy choice for you. As a result of your experiences there, what does scientific innovation mean to you?

MEYER: Well, I think you need to have a need for a product in the marketplace. I think that's one of the most important things. Early, before we had even heard of scientific design's technology, we had Dr. Herman Mark as a consultant. He had pointed out to us that from all the synthetic fibers, polyester had the best combination of properties to be a successful synthetic fiber. We started out, I think, with the idea that here was a product.

[END OF TAPE, SIDE 2]

MEYER: Then we had the raw materials. We had a management commitment. John E. Swearingen had said he wanted chemicals to be ten percent of the Amoco Corporation's income by 1970. Let's see, he became chairman about 1957 or 1958, somewhere like that, so he gave us twelve years to go from fifty million to two billion, and we made it.

I think you need to have the right kind of marketplace. You've got to have a vision of what that product is going to be. Then you have to have the means to make it. I think you have to have a marketing group that works with the customers to find out what the customers really need, and to then make the product that they need, not the product you want to make necessarily. Then you've got to price the materials, be able to price them such that the end consumer will buy them and use them.

TRAYNHAM: The company still makes some profit.

MEYER: Oh, yes. Yes. You've got to have that profit margin in there; otherwise, you give up on it. In our case, these were really terrific unknowns. I think one of the other things is that the willingness to take risks is so important. I see that as a very difficult thing for corporate managers today. There's so much heat from the Wall Street people that it's difficult for a chairman or president of a subsidiary area to make a decision and go with new technology that hasn't been proven commercially, and where there isn't a market proven. In our case, six months after the invention was made, we made the decisions to be commercial and what technology we were going to use. We hadn't done the pilot planting. We built the pilot plants and had them running shortly after, and showed indeed that the technology would work.

I think you've got to have the ability and the willingness to take the risks. You've got to be able to lose some money and not go broke. One company did that. They went with technology, and it didn't work, and they went broke. They just overextended themselves. You've got to be able to take the risks. Then I think you've got to have faith that things are going to work out, commitment to make things work if they can work.

TRAYNHAM: You made reference a little earlier, in discussing the latter part of your career, to scientific teamwork, and it was implied much earlier in your description of your career. Would you comment on scientific teamwork in an industrial environment?

MEYER: Well, there's no question in my mind that without it, things don't work; at least they work with extreme difficulty. Now, we have all kinds of organization techniques to try to set up situations so that you do indeed have teamwork. I think basically, all these techniques do is try to emulate what we did back in those early days. You know, we were a small group of people. There were three scientists and one design engineer in research. There were probably three people in marketing, and then the associated management people. That was our team. We also knew that if we didn't get with PTA, Amoco Chemical might not survive, so there was a strong desire to make this work. Based on that, people just worked together. It wasn't that there weren't at times some rubbing of the edges of personalities, but everybody realized that we wanted to work together to make this thing go. I know from an R&D standpoint, we would do anything that marketing asked to make the project a success, or that engineering asked.

I think the new techniques that they've got for management are really trying to get these sorts of ideals into the people in the systems that are involved. I think one of the problems that I see now is that on most of the projects that I've looked at recently, you don't have small teams; you have major teams, like teams of twenty and thirty and maybe forty people trying to bring out a new project. It is really difficult to get all of these people working together and trying to do the same thing, but I think it's important—essential—that they do.

TRAYNHAM: I'm curious. You engaged in a good bit of what would be called professional networking, both within the company, and perhaps outside the company, during your career. Do you have any particular recollection or comments you'd like to make about that?

MEYER: Well, certainly as far as inside the company, you develop friendships with an awful lot of people over a career that went as long as mine and the kinds of things that I did. I got to know a lot of people, and I used them and vice versa. That was important. Externally, not really. I probably didn't do nearly as much networking as might have been good for myself and Amoco. I did develop some good friendships on the outside, but a very limited amount.

TRAYNHAM: Were there any significant changes in the R&D support during your career?

MEYER: I don't know what you're asking.

TRAYNHAM: Well, were there any upper management decisions that affected the R&D? You indicated, I think, that there was a continual flow of monetary support for your work.

MEYER: Oh, all right. Yes. Well, in the early days, in the 1950s, Amoco Corporation Chemical Company was formed in 1957 as a subsidiary of Standard Oil. In the early part of the commercialization of the oxidation technology, we were losing big amounts of money. In 1962, Standard Oil was also trying to develop overseas presence in the oil production and refining area, and they were losing money there, too. I think it was sometime in 1962, we got together one day and the vice president said, "A third of you are going to be laid off. By 4:30 p.m. this afternoon, you'll all know what your position is." We didn't think too much about it until we got up to the lab, and I was standing there with two of my colleagues and we said, "That's one out of three."

In those days it wasn't a bad deal, because there were lots of job opportunities available for scientists. Most of the people got equal or better positions within ninety days of the time they were laid off.

That was one shot we had, and it brought us down to really a very small department. It was probably the most productive time in my career in the research department, because we had essentially let go of all the young people, and some of the people who hadn't been producing, so we had really very effective people working in the group. Support increased in a manageable way from then on until I guess the late 1980s, when restructuring became the name of the game. From then on until now, there have been big decreases in the number of people in the R&D department, but we grew at a very rapid rate from 1970 to about 1985. We hired a lot of people. I have to admit that sometimes we were hiring more people than I thought was logical for the kind of organization we had and for what we were attempting to do, but I guess when management allows you to hire people, you have a tendency to walk in and not question too much.

TRAYNHAM: Those people hired, whom you felt might have been excessive, did they stay with the company?

MEYER: When the downsizing came, a lot of it was done by retirement, early retirement and so on. I guess I don't really know what the percentage was, but a lot of people whom we had hired did get let go. I don't know enough about the organization right now to know whether they're back into the range that I think they should be or not.

TRAYNHAM: Well, what would you say is important for the future of chemical R&D?

MEYER: Well, I think probably the most important thing is that they know really what is the direction of the organization they're in. You need to know what is needed by management, and what kind of projects they will really think are great if you are successful. It's my belief that if you told management, in a conceptual stage of definition, the kind of project you were talking about, they would not, at that stage, be willing to support that project. You need to do some of what I call basic research: getting the understanding, getting the pieces of technology together that are going to allow for a commercialization. You've got to do that in the least expensive way as you can, to get the definition of the technology, and work very closely with the marketing people to get the definition of the product pinpointed early in the game.

Then I think the other thing is, you've got to recognize what risks your management is willing to take. You always can dream that they're going to take risks outside the level that they have demonstrated they're willing to take, but it doesn't happen very often. You need to know that, and you shouldn't be working on projects that don't come in with that risk level.

The other thing I think is, there is a tendency of people to want to commercialize very fast. I understand that, because the sooner you get commercialized, the sooner you start making money, but in doing that, you sometimes spend money for development of technology that isn't desirable technology, and then you have to go back and do it over. I think you need to be very wise at choosing when you've got the right technology to go commercial with, and not put the development money in until you've got that done. I know sometimes you think you've got the right technology and you don't. That's all right, because you will make mistakes if you're taking risks. I think you need to be working carefully with your marketing and your engineering people to know that you've got all the pieces put together—that if you're working on a new product, you've got the right product qualities, properties, defined. One of those qualities is always the cost of making the product, so you need to have all those things pretty well understood. Then you need to work on the things that are most important first, and set your priorities straight.

TRAYNHAM: You made some reference to the concerns about product development of the financial people in the industry. Do you think, given the situation in today's industry, that the projects that you successfully worked on would have been approved and supported today in the situation that you found then and now?

MEYER: Probably not, because we took some what would be considered today big risks, because we hadn't demonstrated everything, we hadn't crossed every T and dotted every I. As a matter of fact, we hadn't even come close. I think, in looking back, what made us successful is that we had developed a very sound basic understanding of the science that we were working with. We had the physical property data very clearly in hand, and we had the chemical rate data in hand. With that, and using good, sound chemical engineering judgment, we were able to take some of these risks, and they did work. They were within close enough proximity that we could adjust the process later.

For instance, the DMT process that we commercialized also went directly from little sealed tubes to a continuous commercial process, without the pilot plant in between. I don't know, it was maybe five or six years after we had commercialized the technology, one of our plants actually operated precisely at the rates that we set. Now, the thing that kept us from doing that all along was that the catalyst was insoluble and would precipitate out and fill the reactor. Our reactors would get smaller as we went on, and we'd have to clean them out every now and then. That process actually reduced the productivity of the reactors.

I think if you take risks, and you ask yourself what's going to kill you—and you make sure that you don't have anything in there that will kill you if you make a poor judgement, then you can take the shortcuts. I agree that in this day and age, people would not take the kind of risks that we took.

TRAYNHAM: Yet that risk, in this case, was very productive. It paid off well.

MEYER: Yes. Today it's one of Amoco Corporation's major profit-makers. I don't know exactly what the percentage of Amoco's profits is that are derived from it, but it's enough to affect Wall Street. I saw actually some reference to why Amoco's stock wasn't higher, and they said they were concerned about the price of PTA in the Far East. If one product can have that kind of an impact on Wall Street, I think it is enough to influence Amoco's stock price.

TRAYNHAM: Each year, the editor of the *New York Times Book Review* publishes at year end a list of the one hundred books considered to be most significant that were published during that year (4). At the end of 1996, the editor included among them a book by John Horgan called, *The End of Science* (5). A one-sentence summary of the book was given: "Embracing argument by a science journalist that the best and biggest discoveries are behind us." What's your reaction to that?

MEYER: Well, I think that's not a new thought. I'm trying to remember how many years ago, but it's been maybe twenty-five years ago that one of our vice presidents from Amoco Oil said that about the science for refining. Then continuous hydroforming came along after that, and we were just lucky we had some technology that worked. I think that the easy discoveries in science probably have been made, because discoveries come about by the identification for needs and opportunities. Then, once you've got that, the job is—well, it's not simple, but it's relatively easy to get from there to the scientific discovery that meets those needs. The needs and opportunities have pretty well been identified, so from that standpoint, yes; but I think there are still scientific discoveries that are needed.

I know just a few years ago there were needs to improve the speed of computer calculations, and it was dependent on developing the right kind of materials. They had defined in some general terms those kinds of materials, and people were working very hard at doing that. The volume of those materials wasn't going to be very large in terms of pounds, but if they could do it, they could make a lot of money. I think there are still going to be some inventions made, but they're not going to be made in the numbers and the ease with which they were made back then. I think there are some technologies that are producing commercial products now on a large scale and aren't, in my opinion, very good. There's opportunity for people to improve those technologies.

You don't always know whether or not you can do it, but I think that it can be done. For instance, butane oxidation to maleic anhydride is a fairly low chemical yield process, sixty or sixty-five percent. There's opportunity for somebody to develop a catalytic system that I think will take that up to ninety percent. That would really improve the technology, because basically the reactor is a big heat exchanger, because you burn so much feedstock that generates heat that has to be removed. Whereas if you got the selectivity to product up, you'd burn a lot less, and you'd create a lot less energy from the heat. It would just be a great improvement all the way around to making that process better.

There are a few products like that, but not many. I agree that a lot of the commercial scientific discoveries have been made, but I think there are still a lot left.

I think one other thing that people may forget is that there is a finite supply of oil and gas. It's probably going to last through the lifetime of most people who are living now, but sometime, maybe by the end of the next century, we may need to make chemicals from stuff other than oil and gas. Now, how do you do that from natural products, which is where I think it's going to have to come from? How do you do it in a way that is not going to totally disrupt the economics of the world at that time? I think there will be opportunities for scientific discoveries in the future. You never know exactly what they're going to be or how they're going to come about, but I don't think the last scientific discovery has been made.

TRAYNHAM: You retired from Amoco, I believe, in 1992?

MEYER: Yes.

TRAYNHAM: You received the Perkin Medal three years later. What impact did receiving the Perkin Medal have on you, with the fact that it came after your career had terminated?

MEYER: Well, I was talking to somebody, oh, in the past year, I guess. "You know," I said, "I really have been fortunate. I got recognized by the company, which is nice because you're recognized by the people you worked for and with and so on. It's really nice to have that." I said, "Then, I was recognized by the U.S. government. Boy, that's thrilling and exciting. Then I was recognized by the college I went to, because they gave me an honorary doctoral degree. Then finally, I was recognized by the industry I worked in. You just can't hardly come off any better than that, to get recognition from everybody who really was important to me in my life."

I think as a final recognition that was something that I really appreciated.

TRAYNHAM: It's certainly a remarkable record of recognition, as well as a remarkable record of industrial achievement that you had.

MEYER: Thank you.

TRAYNHAM: You made references, two times in the discussion, about your family. Tell me more specifically about your family. You have a wife and five children, you said. What are your children doing?

MEYER: Our oldest son, Kurt W. Meyer, is kind of a computer jock, I guess. As near as I understand it, he's a computer programmer, now working on accounting-type technologies. Our second son has a degree in chemistry and works for what was Union Oil Company—I'm not sure it's still that, because they're in the process of selling their refining operations. Anyway, he's working for that company selling oil products, lubricating oils and that sort of thing. Our third son is a chemical engineer and works for Petrolite Corporation as general manager of their Bayport plant down in Texas. He's essentially head of their manufacturing operations, although they do have some in England that he doesn't have responsibility for. Then our fourth son, Mike, [Michael L. Meyer] is an aerospace engineer, works for NASA at Lewis Laboratories in Cleveland. His last responsibility was testing major rocket engines for Lockheed out at their proving grounds in Sandusky, Ohio. Our youngest child, a daughter, is teaching school out in California.

TRAYNHAM: High school or grade school?

MEYER: She's teaching freshmen and juniors in high school, English.

TRAYNHAM: Well, it sounds as though your family accomplishments merit praise as well as your professional accomplishments. Is there anything else you think of that needs to go on the tape to give a summary of your career?

MEYER: Well, I don't know. I think it would be incomplete not to give my wife credit for putting up with me. I have a tendency to get a little emotional about things and worry about them, and she was the person who kept bringing me back to earth when I was too exuberant or down. She was there to level me out. Even though she never understood much about chemistry, she always acted like she was interested, even though I think sometimes she was totally bored. She allowed me to talk on and on about things that were bothering me.

TRAYNHAM: Well, on that note, I'd like to thank you for spending this time with me today.

[END OF TAPE, SIDE 3]

[END OF INTERVIEW]

NOTES

1. Delbert H. Meyer, "The Preparation of Trimethylamine-p-Toluenesulfonimide," Dissertation Abstracts, XIII, no. 6 (1953): 987.

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3. Delbert H. Meyer, "Fiber-Grade Terepthalic Acid by Catalytic Hydrogen Treatment of Dissolved Impure Terephthalic Acid," U.S. Patent 3,584,039, issued 8 June 1971.

4. "Notable Books of the Year 1996," *New York Times Book Review*, December 8, 1996.

5. John Horgan, *The End of Science: Facing the Limits of Knowledge in the Twilight of the Scientific Age.* (Reading, MA: Helix Books/Addison-Wesley Publishing Company, 1996).

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