CHEMICAL HERITAGE FOUNDATION

PAUL B. WEISZ

Transcript of an Interview
Conducted by

James J. Bohning

at

State College, Pennsylvania

on

27 March 1995

(With Subsequent Corrections and Additions)

ACKNOWLEDGEMENT

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Paul B. Weisz

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> Paul B. Weisz, interview by James J. Bohning at State College, Pennsylvania, 27 March 1995 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0141).

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

PAUL B. WEISZ

1919	Born in Pilsen, Czechoslovakia, on 2 July

Education

1938-1939	Physics Study, Technical University, Berlin
1940	B.S., physics, Auburn University
1966	Ph.D., ETH [Eidgenössische Technische Hochschule], Zürich

Professional Experience

1938-1939	Assistant, Humboldt University, Berlin
1940-1946	Research Assistant, Bartol Research Foundation and Project Engineer, MIT Radiation Laboratory (wartime assignment)
1942-1943	Instructor, Swarthmore College (evening courses to U.S. Signal Corps trainees

Mobil Research and Development Corporation

1946-1961	Research Associate
1961-1967	Senior Scientist
1967-1969	Manager, Exploratory Process Research
1969-1982	Manager, Central Research Laboratory, Princeton, N.J.
1982-1984	Scientific Advisor
1984	Retired

1974-1976	Visiting Professor, Princeton University
1984-	Distinguished Professor of Chemical and Bio-Engineering, University of Pennsylvania, (now emeritus)
1993-	Adjunct Professor, Chemical Engineering, Pennsylvania State University

1984-	Consultant, Catalysis and R&D Strategy

Honors

1972	E. V. Murphy Award in Industrial Engineering Chemistry, American Chemical Society
1974	Pioneer Award, American Institute of Chemists
1977	Leo Friend Award, American Chemical Society
1977	Elected member, National Academy of Engineering
1978	R. H. Wilhelm Award, American Institute of Chemical Engineering
1980	Honorary Doctorate (Sc.D., technological science), Swiss Federal Institute of Technology
1983	Lavoisier Medal, Société Chimique de France
1983	Langmuir Distinguished Lecturer Award, American Chemical Society
1985	Perkin Medal, Society of Chemical Industry
1986	Chemistry of Contemporary Technological Problems Award, American Chemical Society
1987	Carothers Award, American Chemical Society
1988	DGKM Kollegium Award (Germany)
1992	National Medal of Technology

ABSTRACT

Paul Weisz begins this interview by discussing his family background. Because of the political uncertainty of Austria-Hungary in the post World War I period, his family moved to Berlin when he was a young boy. Weisz was educated in the Gymnasium, where he was exposed to basic science and developed an interest in physics and chemistry. His father further encouraged him to pursue the sciences, and Weisz remembers building small radios. Weisz attended the Technical University in Berlin, and spent his free time in the laboratory of Wolfgang Kohlhoerster at the Institute of Cosmic Radiation Research. There, he worked on Geiger counter instrumentation and cosmic ray measurements. Because of Hitler's rise to power, Weisz decided to come to the United States, and arranged an exchange program with Auburn University. He earned his B.S. in physics from Auburn in 1940, and accepted a research position at the Bartol Research Foundation in Pennsylvania. There, Weisz worked on radiation counting, and projects relating to the National Research Defense Council. After gaining clearance to do classified work, he moved to the MIT Radiation Laboratory where he helped to develop a long range navigation trainer (Loran). Weisz returned to Bartol, but soon decided to move away from cosmic ray research. He accepted a position with Mobil Corporation, where he worked on catalysis and cracking catalysts. In the 1950s, Weisz began to investigate zeolites and shape selective catalysis. In 1966, he completed his Sc.D. at the Eidgenossische Technische Hochschule in Zurich, where he had worked with Heinrich Zollinger on dye chemistry. Weisz concludes the interview by discussing innovation in industry, the importance of interdisciplinary thinking, and his later work on Alzheimer's Disease and angiogenesis.

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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INTERVIEWER: James J. Bohning

LOCATION: State College, Pennsylvania

DATE: 27 March 1995

BOHNING: Dr. Weisz, I know that you were born in Pilsen, Czechoslovakia, on July 2, 1919. Could you tell me something about your parents and your family background?

WEISZ: Well yes, I can do that. My father was a soldier in the Austro-Hungarian army, and my mother was the daughter of an officer of the Austro-Hungarian army in the Czechoslovakian part. My father was Hungarian. As far as at that time, of course, they were citizens of the same entity.

My grandfather—that is, my mother's father—was commanding officer of the armory in Pilsen. My father was, during the First World War, assigned part of the time to that area, Pilsen. So he met the commanding officer's daughter, and that's what did it. [laughter] They got married in 1918. I was born in 1919.

I was born, in other words, to a Hungarian father, as the political divisions turned out soon thereafter—a Hungarian father and a Czechoslovakian mother, in Pilsen. The language they had in common was German, being in Austria-Hungary. They spoke German to each other. My first and mother tongue therefore became the German language.

Well, it was 1919. The time that I was born was the time of the armistice at the end of the First World War, and there was a lot of political upheaval. The various parts of Austria-Hungary were trying to become independent, and broke apart into Hungary, Czechoslovakia and Austria. Sometimes I'm not sure if I was born in Czechoslovakia or in Austria-Hungary, it was just at the time of the transition.

I spent my first few months as a baby in Czechoslovakia. Then my father took his family under rather upheaval circumstances, during all this revolutionary nonsense that was going on, back to Budapest, Hungary, where he and his brother had a shoe manufacturing business. So I lived in Hungary, then, for something like four and a half years. When my father and his brother decided that they would like to start another branch of their business in Germany, my father took his family, which meant me, I was the only child, to Berlin, Germany. So at somewhere around the age of four to five, I began to live in Berlin.

As I said, my mother tongue became German. I never learned Hungarian or Czechoslovakian—except I did hear it, of course. I heard it as a child, especially Hungarian, during my earliest years. An interesting aspect of that hearing, to me, was that later on in life, including now, I can pick up a Hungarian-language written piece and I can read it out loud, and people will think I can speak Hungarian. I mean, even Hungarians think so—yet I would have no idea what it is I am saying or reading. Somehow, apparently, the sound impregnated itself at an early age in such a way that I could and still can manage that particular sound. Well, that's just sort of an aside here. [laughter]

BOHNING: All your early education was in Berlin, then.

WEISZ: Yes. My early education was all in Berlin, exactly. I went to elementary school in Berlin, and went to what is known as Gymnasium in Germany—which is like our high school plus two years of college, like junior college. After Gymnasium, I started to go to the Technical University of Berlin.

BOHNING: Could you tell me something about your education, starting from the elementary education?

WEISZ: Yes, I guess so. Well, let's see. First of all, I guess I'll even start a little earlier. At least my parents always proudly told me this, that when we were first living in Berlin, my parents rented an apartment. The first one that we rented was owned by a retired gentleman who was actually a retired printer. He was a widower, and he sort of took to me as a child, as a four-year-old or I guess I was almost five years old. He would spend a lot of time with me, and basically taught me to spell and to write before I went to elementary school.

So then I went to elementary school. I don't remember much about elementary school really. Well, as you know, one remembers those things of the very early age that are, somehow, very outstanding, either because they hurt and were bad things, or if they were somehow very delightful. It's the Gymnasium years I certainly remember more. Well, there are two things, I guess, that come to mind about that time frame. I look back at that part as really having been exposed to some really basic science. I was supposed to learn a lot of other things of course, too, like history and geography, et cetera. But I didn't like history. I felt that, I was really not very interested in what somebody tells me is supposed to have happened one hundred years ago or six hundred years ago. I didn't do too well in history. The other thing I didn't do too well was Latin. Now this was a Gymnasium, which was called "Real-Gymnasium", which was characterized by teaching eight years of Latin! But at least I didn't have to also take ancient Greek, which was a required subject in other Gymnasia.

But I was strong in mathematics and physics, and in chemistry. I think I learned a lot, I guess, partly because of an inherent interest in it. Also, I think the teaching was very disciplined, and it was good. It was more disciplined than our high schools are, in general. In Gymnasium most of the teachers were Ph.D.s. Most of them were dedicated to thorough, good and very disciplined teaching of their particular subject.

I said that I didn't like history, and I didn't like Latin. I was—at least, I felt—very much interested in what makes today's world and today's environment and today's earth, so to speak. Also, I was always interested in discovering the utility of things. I think this is why I didn't particularly think much about history. I was kind of suspicious of the kind of things that were told, were handed down from one person and generation to the next, and on and on, after all—it's what nowadays in court you would call hearsay. [laughter]. It's actually third-hand, or seventh-hand, or hundredth-hand.

In Latin I felt that, "Why am I learning Latin? I will never use it." Whenever I raised the question, I would get the answer, "Yes, but it's a good basis to other languages." That was number one. Number two was, "Oh, you might be going to medical school, and then you'll need it." Well, to the first thing, my reaction was, "Look, why don't I just learn a language which I may use, and forget about the background and relationships, et cetera." Also, I guess I wasn't, at that time, thinking about going to medical school particularly. But I just felt like, even if I had to, why don't I just learn the expressions that are used in medicine, instead of learning Caesar and all the literature of the wars, and all that.

These two things left a considerable impression on me. As I say, you remember things that are either very pleasant or very unpleasant. The reason why I remember those two items particularly, well, going to the Gymnasium you end up with a final exam. That final exam isn't just what you did the last three weeks or three months; it's what you did the whole eight or nine years. So it was a pretty scary time to face that exam, yes, in Latin, in history.

I flunked Latin on the final exam. Actually, the rules at the time were that you couldn't graduate or get your Abiturium, as it's called—the diploma—unless you had a passing grade in everything. But I had at that time, simultaneously with that, three outstanding grades: mathematics, chemistry, and physics. That overcame—I don't know whether by rule, or just by common sense or something—the fact that I would have had to spend at least another year to repeat. [laughter]

The other demon was history, of course—well, that's kind of a scary story, but now it's kind of fun to remember—I said to myself before the exam, "How can I possibly read up on anything when it comes to history? I mean, what is he going to ask me? Am I going to worry about everything from the Phoenicians to the latest things?"

I decided, "I will just take the chance; I will gamble. I will bone up on one subject which happened to have been taught this last year." It was, of all things, migrations of the Germanic tribes. I remember sitting in the examination room. It was sort of like a baseball game, in the sense that you who were going to be next to bat, you were on deck. I mean, here was the table, and the professor would ask the questions of the guy who was sitting there. I was next, and I was sitting near the table when the professor who was still talking to the other guy handed me a little piece of paper and said, "Here's your topic I want you to think about." I opened that little piece of paper and it said, "Germanic migrations"! [laughter] So I actually got a fairly good mark for history on the exam—luck it was, with any other topic, I may still be sitting in the Berlin Gymnasium and not here with you.

BOHNING: Your interest was in physics more than chemistry, at this point?

WEISZ: Yes. Yes, it was largely in physics. But I was always interested in exploring almost anything. Anything that I felt was nature, I was interested in exploring.

I remember during my teens, I was active with chemistry sets and electricity sets and physics sets, and this kind of thing. I sometimes think back at, "where did my early interest come from, or how did it start"—maybe environment? DNA? or what. But I remember that my father lost his father. He hardly knew his father, lost his mother too and was raised by a stepmother. So he was not well off, and his education was limited. However, I know from his talking, that he always wanted a university education in science, but couldn't afford it. He had to work. He definitely hoped to see me get it.

But his interest definitely was scientific. I remember little things that sort of exposed me to problems like this. For example, I was just thinking the other day when my razor failed—an electric one—I remember him making a remark when I began to be a teenager old enough to start to worry about shaving. He would give me a razor and say, "You know, you want to take your blade and before you start, put it under cold water." I asked him why, and he would explain "Well, cold will contract metals and materials and this will make the edge of the blade sharper."

Of course, he was always hoping that I would go into science, and I guess he encouraged me. Not in any way pushing me—as a matter of fact, I remember that after Gymnasium, and I had to make a choice what I wanted to sign up for in the university, I said, "I want to become a physicist." My parents, I still remember—my father at least, maybe my mother too—would say, "Oh well, I don't know if you'll ever make enough money to eat, becoming a physicist," which in those days was quite true. "What are you going to do with studying physics?" is the kind of thing I got.

I tell you, there's one book actually, I think, that had a lot to do with it, which as a teenager I got hold of. In fact, I still have it somewhere. It's called in German, *Umsturz* *im Weltbild der Physik*, which translates—Umsturz is a difficult word to translate, it's like revolution, overthrow, upheaval, I guess—Revolution in the World Picture of Physics, the big aspects of physics (1). It was popularly written, and it was exciting to me. I think it had a lot to do with my early choice of direction.

Also, another aspect was that I was very interested in electrical things. Why? I've been asked this once in a press interview: "What made you first interested in such things?" I remember, I was maybe twelve or thirteen, we moved into another apartment. On the attic space, it came as part of the rental, I found some old—well, I mean, now they would be old—vacuum tubes that somebody had left. I knew they were vacuum tubes from radios. I got interested in, "How do I make a radio with it?" So I tried. In those days, I just wired up all sorts of the connections, not knowing just what a circuit really ought to be like. I tried all kinds of things. I had a pair of headphones and I had to see if I could hear something. I didn't. But my father had a crystal detector very early in the game, where you could hear the local station on a pair of headphones. I didn't succeed then, but I tried.

Another thing which I guess was a kind of first physics experience was, I used to study—do my schoolwork—and at one time, I had a compass sitting on the desk. It was getting dark, and I turned on the light. When I turned the switch, I noticed the needle was flickering and was making a pulse, an excursion. I wondered, "Why does it do this?" I turned it on and off and I began to realize that somehow the electric current, when it started, would do something with the magnetic field which otherwise only the earth has. I mentioned this story once when I was asked by a media person and then afterwards, I thought to myself, "Oh my gosh, that's not a very good example, because if somebody tried it now—one of the youngsters does it now who hears or reads about it—he'll find it doesn't work." The reason is that we had DC in the power circuits then. [laughter] Now, nobody has DC anymore, and this does not work with AC at all.

[END OF TAPE, SIDE ONE]

WEISZ: This was some of the earliest. I don't know whether you want to hear all this kind of stuff. [laughter]

BOHNING: This is fine. Moving forward, you wrote your first paper when you were sixteen (2).

WEISZ: Oh, how do you know that?

BOHNING: Your paper was on the echoes of radio signals. I believe you wrote it back in 1935.

WEISZ: Yes. Well, that was actually the second paper (2). All right, yes, after finding those vacuum tubes, and getting interested in radio, I became a radio ham, actually. I finally got to the point where I could build a set, and I graduated to building a little short-wave set and listening to short-wave transmissions. I joined the amateur club. I was the youngest member—I was fifteen. I wanted to hear far-away stations, that was the thing to do. I learned the Morse code by myself. I was listening, and frequently I would pick up a station in Japan—JAA, in code that was da-dah dah dah, da-dah, da-dah. I would hear this on a certain frequency almost every day. I noticed that often the signal had an echo like you get sometimes on transatlantic calls, you know. I was getting the amateur radio journals they had in those days. I heard about magnetic storms on the sun, that they were being recorded in intensity. I had a diary, a logbook, of when I had these echoes or when I didn't. I looked at the statistics of these solar magnetic storms, and I found there was a correlation with my echoes. I wrote up a couple of other things of these radio journals (2). I guess I was seventeen years old then. I couldn't get a radio amateur license to do any transmitting because you had to be a German citizen. But I was a Hungarian. So that's why I was a very active short-wave listener and observer ever since I was around fourteen, which was let's see, 1933.

BOHNING: Is that right, 1933?

WEISZ: Yes. That's when [Adolf] Hitler came to power. What reminds me of it is an incident about that publication I wrote, one of these articles I wrote. A strange little man approached me once on the street when I was leaving the house. He identified himself as a member of the Nazi newspaper *Der Stuermer*—which was run by the Nazi stormtrooper's organization. He said he wanted to "interview" me, but this was a funny interview—it was on the street. Clearly, they had seen my name writing two or three—maybe three by that time, I don't remember—articles, and suspected a Jewish name. I was born and baptized in the Christian faith, but the name Weiss was often thought to be Jewish. It was a scary thing because this was the ruthless outfit of the stormtroopers getting involved.

Anyway, let's see. I don't know if you want to go into the political aspects of those days.

BOHNING: Well, I would like to know a little bit more about this.

WEISZ: Guide me along here, so I don't talk about everything.

BOHNING: Hitler came to power in 1933. What effect did that have on your schooling? Did you notice any change in your schooling after that? Was there an influence within the schools?

WEISZ: Yes. Well, I guess the thing that comes to mind right away is that there was—I don't know whether this was in history or biology—quite a bit of intrusion of race theories, the superiority of the Nordic race and so on. I didn't pay much attention to it, but it was there, and it was obvious. It showed up in history and, I guess, biology, in connection with heredity and its influence. That was an emphasis that got in very quickly.

Well, there are other things. Some subtle some not, like teaching history the way that they wanted. Germany was über alles, above everything. That was coming through, but then only from some of the teachers.

BOHNING: What about the Hitler Youth movement?

WEISZ: Oh, yes. Well, I tell you, the youth movement, that's interesting. In 1932—so that must have been when I was thirteen years old—I joined the Boy Scouts. Then, oh, was it March 1933? I think it was in March. Hitler took power. Of course, nobody quite knew what all would happen. I went to my Boy Scout meeting, and there were two things that I remember. There was a different leader. Well, I didn't know where he came from. But the Scout groups had leaders, and they weren't there all the time, the same ones. But then he talked, and there was obviously some political influence, suddenly, that came out. The thing that I remember most of all is, I remember him saying that, "Oh yes, well, Germany is terribly oppressed. The Versailles Treaty is such-and-such that we can't do anything, et cetera. We need access to petroleum. We must expand, one of the first things we have to do is to invade Romania, to take over the Ploesty oil fields." I remember that word, Ploesty oil field, from when I was twelve years old, hearing this from this young Nazi leader.

Well, the very next thing, we were told we were now part of the Hitler Youth. It was getting very, very bad. There was Jew-baiting going on, he even distributed songs with words about the evils of the Jews. So I became a part of the Hitler Youth without knowing it. Of course, I was out right away, I pleaded that, "I can't be part of the Hitler Youth. I'm not a German, period." There was no fuss raised about it. But that was my involvement in the Hitler Youth.

BOHNING: Did that affect the schools as well? Did the youth movement permeate the schooling?

WEISZ: Not overtly, as far as the education was concerned. Of course a number of the older boys were involved in some of these movements. I know the parents of some of my classmates were. I remember that one classmate, his father was a stormtrooper with I don't know how many stars, and all this. So I mean, you couldn't get away from it. When I think back at it, I heard about people being carted off, Jews and opponents to the regime, and taken to concentration camps. It didn't happen to anybody close to me. Therefore it was just that I heard about it. I was wrapped up in my radio and other interests. I probably didn't pay as much attention to it as my parents certainly did.

I was interested in all sorts of exploration, science exploration. I knew I wanted to go into science. I wanted to go to America. I had decided, myself, when I was an early teenager. Partly, I suppose, it may have been a thought introduced by my parents but certainly supported and encouraged by them. After Hitler came into power, I know I heard them comment that we wouldn't want to stay there if this goes on.

But to actually do it, for me to simply go to America, was very difficult, for two reasons. Number one, I had nobody—I mean, no relatives or anything—in the States. Besides, my father was not well enough off financially, but anyway, living in Germany, you couldn't take any money out of the country if you had it. Foreign currency you couldn't get, and your own currency you were not allowed to take out, even if you had it. So this was a sort of a triple-bondage situation.

I had an idea about how to do it, however. First of all, let me mention that I learned English. Of course in school, it was the fourth year of Gymnasium that we began to have school English. Actually, a year before that, we started French. My parents encouraged me to learn English, my father got to know a private teacher of English. She happened to be a lady who was a cousin of—what was his name? Fiedler.

BOHNING: Arthur Fiedler? The conductor?

WEISZ: Yes, right. So my father had me get some private lessons, in addition to my schooling, with Miss Fiedler. So I learned some English then from her, too. It was the British English, by the way. Well, let's see. I was starting to talk about how I managed to get out. Do you want to hear this? [laughter]

BOHNING: Yes.

WEISZ: Here's what I did. I went to the Humboldt University, their library. This is the university in Berlin—not the technical university, but the other one. They had a few catalogs of American universities. I picked three of them, at random, in those days I didn't know the difference between Harvard and Podunk College. I picked three. They happened to be Iowa State University; one was Auburn, or it used to be Alabama Polytechnic Institute then; and the third one was the University of Michigan. I wrote to the three registrars. I introduced myself and I said something like, "I'm very anxious to go to the United States. There's no way for me to finance this, etc. etc. I have a proposal, namely, if you know any student in your institution who might have an interest in spending a year in Germany to study, my family is willing to take him on and finance the entire year, if his family will then give me credit to spend a year in the States."

Out of those three universities, I had two favorable answers, actually. Iowa State said, "Oh, it's an interesting situation, and we will look into it." Auburn wrote back that, "Well, we happen to have a student who is majoring in pharmacology. He wants to become a pharmacist, and he might like to come. Why don't you write" etc.

Anyway, I got in touch with his family in Alabama. Their last name was Hartung. It was a German name. It turned out that yes, their son was interested in spending a year in Germany. Actually, the family was descended, from the father at least, from a German settler who went to Alabama. His name was Cullman. In fact, the little town where they lived in was named after him–Cullman, Alabama, which is north of Birmingham. Anyway, this developed into a very fine relationship—at first by correspondence, but then in 1937, I think, he did come over. He spent a year at the University of Munich. This gave me my contact and the financial capability, at least, of getting started. This was responsible for my ability to come to the States. My first stop was down to Cullman, Alabama. I spent time in Alabama with the Hartung family. They became, you might well say, my adopted parents, and I was and ever will be grateful for their care, understanding and help for many years thereafter.

Now, you probably want to go back to education?

BOHNING: Yes, but let me just get the time frame correct. What year was it that you went to Auburn?

WEISZ: I came to the States in March, 1939, which was just a few months before the war started in Europe. It was a traumatic time. At that time, I had entered the technical university, and I had been there. I was so interested in doing science that, even though it was my first year at the university, I wanted to do research. I was more interested in research than in sitting in classes. [laughter] So I went and I actually got in touch with the Kaiser Wilhelm Institut fuer Physikalische Chemie in Berlin. That was the institute that Otto Hahn and Lise Meitner were working in. I talked to one of Meitner's post-docs—Dr. Philipp was his name. I told him I would love to work in the laboratory.

Would there be a spot for me? He told me that, "Well, I'm afraid not, because we're doing"—what we call what is it …

BOHNING: Classified?

WEISZ: Confidential, yes. Classified work. As you know, Meitner and Hahn first discovered nuclear fission. Of course, I didn't have any knowledge of that then, nor did few others, and only a decade later did I learn that it happened that same year I stopped by there. "But," he said, "I'll talk to somebody else about you." Sure enough, soon after that I heard from Professor Wolfgang Kohlhoerster, who was the director of the Institute of Cosmic Radiation Research of the University, and whose institute was quite near the Kaiser Wilhelm Institute, in the Berlin suburb of Dahlem. He was the co-discoverer of the cosmic radiation with Professor Victor F. Hess, who was now at Fordham University in the States. Hess received the Nobel Prize for the discovery.

[END OF TAPE, SIDE TWO]

WEISZ: So this was some few years before that. I did get word from Kohlhoerster, and a chance to work there. I spent time—every free moment I had, I would go out and work in Kohlhoerster's laboratory. In fact, I began, actually to work on my doctoral thesis. I worked on Geiger counter instrumentation and cosmic ray measurements.

Well, as far as education is concerned I must point out some other things first. Number one, you may know that if you go to a technical university—or at least it used to be, if you'd go to a technical university in Germany—one of the requirements even for entry into the university was that you had spent some few months—I don't know how many—as what was known as a "work student" in industry. This experience had a big impact on me—that I was exposed to industry at this early time, during university study, which here would correspond to after the second year of college. It was an institution—maybe it still is—in the sense that the industries made room for so-called "work students."

I was a radio ham, as I told you, although only on the receiving side. But one of the members of the amateur club in Berlin became a good friend of mine. He worked for Telefunken, a large company somewhat equivalent to what was RCA in the States. He was a group leader at Telefunken, and he was happy to arrange my employment directly in his group.

So I went to Telefunken. I think it was in 1937, just after I got my Gymnasium "Abiturium" diploma. I spent something like four months at Telefunken. I was fascinated by the work. He gave me the job to develop the antenna coupler for a radio transmitter which Telefunken had a contracted to design as aircraft radio equipment for the government of what were then the Dutch East Indies. I was working on this thing. In fact, something I did on that led to a patent application, the first patent application I generated, at the age of about eighteen. Anyway, that's part of the early exposure.

Then I also spent about a summer month at another company, which was Osram.

BOHNING: Oh, yes.

WEISZ: They were producing and are probably still producing light bulbs as a main product. There I learned about statistics, because they gave me a job looking at all the lifetime histories of various types of bulbs, and how to evaluate the mean life of the various types, and so on. I guess that all led me from the pure reading of physics and quantum mechanics at the age of fourteen and fifteen, to seeing science at work and making science useful. It really put me into science, but with the idea that science can be useful, is useful, and that's what it ought to be. I also became interested in the process of invention and patents. I think those were some of the early impacts on my education. Oh, there were others.

Speaking of those Telefunken days, your talk about politics, and the interface with the Hitler politics, reminds me of another memorable experience. The Telefunken Company had a field unit in one of Berlin's suburbs called Gross-Zieten, which basically was just a big field. It was a lonely field station with some shacks or huts somewhere outside a rural village. They mainly had all sorts of antennas all over the place. I was told by my supervisor and ham radio friend, he said, "Look, why don't you go out there and see what it's like. You can work on your antenna-coupler, how it works on the antennas."

I went out there. I was in a room all by myself. I found a piece of paper that said that Telefunken had a radio license to be used for experimental purposes. An enthusiastic but frustrated radio ham that I was, not allowed—as a foreign citizen—to get an amateur operating license myself—I was just dying to get my finger on actually transmitting something—and connected with the stupidity of not realizing that this is probably not licensing me, but licensing the company, and so on—I thought, "Oh, what better way to test my antenna coupler but to put one of their big antennas on the air. There is a license with the call letters D2-something or other, so I'll just go on one of the amateur bands. I'll call somebody, and he can give me reports as to how it sounds when I'll make this or that adjustment." I did that. I was on the air no longer than ten minutes when the telephone rang. On the other end was a very tough and stern German military voice that said, "What the hell do you think you are doing?" I said, "Well, I'm just blah blah blah." He said, "Oh, you're going to hear from us." This turned out to be the military radio CIA-type watchdog station. I knew this was going to be very serious.

The next morning when I went in to Telefunken, of course everybody knew about this. My supervisor said, "Well, you're going to have to see Dr. So-and-So." I've forgotten his name but he was no less than one of the members of the Board of Directors of Telefunken. I thought this was the end of the line.

It turned out very differently, in that he was actually sympathetic. In fact, I remember him saying, "You know, I just hope the Gestapo doesn't get involved in this." I mean, he'd almost felt as much in trouble as I was. And he just told me, "Well, you shouldn't have done this," and so on. He was very mild, a wonderful person, actually.

In fact, when later I was ready to leave to the States, I gave him a call—or maybe I wrote, I don't remember which—and I said that, "Maybe you'll remember me, and so on. I'm going to the States." He actually gave me a letter of recommendation to one of the management people in RCA, just in case I would want to get a job here. Well, I didn't, actually. It was a kind of bad-turning-good experience.

BOHNING: You published some papers before you left, also (4).

WEISZ: Yes. That was the work—there were three, I guess, or maybe four, papers—that had to do with radiation detection, cosmic ray detection, and also some electronic circuit design that went along with it. I did that work at the Institute of Cosmic Radiation Research. The director, Professor Wolfgang Kohlhoerster had said that, "Look, you've got enough here to get started on a doctoral thesis." But I had to break that up, anyhow, when I left. So when I realized that I was going to leave to the States, I wrote those things up in separate papers.

Hans Geiger of Geiger Counter fame was one of my professors at the university. He was also chief editor of one of the German physics journals. When I saw him in class one day, I told him I sent him a manuscript. He was amazed. He said, "Oh, I've never had a student that young sending me a paper." [laughter] So anyway.

Actually, I left in March 1939. It was right after the Munich pact or agreement with Hitler, and we all felt that war was imminent. I went first to Pilsen to visit my grandmother, on my mother's side. She was my only close parental relative remaining. I felt that I should say goodbye before I go off, because I didn't know what was going to happen. From there I went by train to Switzerland—and then on to France to get on the boat. Actually, the first of the publications you mentioned came out just about as I left France, I mean, on the boat. It seems a roundabout way to get on the boat, but it all had to do with that problem of taking any money, any currency along.

There were two things that helped. One of my father's customers was a Swiss lady. He told her to keep it and "My son will pick it up when he gets on his way." That's why, from Pilsen I went through Zurich. I picked up all of thirty dollars, which sounds silly these days, but it was of course a lot more then. [laughter] I guess it was worth what might be now one hundred fifty dollars or so, or one hundred twenty dollars, but it was important to me for the trip.

Also, my good Professor Kohlhoerster had said to me, "I'm going to help you get some money out. I will make a request from—I don't know, some agency like our NSF—saying that you're going to make cosmic ray measurements for us on the ship going over." In those days, physicists were interested in the so-called latitude effect—how much the cosmic radiation intensity changes the latitude because of the earth's magnetic field. This way he actually got me officially another thirty or forty dollars. So I came to New York, I guess, with about sixty or seventy dollars, by the time I had spent some for lodging and eating on the way. [laughter]

BOHNING: How did you pay for the passage?

WEISZ: Oh, that was one thing that you could do. You could pay for outgoing tickets in Germany, in the currency there. I don't know why, but that's the way I remember it. So okay, you know, there was lots of intertwining here with science for having the ability to get another thirty or forty dollars. [brief interruption].

You'd better guide me along. Otherwise, I'll start to talk about too many different things.

BOHNING: Did you go directly to Alabama from New York?

WEISZ: Yes. I did after just a few days, actually. There was somebody whom my father knew who was a chemist, actually. Anyway, I spent a couple of days there, but then I went down to Alabama directly.

BOHNING: You got your degree from Auburn a year later, in 1940. Is that correct?

WEISZ: I guess it went into the next year, I'm not sure. Yes, I guess so. I went to summer school there and a semester. What happened was that I didn't know exactly what I was going to do for a few weeks. My problem was that we had a mismatch in the educational systems, here I was where on one hand I had a start on a doctoral thesis, yet by the U.S. system I had no degree, not even a B.S. degree. It was the lady of the house, Mrs. Inez Hartung, who was now a mother to me, who suggested I go to Auburn which was nearest to Cullman, and talk to them. I talked to the head of the physics department, who was Fred Allison. He was a fairly well-known physicist. He said immediately, "Come to summer school and let's see just what we can do."

Oh, the school insisted that I had a couple of required, one was a foreign language, of all things, and I could choose German. [laughter] Then there was sociology, and oh, American government and so many electives to get my credit points up to whatever figure was needed. So I took those courses. That got me into the next year.

BOHNING: Did you take your other courses in physics, as opposed to chemistry?

WEISZ: No. I did take chemistry. Analytical chemistry, I did. I took a course in aeronautics, of all things, too, and I think I sat in on organic chemistry. I didn't have a lot of chemistry in my formal education, let me put it that way.

In fact, I must admit I haven't gone to many classes. [laughter] In science courses I signed up for I did go to courses, because I often found I could—I shouldn't say figure it out, but I could study it out quicker on my own. As you know, in the formal course education that you get, eighty percent you forget anyway and want to.

BOHNING: In 1940, after you left Auburn, you went to the Bartol Research Foundation in Philadelphia. What were you thinking of doing, and how did you make the connection?

WEISZ: Well, I was still torn between physics and electronics, you might say. This came from my contacts with radio, the radio amateur interests as a teenager, the exposure to training at Telefunken. And, on the other hand my exposure and work at the Institute for Cosmic Radiation Research, Geiger counters, etc. Yes, and then, of course when I came to the States, Professor Kohlhoerster did two things for me. One was, of course, getting me those forty dollars. The other one was that he gave me a letter of introduction to his former colleague Victor Hess at Fordham University.

In fact, when I was in New York—we were talking about how long I was in New York—there were two things I did, two visits I made. One was to Victor Hess, who was very sympathetic and promised help. I also saw Dean [George B.] Pegram at Columbia University, who was already beginning to be involved in the nuclear program, and later in the Manhattan project. Of course, I had no knowledge of that then.

Well, there was one other thing I might mention. That is that after I basically ran out of my credits with the Hartung family, I was looking for some scholarship money. I don't recall the details, except that I stayed in touch with Victor Hess. What happened next was, that he wrote to Albert Einstein. The next thing, I had a couple of short letter exchanges with Einstein. He got me a five-hundred dollar scholarship from an organization I believe was called the Educational Alliance, but I am not sure, just to keep me going so I could get my B.S. and get settled. I had never met him personally, but I could see he was a very caring person.

Okay, so Bartol Foundation, that's where we're going. After I had my B.S., I wanted to get involved in research. I was tempted to either go for a fellowship—to get an American Ph.D. I actually got a fellowship offer from Compton, from A. H. [Arthur H.] Compton, who was one of the physicists, you know—two brothers. He was at the University of Chicago. He said that he would gladly take me on.

But I had another problem on my mind. That was that in the meantime, soon after I left Germany, my parents went back to Hungary—because, as you know and as I mentioned, war was imminent after that infamous Munich Conference. Within months after I left, they went back to Hungary. Then, when the war ended, Hungary became a Communist state. Anyway, I knew that I wanted to get my parents out of there sooner or later. So in this time of decision after Auburn, I was torn between going to further study, or to get a job and earn some money. Then also, I was so much interested in doing active research rather than to just study, that I was hoping to get a basically very heavily research-oriented job. It was at that time that Hess suggested that, "Well look, you were a cosmic ray researcher. Bartol Research Foundation is essentially the center point in the world of cosmic ray research." He talked to Dr. [William F. G.] Swann, who was then director of it and Swann offered me a job as his research assistant. That's how I got to Bartol.

[END OF TAPE, SIDE THREE]

WEISZ: You'd better lead me along. As I say, I talk and talk.

BOHNING: No, you're doing fine. You found yourself in Philadelphia, then.

WEISZ: Yes, right, actually in Swarthmore which is near Philadelphia.

BOHNING: You continued to work in radiation physics, basically.

WEISZ: Yes, exactly. Yes.

BOHNING: Now, I'm not quite clear on one aspect of this. This is an arm of the Franklin Institute, located in Swarthmore, correct?

WEISZ: Oh, yes, it was then in Swarthmore. It still exists, but it moved to the University of Delaware campus. It was financed by a private donation of the Bartol family, but it was administered by the Franklin Institute. So the Franklin Institute was the administrator.

BOHNING: It had its own facilities?

WEISZ: Yes. Bartol had a building on the campus of Swarthmore College. In fact, the building was most identified by this—that outside of the building on the right side, there was a big tower of about forty feet high which had a door on the bottom. It was a steel tower, and it was one of the important aspects of the research. The point was that there were cosmic ray measurements that were done under thirty feet of water. Thirty feet of water correspond to one atmosphere of earth—the whole stratosphere and so on, you know. This was equivalent to once more the amount of mass equivalent to the earth's atmosphere. I'm mentioning it because one of my first jobs was, actually, to do some cosmic ray counting in there. That is one of those experiences that had some impact on my scientific view.

I had to do radiation counting with several trays of Geiger counters, counting coincident penetration of several counters by individual rays. The way that it was set up, the frequency of such an impact being registered was something like one in a minute or so—not very frequent, but in the average only because these were random events. I really learned what randomness means, because every morning I had to go in and turn the damn thing on—well, the cameras—that would record each event. In order to make sure that everything worked, I had to wait for the first incident—event—to come.

At times, it turned out that I would stand there and wait and wait. A quarter of an hour would go by and I would decide, "Something is wrong. I'd better get out my equipment, oscillograph and all sorts of equipment and tools, and see what's wrong and fix it." Just as I'd get all this stuff together, bang, there was the event, snap [laughter]. So I really learned what randomness really means. While there is such a thing as an average, you might stand there for half an hour and nothing happens. Then the next thing, bum-bum-bum, you get something like that.

Sorry, where were we?

BOHNING: You were doing radiation physics at Bartol.

WEISZ: Oh yes, radiation physics at Bartol. That's it. [laughter]

BOHNING: However, the war started shortly thereafter.

WEISZ: Yes, the war started, that's right.

BOHNING: It had been going on in Europe, but then the United States got involved.

WEISZ: Exactly.

BOHNING: Did you manage to get your parents out?

WEISZ: No, no. I didn't get them out till sometime in the 1950s.

BOHNING: Oh, my goodness.

WEISZ: Yes, yes. It was completely impossible, of course, during the war. After the war, it was the Communist regime. Nobody could get out. It was the iron curtain. Well, it's another story, how I got them out.

Well, let's see. We were at war. I was at the Bartol Foundation, yes. Of course, when the war started, we began to have classified projects coming in. Immediately, I had a problem because I wasn't an American citizen. That led to a whole other part of the story. But as regards my work, regards the science and the technology here, I began to be put on projects that were related to the research under the NRDC—the National Research Defense Council—on projects, at least parts of projects, that were not classified. They had to do with navigation instrumentation. Also, prior to that, it had to do with some of the early work using X-ray crystallography and its application to technology.

It was an interesting project. What the signal corps and the armed forces needed, of course, was quartz crystals for radio transmitters—for stable frequency radio transmitter operations. Quartz was, mostly, imported from Brazil. The crystals had to be cut with great precision to the proper axes—mineralogical, crystallographic axes—and size and thickness, and all that, for the purpose of making these oscillating crystals for very specific radio frequencies. For that you've got to find very exact positions of crystallographic planes—axes. It was to be done by X-ray crystallography. A good x-ray detector was needed. I got involved in developing a Geiger counter for soft x-ray detection for these x-ray units. I developed the counter for that—I developed the thing—and it was manufactured then by a company called Phillips Metallics.

It was a logical continuation of my cosmic ray work, which before this had dealt mainly with radiation detection and specifically with Geiger counter design and methodology. Then and later I found a number of patentable methods for various Geiger designs and new applications. I wrote papers and also filed patent applications. I learned a lot about patents as a result. In fact, after I joined the Mobil Company later, one of the attorneys said, "Why don't you present yourself to an exam to the patent bar? Maybe you'll get admitted to the patent office," which I did.

Well, okay. We should be back to where? [laughter]

BOHNING: You taught electrical engineering at Swarthmore College for one year, didn't you?

WEISZ: Yes. I guess it was the time I still was not cleared for confidential or secret work. There was need to have instruction to signal corps trainees in electronics and radio engineering. Those were the two courses. I thought that was great, I could handle that. So I did that, and I taught these courses in the evenings.

This had an influence on my life, although not all related to science. Another instructor and I were both teaching the same course to two different groups of trainees. His name was Walter [W.] Felton. He was about to get married. He invited me to meet his wife, and her sister was visiting. Her name was Rhoda A.M. Burg. She's my wife now. [laughter]

BOHNING: That's interesting. Did you ever get a clearance to do classified work, then?

WEISZ: Yes, I did get a clearance. Again, this was another chapter for the memoirs. Well, clearance was denied, I think not just once but twice, by whoever was involved in that. In those days, the negation of clearance was always accompanied with a form letter that, in the last paragraph, said that, "You can appeal this if you can prove that you are not disloyal." Well, how do you prove that you're "not disloyal"? [laughter] This was an interesting judicial question, actually, that arose in those days. I talked to attorneys about it, and so on. My good friend and mentor and director of the Bartol Foundation, Swann, said to me, "You know what? I will do the following. You give me a list of all the people whom you can think of whom you've ever met since you came to this country. I will write to all of them. I will ask them about what they think or know of you, and we will put all that in." That's what he did.

In the course of that, I found out—after a very long time, unfortunately—what started the problem. At the beginning of this period I had visits by numerous investigators. One of them was an army intelligence investigator. He was a very rough guy. Well, he just came in, and he said, “Can I talk to you?” I said, “Sure.” We sat down, and the first question he asked was, “Are you a Nazi?” I thought, “My God,” you know, I thought he was joking.

Well, as I said, much later I found out that this man had been in Alabama, too, investigating me. It turned out that my sponsor who helped me come, Mr. [Philip G.] Hartung of Cullman, Alabama, who was a pharmacist, was also on the State Board of Pharmacy. He was involved in politics, and he was running for some kind of office, and his political opponents had spread a rumor that, “This man has even brought a Nazi into the country.” So that’s how this came about. But anyway, Swann got all these letters, and went in and got another hearing for me, and ultimately, I got clearance after that.

I don’t want to belabor that part of history, but there was another more humorous encounter with FBI investigators during that time. This one does have some science implications, as a matter of fact. Two guys came in, identified themselves as FBI agents and, “Can we talk to you?” “Sure.” They were very nice and civil. I remember them well, because they reminded me of a team—namely [Stanley] Laurel and [Oliver] Hardy. One was tall and was sort of the straight man. The other one was, oh, he was talking high pitched, like this, you know, and he was short and plump. We sat down in my laboratory. They were asking me all reasonable questions. While the straight man, if I want to call him that, was asking me some questions, the other fellow said, “Do you mind if I look at your correspondence?” It was filed there. I said, “No, that’s all right, go ahead.” He was looking through this correspondence while I was talking to Laurel. [laughter] I guess it’s Laurel, I don’t know. All of a sudden, exclaimed to his colleague “Hey, you know what? Here’s a letter from the boss.” There was a letter signed by [J.] Edgar Hoover to me. He was very impressed. [laughter]

Now, the way this came about was that during my Geiger counter work with very soft radiation I had this thought that there might be a very high-tech way of marking money bills—using a small imprint with this soft radiation, harmlessly absorbed by the paper. Yet you could detect it with a specific instrument. I had written to J. Edgar Hoover about it. He wrote back and said that this was not within his jurisdiction, that instead I should contact the Secret Service. That was the letter. [laughter] So, anyway, that was a kind of a nice interlude.

Anyway, I did get over that problem. I also moved to MIT then, during that time. It was shortly thereafter or maybe even before.

BOHNING: Did you move there because of their radar work?

WEISZ: Yes. Well, it was at that laboratory. It was called the MIT Radiation Laboratory. But I was concerned with the Loran development. Loran stands for Long Range Navigation. It had to do with development of a Loran trainer. We developed a device by which you could imitate on a large table the Loran of say, a certain large area of ocean or land. It had an artificial boat or airplane, certain mock transmitters located in the area. All real dimensions were scaled down to table dimensions by the ratio of light velocity to sound velocity, because all our mock equipment on the boats etc. were operating with ultrasound. That was the task at MIT, and ended up with an assignment to the Quonset Naval Base to install the first of these trainers.

BOHNING: Well, I think that brings us to the end of this tape.

[END OF TAPE, SIDE FOUR]

BOHNING: Well, of course, that brings us to how you made the connection with Mobil.

WEISZ: Yes, okay. You know, it was all sort of step-by-step. What happened was that the war was over. I was back at Bartol. At this point, I knew I didn't want to be in cosmic ray research. I had delved into a number of fascinating challenges in applied science, and I had been doing a lot of work with Geiger counters. It happened that the Mobil Corporation got in touch with me. I had published on Geiger counters by this time, my name was sort of connected with Geiger counters (5). Now in those days, it wasn't Mobil. It was Socony Vacuum Oil Company. Anyway, they wondered if I would be willing to be an expert witness in a lawsuit. It dealt with the use of some Geiger counter equipment for surveying in boreholes to test the geological formations, for so-called radioactive logging. Sure, I was very happy to do that.

I got to know their attorneys. I was impressed by them too—mainly because, here I was an expert witness engaged by them, and they never mentioned to me what they were trying to prove or disprove. So okay, I was an expert witness there. Then about a year later, I had a call from them, saying that they would like to know whether I would be interested in joining the company.

Basically, the idea was—well, it was expressed to me that way, and it was true. "Physicists have done a lot for this war. We wonder what physicists might be able to do for the oil company's technology?" That's how the connection occurred. I was offered the job with just that kind of open door of "what you may be able to do."

I joined their laboratory, which was in Paulsboro, New Jersey—it's still there; it was a process research lab next to the refinery in South Jersey. It was headed by Paul [V.] Kaiser. He was an MIT graduate. Years later he ended up being on the Board of Directors of the corporation. Anyway, the first day, he had me to his office. One of my questions was, "What do you want me to do?" He leaned over towards me, and he said, "Paul, I want you to do whatever you want to do." I thought to myself that it was a joke. I said “Well, you mean if I want to do cosmic ray research, you’re going to let me do that?” He answered in an absolutely serious way “If that’s what you want to do, you go right ahead.”

I was puzzled, but it didn’t take me long to realize what he was doing. He was, essentially, challenging me and testing me, testing my judgment and true interest to seek a path of relevant contribution to their technology and interests. It was a real test. After all, if I did not prove to be worthy of such trust, he could fire me. I often thought of this years later when I had the position of a laboratory head. By that time, and I can say this time, we had all kinds of—what is it called?

BOHNING: Affirmative action?

WEISZ: Exactly, that and the whole “department of human resources,” telling us strict formal rules to replace human judgment, rules about how you could dismiss somebody—which became almost impossible unless you had a three-year docket of what all he did do wrong. After all it’s hard to prove what somebody didn’t do and so forth.

Such rules are easy for routine jobs, the task is defined, you do it or you don’t. Easy. When it comes to judging the talents for an objective which is the search, the research of innovation, there are no simplistic rules for counting the pluses and minuses, for the staring, the hiring, and the stopping, the firing.

Now needless to say, we also have a drifting of corporate management toward domination by individuals who are not technically or scientifically trained, but either trained in law, or they have a degree in—what’s it called?

BOHNING: Business administration?

WEISZ: Business administration, looking for the best arrangement for profit NOW. “Never mind all this stuff about the future, which is research. We’ve got to have good management that knows how to manage people,” and all that sort of thing. Anyway, I think that the spirit of searching for as yet undefinable innovation capabilities was encapsulated in Paul Kaiser’s “Look, if you want to do cosmic rays, you go right ahead.”

BOHNING: Were you assigned to a group?

WEISZ: That’s another interesting point. I was not assigned to a group. But Paul Kaiser said. “I’ll tell you what I’ll do. One of the things that you might enjoy doing is, I’m going to let you go around and talk to the various people in the various departments and just find out what all is going on around here." I had a chance to talk to the heads of the departments—why were they doing this and what were their problems, and so forth. Beautiful.

So okay, but they did leave me alone. They did leave me alone, I played around with lots of things—you might call it. I was intrigued by the overwhelming role that catalysis played in the major and most processes. Kaiser came down to my office one day. Here was the director, coming down to my office. I was flattered. It was a lousy office—there were three desks in a space which was smaller than from here to there and you, almost. He said, "How are you doing" and all that. He commented that he "would be very happy if you really looked over this whole challenging business of catalysis." Well, that was a kind of official blessing of my thoughts. Somehow, it went back to a memory that also made an impact, I think, in my choices of careers. That was still in Berlin when I was a youngster. That was the middle teens—maybe sixteen, something like that. I walked past a bookstore, the kind you have near universities with scientific books, also popular science books. There were two books in the window that intrigued me. One had the word "Catalysis" in the title, and another the word "Indigo." I went and asked the chemistry professor in Gymnasium "What's catalysis?" He said what was in those days and for a long time, still maybe, is the definition of catalysis: "Well, a substance which sees to it that a chemical reaction will go, or will go fast. But itself, it's not changed." When I came to the oil company, here it was, one of the most important aspects of the industry.

I got to working with cracking catalysts, which were little porous beads of aluminum containing silica. These pearl sized silica-alumina beads were formed in a special plant, they're first made as a hydrogel–very soft materials like tiny balls of jelly. They're about this big. Then they're dried. In the process of drying, they shrank to the final hard, small bead of about two-millimeter size. Unfortunately, during this drying process a high percentage would split and shatter and go to waste. Why do they break and how could we stop that expensive waste of material? I decided that the very energetic splitting may have to do with the surface tension forces at the air-water boundary. I found that adding certain detergents to the water did indeed markedly reduce the breakage.

I provided a solution and caused a pleasant stir. They used the detergent. It worked. I was a success—at least I was for two days. Then the supervisor of the plant came over—mad as can be. "We can't use this stuff. There is foam all over the whole plant. You can't even walk through the plant." [laughter]

I could have given up. But I started to madly study defoaming technology. I mean study for just a few days. I found that a higher alcohol like decanol could quickly disperse that foam in a dish. The foreman wanted to see it work in his plant not on my lab bench. I walked over to the plant with a little bottle of decanol. Sure enough, the plant inundated in foam. I sprayed a tiny amount on some of the edge of the foam right where I was standing—and I was amazed myself. Not only did the bubbles collapse right there in front of me, but the break would just propagate and make a long path. The foreman was standing there, he was amazed. I was the genie with the little bottle. Just a little puff and all would clear before me. I was a hero. [laughter] That wasn't the end, though.

About a week later I got the word that the workers in the bead plant were ready to go on strike, because of the smell of the decanol. Now my great work was threatening union problems! From hero to failure. Not a good time to quit.

How to find another defoaming agent, one that didn't smell. Smell? Well, I remembered the times in European hotels where they used to have, oh, you know, these little bottles of bath lotion, after-shaving and such things, and in some cases, they gave you a little bottle of pine oil, which made the bath smell good. I had noticed that the moment you put some good smelling pine oil in, any bubbles that you had would disappear. So I tried the pine oil. It did the job, in the bead plant and I was a success again. In fact, someone from "headquarters" came and told me something like "My God, you're worth a hundred thousand dollars a month, for the savings in the bead plant," or some such to me very impressive figure.

Well, I tell you. To me, the important lesson of that you must left and right, beyond just one field of specialty. Few real problems are specialty problems. If after the first step, one had to now find a defoaming specialist, and after the second disaster, one had to find another type expert, or to give up. No real solution could be forthcoming.

So okay, well, perhaps we should move on.

BOHNING: It was nine years before the major event in your career—that is, getting into the zeolite business. I was curious as to what were the types of things you were looking at in that nine-year period before you got to the zeolites, especially in light of your telling me that you were free to do what you wanted.

WEISZ: Okay, right. Well, this goes back, actually, to a number of mixed stimuli of interest that probably were kicking around in my own mind. On one hand, at Mobil I had covered a number of investigations in catalysis, heterogeneous catalysis, with a variety of the then known catalyst materials. Before that, when I was still thinking about other fields that excited me I had also seriously thought of the challenges in biophysics, or the way I saw it the physics or chemistry involved in biological things.

I had a friend whom I met in Auburn, Charles Dwight Prater, a very bright student. He was gung-ho on chemistry, gung-ho on science. We became very good friends. He ended up going to graduate school at the University of Pennsylvania's Johnson Foundation for Medical Physics getting a Ph.D. in biophysics at the time.

[END OF TAPE, SIDE FIVE]

WEISZ: Around the time I joined Mobil, I used to go to their seminars—just evening seminars. It stimulated my curiosity about biological problems. I would read and I would listen to and be impressed by the chemical selectivity, oh, the specificities of the enzymes as catalysts, and how their specificity depended on their precise structure. It was in my mind when I became almost shockingly aware of the very crude chemical selectivity I found in the industry. There was and there still is that huge challenge, the gap between the selectivity that nature achieves and that I saw in the man made very crude catalysts.

Well then, it was somewhere in the 1950s, synthetic zeolites 4A and 5A became available from the Linde Division of Union Carbide. They were used for absorbing moisture. They also absorbed some light gases, and just certain gases. They were solid crystals, actually crystalline sodium and calcium salts of a silicon aluminum oxide.

Here was a material like my catalyst beads, could I introduce catalytic activity in them and combine that activity with the selectivity depending on that certain size or shape of molecules fitting into that solid structure? Imitating what enzymes do?

Once again, it's a case of being involved in one thing and yet exposed to other things and thoughts or exposed to this, and combining those two things—rather than staying with your accumulated special skills, period.

I don't know how much time you have. It's lunchtime, really. We can go and have a bite, maybe.

BOHNING: All right.

BOHNING: Well, before we broke for lunch, we were reaching the point of 1955.

WEISZ: Zeolites.

BOHNING: You had talked a little bit about some of your activities leading up to that. I had read that you learned about Linde's synthetic Zeolite A. How did you learn about it?

WEISZ: Oh, I think it was the literature.

BOHNING: Okay.

WEISZ: Yes. Oh, well, we actually got some of the materials in the laboratories for drying gases and things. Yes. That's what set me off. The only problem was, that the A-zeolite has this very small pore which only takes in only small molecules, normal hydrocarbon chains. So I led to the first thoughts of one, could I generate any catalytic activity anyway, and then, "Where could this possibly be useful in the petroleum industry?" Some time much later Union Carbide did put on the market another material, which had a ten-angstrom instead of a five- or four-angstrom aperture. That was the X-zeolite, but that was much later and had little to do with the shape selectivity work.

So I worked with the A-sieve. There was a time, after starting with this, that we were actually able to put catalytic activity into an A-sieve, and we were able to selectively react linear alcohols—and clearly differentiate between a linear alcohol and a branched alcohol, and we were even able to catalytically combust, burn with air, only n-butane out of a mixture with iso-butane, for example, or hydrogenate a butene without touching an iso-butene. But who wants to catalytically destroy alcohols anyway [laughter]. It was all very exciting, the new selective capabilities, but here I was sitting in an industry which, of course, is full of hydrocarbons. Their products are mainly fuels, as you know. Here was a chemically very unusual and new capability, that you could only pick certain types of molecules. Well, petroleum's got thousands of different molecules in it. Why would there be some practicality? How could you possibly use it? Most of the products are fuels, all you're going to do is burn the stuff, for example.

So I think one of the big efforts that followed—and that took patience and tolerance and cooperation by the corporation—was, to explore questions namely, "How in the world can we fit this chemically very basic capability into something useful in this industry?"

I was convinced that this was so basic that there must be *some* place that it could be used. So I held on to that. Yet it was difficult to plead for time, I'm not talking about any particular individual, but just generally, it was natural to have the reaction "Why in hell do you want to do that? After all, we use mixtures in stuff we burn. You're trying to put something very sophisticated in this." This having been said and done, however, let me say one thing. There was patience and cooperation by the company—I mean by my management. I suggested, for example, that, we form a study group, have, on loan, a number of engineers with experience from refining, from lube refining, from catalytic cracking, and so on—and form a little study group, to delve into what we need, what relationship we have between chemical composition and quality parameters and so on. So we could work with each other, actually by such an assignment—not just by casual meetings or committee discussion. I had that cooperation, I must say. We had a group of four or five people, sat down, and for I think roughly six months, we explored things. It was the beginning of the Technology Exploration Group, as we called it.

There were two things that struck a potential. Namely, one was the fact that normal paraffins are the lowest octane number components in gasoline and actually drag down the average octane number, therefore. So if we could just get rid of them, that would be an advantage to the remaining gasoline, provided you didn't lose too much the remaining gasoline volume—and what were you going to do with the eliminated part, that is its resulting products. This did lead to, actually, the first industrial process based on shape selective catalysis. It was called selectoforming. It was a case of using a zeolite catalyst with narrow pore structure, somewhat like the five Angstrom size of zeolite A, which would crack out only the normal paraffins of a gasoline stream and make propane out of it—which, in certain areas of the world, had a high enough price so that you could afford to lose that gasoline at the expense of raising the octane number of the rest of it. This was done in Europe, because of the selections in the market.

BOHNING: What year was that?

WEISZ: Gosh, I'd have to look it up.

BOHNING: I haven't been able to find the year when that happened. From 1955, when you started with Zeolite A, how long did it take until you had selectoforming plants operating in Europe?

WEISZ: Well, we published a paper well after the development and commercialization [6]. This paper came out in 1968, so the actual time of getting into operation was probably 1965 or 1966 or so.

BOHNING: It took more than five years.

WEISZ: Oh yes, absolutely. The struggle was basically what I was saying, that, "Yes, well, you've got something sophisticated, but where does this fit in?" Which means that I really—I and my colleagues—had to learn about what the chemistry was of some of the quality problems that we're facing, in even something as unsophisticated as a mixture that you burn. And that was just the beginning. Zeolite A was not stable enough to be transformed to a very active catalyst and stable enough. We needed to search and find an entirely new zeolite, and we looked then for the possibility of finding a natural mineral zeolite with suitable stability and structure. We had the help of company geologists in that effort, and we, one of my most active exploring colleagues at the time, Dr. Vincent [J.] Frilette and I would search for samples around the world. From those we had to learn what structures would be stable to transformation to acidic catalysts. We found that the silicon to aluminum ratio had to be at least a certain value, and so on. That was one phase of the research. Another was to find structures that would accept certain shapes and sizes. As a result of all this, and with help from the geologists, we finally came to mine and use a zeolite from the Nevada desert. The zeolite was of the erionite family. All that effort, involving a number of skills, people and, of course, time, then led to the next phase involving still more time and different skills and people, the development of the specific process.

Well, I mentioned that our group identified two basic process potentials. The other one was another interesting case. I'm not sure I can remember all the details, but we realized that, in lubricating oil, you had the situation where the paraffinic long-chain molecules that are basically waxy, reduce what is known in the industry as the viscosity index. It relates to the loss of fluidity at lower temperature. Let's say, you have a lube oil in your car which is working fine, with just the right viscosity in the summer time. But winter comes along, so the temperature goes down twenty degrees, normal paraffins start to form wax-like aggregates, actually, and you loose fluidity, that is a key lubricant property. Therefore lube oil stock must be limited in long-chain paraffin contents, and in fact for that reason the industry can use only lubricating stocks from certain crude sources, and then still has to put them through expensive additional dewaxing processes. They are expensive because they are solvent refining processes, batch processes, using up solvents, having a large investment cost et cetera. We saw that we could potentially look forward to a continuous catalytic process to dewax all sorts of crude petroleum stocks.

That incentive and potential rested on the shelves for decades. It was so long afterwards that it was reinvented, presumably by other people—or they thought they did. It's a fascinating case, probably a typical example of how innovations have to wait, even get forgotten until many factors come together to create incentives or even recognition of incentives. Until then the best idea must rest. Again, the world changed. One thing that happened was the oil embargo. The oil embargo really put a pinch on availability and narrowed the choice of just those very specific crudes suitable for lube oil production. This became an early, perhaps the first realization to the value of developing a lube oil selective and simple catalytic dewaxing process. Add to that also that existing processes are hard to replace as long as their investment is not depreciated. Also, of course, it helped years later, we had developed other processes and especially much more appropriate zeolite catalysts for this application as well. The worldwide replacement and installation of catalytic dewaxing is therefore going on as late as right now.

BOHNING: You commented that you had to keep people convinced that this was so basic, they needed to keep supporting its development. This went on for several years. Let me now back up a little bit further. You started in 1946 with carte blanche, as it were, from Paul Kaiser . What was involved in your relationship with Kaiser that kept that going, especially in the nine years before the zeolites came along?

WEISZ: Well, I don't think it was a relationship with Kaiser. It was the continuing lasting spirit of most of Mobil's management responsible for research, people that followed in his footsteps. I have said this before, that the spirit of Mobil towards innovation was, I think, unique—unique in the sense that you didn't have many corporations doing this at that kind of a level of basicness and search for innovation, except Bell Telephone, in those days Bell Labs. I felt that we were the Bell Labs of the oil industry at the time.

It was like "Oh, you do whatever you want." All right, okay, but it was important that you also began to learn what the other relevant parameters in the world were—what they were, how flexible or rather variable they are, their demands and restrictions. In another way of course, it's learning and understanding the market—an understanding of the relevancy of your chemistry activity to it, if you are dealing with chemistry or whatsoever.

Well, I'll give you one example for one such a learning experience from the time I became the head of Mobil's Central Research Lab in Princeton. Now this was—what was that? That was 1965, wasn't it?

BOHNING: It was 1969.

WEISZ: In 1969. You're right, yes.

BOHNING: You were manager of process research in 1967. You then became manager of the central research division in 1969.

WEISZ: In 1969. Okay, I stand corrected: 1969, yes. Well, we just had some changes in the top executive makeup—the board of directors—and who was going to be in charge of the research activities.

[END OF TAPE, SIDE SIX]

WEISZ: Where was I? Oh, yes. Okay, I took over the lab. Then, of course, I took over a number of projects that had been going on. One of the projects had to do with making protein feed additive by growing a yeast which could grow rapidly getting its nutrients from a certain wax fraction of petroleum. It was a yeast that was discovered, actually, in our Dallas laboratory by our microbiologists there. They found that this yeast would multiply at—I forgot the exact numbers, but instead of at the usual thirty-seven degrees centigrade level, it would work at maybe fifty degrees C, or some such high temperature. Since yeasts are high in protein content, they could be used as a cattle food additive.

At that time, the usual feed additives for additional protein supply had come, generally, in the market from anchovies. Most of them came from Peru. Well, the economics of this process of feeding this particular yeast in a big reactor with a petroleum hydrocarbon fraction looked very good. It was something like we do this for fifteen cents a pound while the current cost of anchovies was eighteen cents a pound, let's say. Besides, it looked like the anchovy market was only tighter because of the overfishing.

Okay, I had taken over this process. But our new executive vice president, Ted [T.W.] Nelson, he was a fine man and smart, but a tough guy. Many feared him, actually I admired him. He told me at one of his visits, he said, "Cut that out. I don't want that bug project, period." That's what he called the yeast project.

We had put a lot of manpower into it, a lot of time—even some pilot developments. We had patents at least applied for. I felt a natural resistance to giving up the project. In my mind, it was sound, it worked, et cetera. His answer was, "Look, we're not in the market of that kind of thing. We're not knowledgeable in this market, period." That was it.

I cut the project. It had some manpower implications, so it wasn't easy to do. I had heard that BP, the British Petroleum Company, had been working on a process for this purpose. So, I thought I might rescue some of the investment by selling a license. I took myself to London and discussed the fact that we had something and it would do so-and-so, and so forth. We had a good meeting, but it didn't go anywhere.

But I found out very quickly also that the Italian company, ENI—the state oil company—was also interested in feed supply, protein supply. So I went and contacted them. I went to Milan, and we talked about it. They seemed interested. I also realized that they had actually done some work with the BP process. I pointed out that one of the big advantages of our process was the high temperature growing condition for our yeast. A big plant investment goes into cooling the reactor and it is smaller the bigger the difference in temperature in relation to the cooling water you have available. The BP process was based on having the Scottish waters on the Atlantic. Here Italy wanted to do it in the warmer Mediterranean, where there's a differential disadvantage of perhaps ten degrees, then. I pointed out the great advantage to them that our organisms operate at the higher temperature. They were very interested.

So it led to a second visit. I was in Milan the second time, and we were discussing it further. At one point they said, "Have you had any field experience with cattle, with this product?" I said, "No, we don't, why?" The answer was the following. "Well, you see, in every region of Italy, every farmer who has a dairy farm is proud of the particular flavor of the cheese that he makes. We have to be sure that your protein additive will not alter their cheese flavor." Of course, we had no experience in feeding cows and make cheese, and have Italian farmers taste the cheese. I couldn't help but remember my executive vice president, "don't get into something where we don't know the market." This was a beautiful, personal example of that.

Now actually there was another corollary to that. They did build a plant in—Sardinia, is it? Yes, Sardinia. I understand it went for about two years, but went out of business. The price of the anchovies went down again, they weren't so overfished after all. [laughter]. So again, it's fine to innovate, but don't waste your time unless you're knowledgeable with the world that is to like it, the market for short.

BOHNING: In terms of the zeolites, when did you first realize that you had something that was exciting and new? Did you perceive this gradually?

WEISZ: Well, I guess I was excited from the beginning, simply because I thought that, "Look, somewhere we can use selectivity of that type in chemistry, period." I realized that maybe in the petroleum industry, it wasn't quite as easy. But I could see that in the long run, it had to be useful. That's why I urged forming the study group to examine more chemistry that our various petroleum processes must be relying on. Even if all was to fail with petroleum, I could see where in the chemicals industry, specialty special chemicals, say the pharmaceutical industry it should be possible to find a myriad of possible applications. I fact, when I retired from Mobil, I did work as a consultant for a pharmaceutical company as a consultant with that purpose in mind. But I don't think there was any particular single moment at which this occurred. Except, as I mentioned my lingering thoughts about the specificity of enzymes and the recognition of the disappointingly poor chemical selectivity of the catalysts I ran into was working together when the so-called molecular sieves appeared for drying gases.

BOHNING: No light bulb lit up in your mind, then.

WEISZ: No, except for that.

BOHNING: Yes. Did you coin the term, "shape selective"?

WEISZ: Yes, I did. In fact, after we had published a number of papers (7).

Reinhold Publishing asked me in 1966 to author a new entry entitled Shape Selective Catalysis for their *Encyclopedia of Chemistry* (8). So that made it official.

BOHNING: At that time, did you really know the shape of the cavity? You knew the size of the cavity, but did you really know its shape?

WEISZ: No, not at the beginning. But we all, you know, could make easy models of the shape of the molecule. And we just knew the size of the hole, the aperture. It was like having a hoop. What kind of animal will fit in? But then we began to study the zeolite literature, since zeolites were a family of minerals, and structural studies began to be studied and published for some time. So some time after we started to experiment, we began to be acquainted with cavity structures.

BOHNING: Did the term, selectoforming, refer to something like platforming? Who coined that term?

WEISZ: I coined it. I coined it because of our friend Val [Vladimir] Haensel who had coined the name platforming. For the platinum reforming catalyst [laughter]. I wanted to make sure that this became a petroleum process, too, and that it dealt with something that was already associated with gasoline and the so-called reforming process. So I suggested to the attorneys—in fact, I guess I probably even wrote a few paragraphs towards the patent application—and I said, "Why don't we call that selectoforming." So the term selectoforming went right into the original patent.

BOHNING: When you were able to convince management that this was something that needed pursuing, you commented that it triggered many reactions. The first of these was a program for synthesizing new zeolite structures.

WEISZ: Oh, yes, yes. Now I tell you, this is a good example, I think, of where your management, in this case the immediate management involved above me, had enough—call it sympathy, understanding, cooperation, what have you—to also be willing to undertake other aspects of research that would fit in with this whole thing. It came about because, well, I had started with demonstrations with A-zeolite, then the development using erionite. One limitation to what chemistries, what catalysis we could do usefully had to do with the size of the hole, the aperture of channels in these zeolites. If we had other bigger sizes we had more choices than just normal hydrocarbons. It would open up other chemistries that could be selectively done.

My management, my immediate managers—in this case, Dr. [S. L.] Meisel, and Dr. R.W. Schiessler who at that time were responsible, I think realized the situation, and just as cooperation by our geological groups had been initiated, they also saw to it that some inorganic chemists, who were interested in zeolites, would be encouraged to work on the synthesis of new zeolites.

That of course became very important, because the zeolites that had been available at that point—were only Zeolite X and later Y from Union Carbide and the narrow pore zeolites A and some natural mineral ones. The catalytic aspects of work started a tremendous amount of industrial development with Linde's X and Y zeolite as cracking catalysts, but these were not the shape selective catalysts. It took important new contributions in zeolite synthesis research by my colleagues in the catalyst development group to make a lot of new processes possible. Some work initiated by Dr. George [T.] Kerr led to a number of new ones. The work that followed led to zeolites with not four and five, but more or less seven-angstrom width in the channels. That led to the ZSM-5 zeolite and ZSM-5 process technology, which is growing in numbers of process applications still to this day.

BOHNING: Carbide had an active zeolite synthesis program too, didn't they?

WEISZ: Yes, Carbide had an active one. Of course, we didn't know much about there program. We just learned that they had made the A-zeolite, and later the X-zeolite. We developed the catalytic cracking that used their X-zeolite, and then later their Y-zeolite as raw material. Mobil wasn't in the business of catalyst manufacture, except for their cracking catalyst. For their zeolite modified cracking catalysts, Mobil got together with Union Carbide as suppliers of X and then with Y zeolite. But this was not shape selective catalysis. Mobil much later started to produce their shape-selective ZSM-5.

BOHNING: Am I correct that Mobil's interest in catalysts goes back to Eugene [J.] Houdry?

WEISZ: Yes.

BOHNING: There's quite an interesting little story about that whole legacy. Were you aware of any of that when you arrived in 1946?

WEISZ: Yes, I was aware of it. I was not knowledgeable about the details, of course, since Houdry had left Paulsboro before I got there. I actually met him later. He then had an outfit in southeastern Pennsylvania.

BOHNING: Wasn't it in Wayne?

WEISZ: It was in Wayne? No, it was on the Delaware River. Well, he may have had something there. He had Houdry Development Corporation in Marcus Hook, I believe.

BOHNING: Yes.

WEISZ: Let's see, I met him at the first International Catalysis Conference. Philadelphia actually was the first one. Gosh, I have to look and see what year this was. Yes, he was a nice man. I enjoyed talking to him, yes.

BOHNING: Another thing triggered by the demonstrations of new potential using Zeolite A was that the Mobil management initiated something called a process exploration group.

WEISZ: That was my group. That was the group I was mentioned, where I said, "Well, let's get about four or five people from the operating areas to sit down with me." Yes.

BOHNING: That was your first management job.

WEISZ: Yes, I guess it was, that's right. Yes.

BOHNING: How did you feel about working on the management side?

WEISZ: Oh, I enjoyed it. I enjoyed it, and there's something that I learned out of my experience. I enjoyed the work of fitting innovation into the bigger picture of needs and reality. If you want innovation by research, that is also needed at the management level, not just administration duties alone. Budgeting, yes, that deals with the overall picture, of course, and carefully dealing out responsibilities for parts of it. But when it comes to the usual diverse administrative functions you can't expect to throw it all on the same person if you want to manage creativity and interaction among many individuals.

I had support, again, from management, from Cy Meisel, whom I mentioned before. I recall the first such help I had was from Jack [J.J.] Wise who earlier had won one of those Mobil fellowships where someone could go back to school to get a higher degree. He had come back with a Ph.D. in metallo-organic chemistry from MIT, as I recall. He is now a Mobil vice president.

The same was true when I took over the Central Research Laboratory in Princeton. I had the very important help and cooperation of having an "administrative manager."

Such at least partial division of labor at the management level in research is very important. Otherwise, it's hopeless, maybe even disastrous. I see it happen at the universities, too. Especially when chairmanships are rotated, like everybody takes turns running the whole department's business. An extremely innovative faculty member with outstanding imagination and vision may become chairman of the department. The Dean may conclude he is absolutely no good. He may loose stature, etc. Even in a university, one could have a chairman and an administrative chairman, or a co-chairman, or whatever, to split the administrative from the intellectual leadership duties.

I read once, about the history of university structures in Europe there was a term which called the "academic dean". The academic dean had very little to do with things that deans do in our society, namely, mostly trying to raise money, what they call quote "development." But he was the academic dean who set the leadership in, "Where do we go intellectually, with 'what we teach, why we teach it'," et cetera. Regrettably, we don't have much of that anymore in the private university system.

[END OF TAPE, SIDE SEVEN]

Well, I'm going off on something else.

BOHNING: In that time period, you got an Sc.D. from the ETH in Zurich. How did you include that in your whole list of things that you were doing?

WEISZ: Well, of course, that goes back a little bit. See, my career at Mobil proceeded before the doctorate degree because of the screw-up and mismatch between systems that I went through. But I did want to revisit the academic world, and obtain a doctorate degree. Again, I had cooperation with my superiors. Bob [Robert W.] Schiessler, who was then head of research—incidentally, he was a former Penn State chemistry professor and, I think, chairman of the chemistry department. He was sympathetic with the situation. We already had the incentive fellowship arrangement in preparation at Mobil, and I was among the first to benefit from this privilege.

I had the opportunity to go to the ETH, the Swiss Federal Institute of Technology, the Swiss version of our MIT. I worked with Professor [Heinrich] Zollinger, he was the head of the department of chemical technology. He was an organic chemist who had done a lot in developing azo-chemistry and he was deeply involved in dye chemistry and dying technology. You know, it was and is an important branch of Swiss industrial activity. In our first contacts, he told me about problems involving the process of dyeing, of absorbing in other words various dye compositions on different types of fibers. I saw that here was a similarity between my experience in absorption of materials, like impregnants on various catalyst materials, how rapidly they enter, how they distribute, et cetera. So my thesis became a thesis concerning the mechanism of dyeing of fibers (8). I saw that this technology involved a mechanism where diffusion is coupled with simultaneous adsorption. I mean, you diffuse in—but some dyes will absorb more per entrance path than others, and so on. So the interaction between these two will affect the course of the overall speed of the dyeing process—how fast it is.

This way I got involved in dye chemistry—in the interaction of dyes, complex structures with various types of anionic or ionic or nonionic surface structures of various fibers. Actually, it led to the founding of one of the theories of dyeing, because prevalent was the theory that the way the dye gets in was that it absorbs while the fibers are in motion and therefore the motion of the fiber moves the dye molecules. I demonstrated—by using a cracking catalyst as a model, with dyes—that it involved ordinary diffusion of the dye molecules in water contained in the pores of the fiber, but accompanied by equilibrium absorption between the water phase and the fiber surface sites (9). It became known as the pore diffusion theory of dyeing. Actually it involved the development of the general principles of rates of sorption in any media when you have simultaneous sorption, in other words immobilization, also taking place on the walls of the media (10).

While this work was aimed at questions in dying technology, it was later the ground work actually for understanding the effect of shape-selective diffusion rates in the shape-selective catalysts many years later in 1980. Anyway, the dying experience and the catalyst experience had a lot of interesting basics in common, all related to diffusion phenomena that can be found across many areas of science and technology. I have enjoyed pointing out these common basic phenomena in many technologies, as well as in biological systems, even up to fairly recently (11).

BOHNING: I'd like to read a few quotes from your Perkin Medal address (12). You said, "Looking back, we see that a fundamentally new approach never fits quickly into established practices or perceptions. But if potentials are truly broad, the patient nurturing by management of such new skills is rewarded later in multiple ways."

WEISZ: In a way, I think I have alluded to this here and there in our discussions already, I didn't remember that quote there. I think those words are better than what I have said to you earlier, here.

BOHNING: Do you think that kind of patient nurturing exists today?

WEISZ: Rarely. Very rarely. I think that we have gone into the "what can you do for me today thing". I think even my hero and great past example, Bell Labs, has changed a great deal—I don't know how much. Let's remember that that's where the basics to the transistor created and the transistor technology was invented, which has transformed the world to space travel, computers, the information highway, and every nook and cranny of our daily lives! I remember the spirit at Mobil was like that in my time. Since then their organizational setup has changed. I just hope that the spirit has not.

It's very sad. What would help is to see to it that managements include talents of vision and insistence on tomorrow problems, recognizing that there is a percentage of revenue you could and must afford, and support the right kind of sub-management that will indeed think in terms of ten years, not in terms of the next quarter.

BOHNING: But is it true that you can't have creativity on demand, as it were?

WEISZ: Yes. Well, to some extent you can to obtain creativity for improvements, sure. You can say, "Look, I can give you ten hours to see if you can shape this screw in such a way that people don't cut their finger on it or something." "Oh, okay. Yes, I'll probably find a way." But it's still a screw, and it's still a finger, and it's still the same purpose—except it's maybe a little better for one reason or another. But creativity for innovation is different, yes.

BOHNING: One person whom I've interviewed told me that, after he had made his creative discovery, he wanted to follow it into development to see it through to its final marketing. However, the management would not let him. Instead they said, "You're so great at coming up with this idea, we want you to go back and come up with another one" (13).

WEISZ: If you can find somebody else who can take over, that's all right, I think—if there were some kind of transition possible, an interactive transition, which is important. You can't just say, "Well, go back to your lab. I'll find somebody else in Dallas, Texas, or in the other lab section who will take this on." There's got to be transition, there's got to be interaction, interaction, personal, I mean, and that means active brain overlap between the scientist and the next developer in line!

BOHNING: What about in your case, with something like selectoforming? How involved were you in the stages that led to seeing it to the plants in Europe?

WEISZ: Yes, yes. I was involved. Now, let me put it this way. I didn't travel to Europe to talk to somebody about the market aspects of doing it there versus in the States—you see, that also was an important consideration. Also I did not have much to do with the process design. But the important thing was that I was informed about these matters, there was interaction of knowledge. There was overall and interactive strategy. But again, let's face it there were many, many great people and parts of the organization involved.

Without them there would be no commercial process. But lots of it is inter-departmental, inter-organizational, even between Mobil Companies, in the U.S., in Germany et cetera—it's up to good management to see to that all this is done. So I cannot say that I had no help. I had help—or, I should say, I helped them, at least at the beginning of the avalanche. We all had pieces in the case of real innovation.

Now, the big lesson was and is, compartmentalization is one of the key problems to control in any large organization. Unfortunately I find that it's true at the university, too—everywhere. It only hinders, but, of course, one must have some kind of organizational order, like an orderly filing cabinet, which does mean organizational compartmentalization, yet at the same time you must have human brains that can play across, inspire having the right pieces of the filing cabinet pulled together to serve a common objective.

BOHNING: I would like to quote another thought from your Perkin address. "At the same time, if a single scientist's findings are to result in substantive progress, he must share effective interaction and creativity and hard work with many other individuals across the wide spectrum of management and staff."

WEISZ: Yes. Well, this is the other part. You know, I talk about what management should do. That is only part of it. There is the other side of the individual who expects to be productive. Often when I interviewed a candidate—he or she would tell me, "Here is what I have been doing in my thesis, and here is what I'm interested in." Period. Or, in exploring some thoughts and I might say, "You know, I wonder what is the possibility of such-and-such if we used a naphthalene molecule" and I would get the answer, "Well, don't ask me. I'm just an inorganic chemist."

But this in my mind goes back, actually, to our conventional university training, and the fact that we're segregated—whatever we do—even from the very beginning. "You're going to get a Ph.D.—in what? Organic chemistry? In inorganic chemistry? Biochemistry?" That's it. If you go take courses in any subject, well, usually those courses are not designed to give you information of more general usefulness. They'll be detailed sophisticated with depths you will probably never use. And when you are taught basic things like, say, thermodynamics, it stops there without adequately relating some of its relevance to the many things in the world and experience, and show how they apply to them. I think this is one of the problems we have in our educational system.

The journals, you take the journals. I mean, how many interdisciplinary journals besides *Nature* and *Science* do we know? The *New York Times*, maybe? [laughter] But that's it.

BOHNING: In your Perkin Medal address, you put forth the concept that, “Molecular structure-selective science will become applicable to all fields of chemical activity.” How far along the way are we to that prediction that you made ten years ago?

WEISZ: Well, we are slowly getting there. That’s an interesting question to look at and analyze. Molecular structure selectivity, is that what we’re talking about? Yes, okay. Well, we are well on the way now. The first step, we have gone from the petroleum industry into the petrochemical industry with shape-selective catalysis. For example, the production of paraxylene, I think that the raw material for any polyester, you may wear some of it, relies on paraxylene, and we are probably approaching where there is a fifty fifty chance or greater that your material’s paraxylene came from one of many of Mobil’s shape-selective catalyst processes in any of our continents. There are many other shape-selective processes like styrene production, or cumene. Now, these are still hydrocarbons. When it comes to other chemicals, that is the next step right now.

The pharmaceutical industry is a natural for that. What I discovered is that there is a very slow development in the pharmaceutical industry. There is a hangover of homogeneous catalytic chemistry and a very slow understanding of the practices of heterogeneous chemistry—I mean heterogeneous catalysis. For example, I look at a pharmaceutical industry. I find that anybody, who does any, let’s say, hydrogenation, always does it in a vessel, not with a catalyst in a flow reactor, no, they do it in a—what do you call it?

BOHNING: Do they still catalyze in a batch process?

WEISZ: In a batch process. Batch processes are still all over the place. The catalyst—and thereby, the ability to go in the direction that we were talking about—has not quite made it yet in the pharmaceutical industry. I think it takes generations, simply because look, if you have a laboratory full of batch-processing people who have never done any other work, nothing happens until somebody else gets hired or some odd person starts to do want to do it differently. Then he has to battle everybody else.

There are prejudices—well, I shouldn’t say prejudices, but traditional conceptions—like, “Well, you see, in the pharmaceutical industry, we don’t work on a few products in large quantity, like the oil industry. So we’ve got to have flexibility, running one thing, then later something else. That’s why we’re doing it in batch.” That’s a misconception. They don’t realize that with a catalyst, in a flow system, it’s just as easy to do something else tomorrow than today. But they haven’t experienced that yet.

Also, we’ve got to understand that propagating change in their process technology is a lot more difficult in the pharmaceutical industry, because the pharmaceutical industry is not the kind that publishes as liberally, like some of us in other industries. In fact, there is even a tendency to depend a great deal on secrecy rather than on patents—which also doesn't help.

[END OF TAPE, SIDE EIGHT]

WEISZ: All these are handicaps to innovation. But aside from that there are definitely strong forces pushing in the direction of seeking shape and structure selectivity. Certainly the appreciation for the great importance of structure is growing in biotechnology. Why, when it comes to pharmaceuticals, take, for example, that disaster with what was the name of it that caused newborns to be totally deformed?

BOHNING: Oh, thalidomide.

WEISZ: Yes, thalidomide. That was a disaster. Yet years later, they found that what was involved in having that disastrous property was only one of the optical isomers that was doing this. So all of a sudden, this generated attention, insight, alarm, about the importance of molecular structure now even to that further degree of more subtle structural importance.

BOHNING: We had passed over this earlier, but I wanted to ask you about when the Princeton lab was installed. I understand that there was a big shake-up, and after that, people moved to different places. I believe you went to Princeton. But I don't know what year that was, and I don't know what the magnitude of the rearrangement was.

WEISZ: Oh, it was 1969. Well, there were shake-ups every so often. [laughter] I mean, little ones. I must say that Mobil was always very conservative on shake-ups, too. We saw Exxon for example having swings, including building a huge new laboratory complex and adding manpower some ten percent per year, or whatever the number was. At Mobil, we thought that that was crazy. They couldn't possibly absorb those additions—and they didn't. Finally, all of that expansion collapsed. But no, talking about the word "shake-up," I think well, you know, an individual may feel shaken up if he has to move his family to another location, that's a big shake-up—even if organizationally there is no dramatic change. [laughter] But I think that in the overall picture, Mobil didn't have any significant shake-ups.

There is a corporate tendency—of trying to trim the people who don't generate things for today or tomorrow. That is hard on research for innovation. Business makes efforts to quantify and evaluating the worth of research in terms of some numbers, in order to decide, "do we really want to do this, spend money for this?"—research, I mean.

The businessman, the MBA, the economist knows the value of things by demand and supply. You take one and divide it by the other. Demand and supply. Demand over supply gives you the value. The problem that you have in research is that it's easy to say what's the demand now and what's the supply now. But to get what that ratio is in the future is an unknown, especially the demand, is not possible

Another way to put it is cost-benefit ratio. Again, you have the same problem. "Cost today? The research expenditures. Fine. What's the benefit?" "Well, I don't know yet, because I'm working for six years on it or more." There is no formula you learn in the MBA course for the benefit of a basic innovation. There it takes imagination as to what is on the denominator. It takes imagination and experience, it is the sum of both that produces vision. Conviction alone, and a fine record for an MBA degree are helpful, but do not provide the stuff.

BOHNING: You left Mobil in 1984. You would have been sixty-five then. Was that predetermined by the company, or by you?

WEISZ: No, that was predetermined by the company. My fate in life has been that the rules of retirement age always change the year after I have retired. This happened at Mobil. Then I went to the University of Pennsylvania. It was the same thing happened there—at seventy. Now, apparently, you don't have to retire. But unfortunately, that was a few months after I had to. [laughter] Yes.

BOHNING: Why did you go to Penn?

WEISZ: Oh, well, I guess it's like this. I can't just go fishing. I need to do something constructive. It seems to me that the more experience you accumulate the more useful you can be with that experience, so why stop in your tracks just then, especially then?

I had offers from oh, three or four universities in the East. I didn't want to go very far. Well first of all, I wanted to still be involved in science. Another thing was an old interest that back to before I went to Mobil, I was always interested in the biomedical arena. So I was really looking for, "If I go to a university, I want an opportunity to do research in the medical field, biomedical field." When Penn offered me to join the Chemical Engineering Department, I told them that I'd be interested, provided that I can devote myself mostly to interacting with the medical folks. Joe [Joseph] Bordogna was dean at the time. Joe said, "Oh, that's wonderful. The doors are open. Talk to anybody in the medical school," and of course, Penn has a fine Medical School, which my other offers did not, and there was the additional incentive, I didn't have to move. [laughter] I mean, we were living in Bucks County at the time, so I could drive in every day.

That's why Penn. Of course, what soon learned then was that it's easy for a dean to say that doors are open, but then how do you go from there to do the research you hope to do? There comes the task of spending most of your time writing proposals and going down to NSF and NIH to convince somebody to give you money, and perhaps for something that is completely strange to them. And here I wasn't someone who had the usual time of years of learning how to please the funding people.

It was a very instructive experience, but I don't know if you want to get into that.

BOHNING: Sure, why don't we detour for a moment.

WEISZ: Well, I was on my own. I had no experience with the basic ins and outs of getting proposals up to be favorably received. Moreover, I found it especially difficult if not hopeless in some really new innovative terrain, here the examiners are unfamiliar and there are really no great so-called peers. After all, by definition, if you try to innovate something quite new, there are no peers. Well, there was a well grazed area, of course, that I knew well and NSF also, that was catalysis. There I managed to get a grant for graduate research in reasonable time. That worked out well. But really I wanted to do things that bridged my experience to medicine. And that was a very different matter. Let me tell you about the first stimuli and excitement.

I had a call—this was actually just toward the end of my career at Mobil—from, Eugene Roberts [Beckman Research Institute of the City of Hope, Duarte, California]. He's the discover of GABA, one of the neurotransmitter substances.

BOHNING: Oh, yes, yes.

WEISZ: He called me, he saw a paper I wrote in *Nature* (14) about the very high catalytic activity of aluminum atoms in silica. Now, here's an example of looking across the border to another discipline. He was a neurochemist—I mean a neurobiochemist. He was reading *Nature,* and spotted an article I wrote on catalysis on an inorganic oxide, namely our zeolite. He told me "That's very exciting. We'd better get together, because there might be a connection to Alzheimer's disease." That was intriguing, of course, I went out and we got together.

I learned that, in Alzheimer's disease, for years—for decades—it had been known by the medical people that whenever they sent brain tissue of an Alzheimer patient who died for elemental analysis, the trace aluminum content was always higher than for normal brains. There was always something about aluminum that correlated with Alzheimer's disease. The next thing, people at Cambridge University in England had done an NMR study of the so-called senile plaque, which is the only physiological anatomical symptom that you could find in Alzheimer's brains, and found some aluminum signals in there, and not only aluminum, but also silicon. That's what alerted him to ask himself, and then me, "Could this possibly have something to do with the chemical activity of this stuff?" Well, we had, in our laboratories at Mobil, done many NMR studies, and we knew that the catalytically active aluminum associated with a silicon lattice showed up as a distinct peak in the NMR spectrum, in addition to the usual aluminum signal. When I looked at the Cambridge publication, my God, that plaque from the brains had this catalytic type of aluminum in it.

So we talked about this. I actually got started on working on Alzheimer's disease with the idea that look, if aluminum of that kind can crack the robust carbon-carbon bonds of a hydrocarbon—and here we have these delicate amino acids, proteins and a variety of biochemicals with all kinds of more delicate chemical bonds—why, that can raise Cain! This aluminum could indeed influence the chemistry around it. Also, what is this silica-alumina material, where does it come from et cetera. For example, the NMR indicated that the silica-alumina ratio was one-to-one, which to us in the catalytic lab always points to a clay. Well, dust is largely clay, does it mean it is dust, traces of which get up the nose to reach portions of the brain. So well, one thing led to another. I started a little program to follow that up. I did show that, a sugar, or a peptide can indeed be transformed and altered by silica-alumina catalysis. The work didn't go on very long, simply because I got involved in something else, which also was medical, which became very exciting, and made the *New York Times* and all sorts of other press later. But anyway, this is the way I started into the biomedical area. I regret to have left the Alzheimer work, because it still is intriguing. I must say, like in relation to many medical problems, there is such frantic dedication now to the DNA related biochemistry that there is a tendency to relate every problem solely to that. We are now tending to ignore the fact that the body, after it has its initial instructions via RNA/DNA, still has to do all sorts of let's call it simple chemistry. We should follow those things, too.

BOHNING: The other item you're talking about is angiogenesis, then.

WEISZ: Yes, right, right. It was the catalysis connection that got me into that Alzheimer problem, but this one, you know, is a case where shape selectivity came up in my mind as the driving stimulus. What happened there is that Judah Folkman at Harvard, who is, I think, one of the foremost medical researchers in the country—in fact, I thought him to be a logical Nobellist last year, but he didn't get it—had just published a paper (15) which caught my attention. Yes, that was in *Science* again. He had in the seventies demonstrated the role of angiogenesis in cancer growth. Angiogenesis has to do with the spontaneous new development of blood capillaries. A tumor will exude a growth factor which, when it diffuses to a blood vessel, will cause that blood vessel to sprout and grow new vessels towards the tumor. So it creates its own blood supply system to obtain its nourishment which supports is further growth. He had proposed that, if we could only find a way to stop angiogenesis, we can stop tumor growth. In fact, I was still at Mobil when he published some of those papers in which he demonstrated it (16). He wrote very beautiful papers that even I could read. He set out to search for a way to stop this phenomenon of induced angiogenesis, as a real exciting approach to stop cancer growth.

Okay, so anyway, just around the time that I went to Penn, he and a couple of other people discovered that if you combined heparin with hydrocortisone in a certain ratio, it would inhibit tumor angiogenesis. That was the first time anybody had seen the possibility of actually doing this. That was very exciting, because it might be a rather universal approach to cancer treatment or prevention.

Well, first of all, I had followed his earlier work, including his observations that he could induce blood vessel formation by a small implant of cancerous tissue. He had described these experiments which he made in and near the cornea. There was a case where the effect dependent on location of the implant relative to the nearest blood vessel, and I had supplied to him an explanation for that. I realized that this was strictly a diffusion problem. I talked to Judah about it, and with a colleague, John F. Marshall, at Mobil, we wrote a small paper about it (17), and that is how we got acquainted.

Then when he came up with his paper of the first success of inhibiting the angiogenesis (15), I noticed a couple of things from the chemical point of view—namely, here he used hydrocortisone, a hydrophobic small molecules, and heparin which is a long chain of sugar units, with lots of ionic groups—sulfates, phosphates, and others on it, like carboxyls. But in water, the long sugar chain takes on a helical coil format, with the ionic groups sticking out outside. Then the inside is hydrophobic and could bind hydrocortisone in there, as it is itself not water-soluble. I knew that cyclodextrins, which are small cyclic sugars , that have the form of doughnuts have a hydrophobic hole which, in fact, is the same size as the channels of the shape-selective ZSM-5 catalyst we had [laughter], and I knew that that hole size could take in a molecule the size of cortisone. So I suggested to Folkman, "Ha, what we've got here is the heparin carrying the hydrocortisone in its inner helical space. Why don't we, instead of taking heparin, take cyclodextrin. So it may be just that this will carry in the water phase to wherever it's needed." That wasn't just of academic interest, because heparin is also a powerful anti-coagulant, that's its major use and fame, and the high required dosage would actually be dangerous and not be acceptable.

So we tried it. But it didn't work at all. Okay, but it was the stimulus for what turned out to be an important beginning of new insights. After all in the cyclodextrin we had, if anything, an enormously oversimplified heparin. It was a really just a small and bare saccharide molecule. It was rigid instead of a long flexible chain, had no substituents at all, while heparin had a lot of varying kind, and in complex if not irregular sequence. So, why don't we take our simple cyclodextrin and start to add one feature at a time and watch? Well, as a first step, we put a lot of ionic groups on it, but just one kind, sulfates. Without going into lots of detail, it worked. Judah [Folkman] called me excitedly, when he tested it.

So, we published that later in *Science* (18). It made the media, in many countries (19). There were funny articles about how a surgeon and a quote "oil chemist" got together and did this, and so forth. [laughter] Anyway, that started me to explore generally the properties of this sulfated cyclodextrin—which became really a simple and unique heparin mimic—for a large variety of cell biological properties. The excitement about it is that it modifies cell behavior in many ways and is potentially relevant and useful in relation to many cell-related diseases. We have recently published a brief summary of this (20). The work has propagated over a number of colleagues in many disciplines and departments at the university to whom I am grateful.

Just now I have a project with cardiovascular surgeons, making use of it to inhibit restenosis of blood vessels that too frequently occurs after angioplasty (21). The damage that is done by that procedure can start the proliferative growth of certain cells, that creates the restenosis, it again narrows or closes the vessel, just what you were trying to repair. There are now almost too many other potential applications for our materials now, all to be followed up in greater depth, and in many medical disciplines.

[END OF TAPE, SIDE NINE]

WEISZ: You know, before we leave this subject, let me just comment on another item which is important in exploration and innovation. I said that the beginning of that work was inspired by my involvement in shape-selectivity, it was the assumption that cortisone fitted into the heparin helix. Now, after all the work and new leads and potentials that have developed, I can tell you that the inclusion theory, the fitting of one molecule into the other, was a wrong assumption. But the idea was the driving force that led to trying something, which we wouldn't otherwise try, period. That is an important factor in making new discoveries.

BOHNING: At this point, let's look at our agenda (22). I think you may have covered most of what is on there, but there are a few things we need to touch on yet.

We've already talked about management, and we've talked about innovation. We now come to teamwork, both from a participant's view and from a manager's view. How important is it? I would like to know how you handle somebody who is very innovative but who says, "I want to work alone," or "I don't want to be part of a team as such."

WEISZ: Well I think, you know, one thing that we might think about or talk about is that as always, when we use certain terms, it raises very specific images. Sometimes they're misleading. Here's what I mean.

I think that the important thing that leads to successful practical developments of something broadly new is interaction. This could mean teamwork, but not necessarily—because to some people, teamwork means that there are several people knowingly hammering at the same thing at the same time. That isn't quite what I have in mind, actually. What we need for innovation is interaction that can take various shapes or forms. The most important thing is to exchange knowledge, to be willing to listen and learn from each other. Yes, talk to each other. On the other hand you are talking about the innovative person who says he "wants to work alone"—that's all right for his part of the work, but he's got to have the desire and the ability to interact. After all, gee, let's take it to the limit. The complete non-interacting loner, nobody'll ever know about his accomplishment. He'll die, and that's it.

For any useful impact, progress for society, it's the interaction that is a key necessity. You don't have to have the people in the same room. But must have willingness and ability to transmit and receive information across individuals of different disciplines. This is a real problem in many branches of our society. That goes for industry and that goes even much more so for academe.

To a large extent, it is management people at all levels that must induce interaction, by a variety of mechanisms, always insisting on its importance. When it comes to R&D and innovation, that interaction cannot just be pro forma, it must attempt intellectual interaction, understanding in the course of it. So, the management levels that are to influence that must itself be capable of that.

BOHNING: You just touched on the next question I had, which is, the networking with peers—both inside and outside your own company.

WEISZ: Yes that's right. Certainly inside, and hopefully outside—to the extent that you're not constrained by needs of confidentiality due to competitive activities.

BOHNING: That's why I had asked you before about how you interacted with the synthesis program at Carbide. Obviously, you were competitors to some extent.

WEISZ: Yes, but only at some point much later, after much of our developments, and then only in a very limited way. After all, in that particular case, we were long term practitioners of catalysis and catalytic processes; they did make zeolites, like A-zeolite, and then the X-zeolite, not for catalytic markets, as far as I know. They were trying to break into making, manufacturing some particular catalysts, after we had begun to pursue the use of zeolites in cracking catalysis. Now when they made the X-type zeolites and Mobil developed their application in their catalytic cracking catalysts, there followed some commercial interaction between them. I don't think therefore that we were actual competitors. Of course, each of us was active to some extent in publishing scientific papers.

BOHNING: Along the same lines, you'd published a lot of papers from an industrial laboratory. What was the attitude of the company toward that kind of publication?

WEISZ: No problem. Basically no problem. Things were handled in a very orderly manner, in general publications were delayed until some patents were applied, just delayed, and that's all. Oh well, some people would fuss because of the delay, of course, but this is a fair and logical way to preserve both the traditional rights to publish and at the same time provide commercial safe-guards. I think that in some industries publication is a tougher problem than in others. Like, my impression is, in the pharmaceutical industry, it would be very difficult to have much flow of innovation out of their organization. I found, as a consultant later, that there is great emphasis on secrecy, far beyond getting patents, or even instead of disclosure by patents. The reasons are complex.

I think a lot of people who come out of academe don't understand the patent system, not even the philosophy of it. I think this is unfortunate. I spent a lot of time, as a manager, getting attorneys and the inventors together, let them understand each other, that it's not quite the same that each of them is interested in. The scientist is proud of new knowledge, the attorney is interested in exactly what specifically you use it for and how, in other words with "what you do with a discovery, not what it is." Already at the universities, most researchers don't know these things. Some even hate, yes hate patent lawyers. This is a sad situation. And we have no current mechanism in our education system to simply and routinely teach the basics of that bridge. It's another case of separated or isolated disciplines, if you will. I can see teaching the basics to that bridge in two or three class sessions, at most.

BOHNING: I'd now like to touch on changes in the company R&D support during your career (22) and also like to focus on the next question, which is, the attitude about the changes in the company attitude toward R&D.

WEISZ: Well, I think we've sort of covered that. I feel like I've been very lucky during my career, and as I told you we generally had a continuing good spirit toward forward research, but I have seen the forces at work towards less R and more D, if any R at all. Some of these forces or questions are always present. But let's look at the general problem in the industries we have today and tomorrow.

It's a social disease we have grown for a certain block of years of "the MBA is what you want, what you need." It was so overdone, but it's still hanging around, it's still hanging over. This idea of learning to manage, manage, manage, forgetting that we must also learn the multitude of things that may need to be managed.

BOHNING: Number fifteen, what is important for the future vitality of chemical R&D? I don't know if the word vitality is on your list.

WEISZ: No. I haven't seen it as such. [laughter].

BOHNING: I inserted it later when I re-read the agenda. More to the point, what is important for the future vitality of R&D—or is it already more abundant? [laughter]

WEISZ: Well, let's see, what do you mean by vitality? It depends on what you mean by vitality. When would it be more vital? If it did what? [laughter] I don't mean how—I mean, I think you're asking me how. Therefore, what is important to do.

BOHNING: Right.

WEISZ: It depends on what we mean by vitality. I think it's pretty much things that we have touched on here and there.

BOHNING: Okay. What did it mean to you to win the Perkin Medal? I know you'd received other honors, both before and after receiving that one.

WEISZ: What did it mean? Well, I tell you. It felt good to have before me a broader audience, including CEOs of major companies. I felt good about the fact that there is a means for recognition for innovation at such broader level—also recognition for useful innovation, not just for knowledge alone. Also, it was an unusual opportunity to pass on some general philosophical things, that I wasn't just going to tell them about shape-selective catalysis (12), but things about the process and adventure of useful innovation. It's an opportunity in that sense. I don't know how much impact it makes, but anything makes a little bit of impact.

BOHNING: I don't think I had the National Medal of Technology on my list. When did you receive that?

WEISZ: Well, let's see, that was in 1992. Yes, that was 1992. I received it from George [H. W.] Bush.

BOHNING: Wow. How did you react to receiving that?

WEISZ: Well, very similarly, I guess. It was a good feeling to be there—to be, you know, honored at that level, and to be cited for a specific discovery and its results, which was even quantified in the citation as having produced products worth billions of dollars.

It was very good. I must say that aside from the affair at the White House, the rest of the program was not usefully organized, if not disappointing. We were told that there would be a technology symposium. It turned out, we were to listen to speeches by government people, the head of the National Bureau of Standards, the Science Advisor to the President and others, with media, television and press swarming around. Oh yes, there were five minutes left for questions or comments from medalists. [laughter]. The detailed arrangements were and I suppose still are handled by a National Technology Medals Foundation. It was a media show. I hope they do better now.

BOHNING: I'm not familiar enough with this, but is there any monetary amount attached to the National Medal of Honor?

WEISZ: No. Not at all. I got a nice picture of President Bush shaking hands with me, and a citation, and then the medal itself, of course.

Well, you know, that brings me to a related topic. After all, what the medal and that Foundation is supposed to do is to motivate and stimulate people, the country for innovation. That media show does not do that. But there are potential ways of making an impact, including by that Foundation.

Take the system of grants that are given by NIH, NSF, et cetera for supporting research, presumably for innovation, of course. The present system does not support intensive innovation. The so-called peer system of judging a proposal is like Macy's wanting to put on the market a brand new piece of merchandise and having to get the approval of its peers, the Gimbel's, Wannamaker's, etc. Let's just remember that the peers are subject to the same pot of funds. In addition, it is so natural, so human, for all of us to have positive reactions and enthusiastic support for those things that fit neatly in with what we have done ourselves, rather than for someone proposing to leap away a big step that.

Besides we have a system of project managers in the funding agencies with clearly specified boundaries of the fields which they are to judge. The greater the innovation proposed the less it fits a defined work area of the say NSF project managers. Some of them also are just trainees, young, for a short time.

So, in summary, if we are to have decision makers regarding the support of true innovation we must have individuals with broad experience with innovation, and wisdom too. Where do we find such people that we could invite to participate in the work of the funding systems? There are the elected members of the national academies, and there are the National Medalists, and others. There are effective tasks for the Foundation, and others, that's a thousand times more relevant than media shows.

BOHNING: I've come to the end of my list. Is there anything you'd like to add that I haven't covered? I know there are many other topics we could talk about. [laughter] Also, you know, we haven't exhausted the possibility that we could set up another interview.

WEISZ: No, well, we have covered a lot of ground. I am sure we could go on and talk about many other things.

BOHNING: I've started to put together a structural framework, starting with [Vladimir] Haensel (23), that deals with major innovative concepts in the chemical industry. On this framework I can hang a series of events. That's why I asked you earlier today, when you first discovered zeolites, was it like a light bulb lighting up? In most cases, it seems that scientists do not experience that with their discoveries.

WEISZ: No, exactly. As I mentioned before, it was a number of things coming together, and many steps of added development.

BOHNING: Instead, it seems as though there's a sequence of things that occur. So I am looking at that and trying to see what's going on in the industry at the same time that one process is developing—because no one operates in isolation. Carbide's doing things while you're doing things, and so on.

Incidentally, I talked to Edith [M.] Flanigen (24).

WEISZ: When did you talk to her, by the way?

BOHNING: Last December, I believe.

WEISZ: Well, she was a fine scientist, a fine lady. I did not know her or of her work until much later in my years of work with the shape selectivity. But I learned about her very important contributions to zeolite science. She also was involved in making the structure we at Mobil made and called ZSM-5 and they called it silicalite. I got to know her later personally and it was a pleasure to know her. In fact, we met personally at a scientific meeting in Berlin, and we talked then about a dispute over some license matter between our companies which we both thought to be regrettable, and I believe we stimulated both our managements to settle the issue.

BOHNING: Okay. Well, just for the record, I want to thank you for spending the day with me.

WEISZ: Well, thank you for being a patient listener. It’s in a way fun to get it out of your system. [laughter]

BOHNING: Well, I’ve enjoyed listening very much. Thank you again.

[END OF TAPE, SIDE ELEVEN]

[END OF INTERVIEW]

NOTES

1. Ernst Zimmer, *Umsturz im Weltbild der Physik,* Verlag Knorr & Hirth, Munich 1969.

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