CHEMICAL HERITAGE FOUNDATION

VLADIMIR HAENSEL

Transcript of an Interview
Conducted by

James J. Bohning

at

University of Massachusetts, Amherst

on

2 November 1994

(With Subsequent Corrections and Additions)

ACKNOWLEDGEMENT

This oral history is one in a series initiated by the Chemical Heritage Foundation on behalf of the Society of Chemical Industry (American Section). The series documents the personal perspectives of Perkin and the Chemical Industry Award recipients and records the human dimensions of the growth of the chemical sciences and chemical process industries during the twentieth century.

This project is made possible through the generosity of Society of Chemical Industry member companies.

CHEMICAL HERITAGE FOUNDATION
Oral History Program
RELEASE FORM

This document contains my understanding and agreement with Chemical Heritage Foundation with respect to my participation in a tape-recorded interview conducted by James J. Bohning on 2 November 1994.
I have read the transcript of this interview supplied by Chemical Heritage Foundation.

1. The tapes, corrected transcript, photographs, and memorabilia (collectively called the "Work") will be maintained by Chemical Heritage Foundation and made available in accordance with general policies for research and other scholarly purposes.

2. I hereby grant, assign, and transfer to Chemical Heritage Foundation all right, title, and interest in the Work, including the literary rights and the copyright, except that I shall retain the right to copy, use, and publish the Work in part or in full until my death.

3. The manuscript may be read and the tape(s) heard by scholars approved by Chemical Heritage Foundation subject to the restrictions listed below. The scholar pledges not to quote from, cite, or reproduce by any means this material except with the written permission of Chemical Heritage Foundation.

4. I wish to place the conditions that I have checked below upon the use of this interview. I understand that Chemical Heritage Foundation will enforce my wishes until the time of my death, when any restrictions will be removed.

a. VH — No restrictions for access.
NOTE: Users citing this interview for purposes of publication are obliged under the terms of the Chemical Heritage Foundation Oral History Program to obtain permission from Chemical Heritage Foundation, Philadelphia, PA.

b. _____ — My permission required to quote, cite, or reproduce.

c. _____ — My permission required for access to the entire document and all tapes.

This constitutes our entire and complete understanding.

(Signature) Signed release form is on file at the Science History Institute
Vladimir Haensel

(Date) May 1, 1998

Rev. 4/14/98

Oralhist/standard/rclinfo/finrelfm-nonedited.doc

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

VLADIMIR HAENSEL

1914 Born in Freiburg, Germany on 1 September

Education

1935 B.S., general engineering, Northwestern University
1937 M.S., chemical engineering, MIT
1942 Ph.D., chemistry, Northwestern University

Professional Experience

1937 Chemical Engineer, Universal Oil Products Company (UOP)
1939 Research Assistant, Ipatieff High Pressure Laboratory
1942-1945 Chemical Engineer, UOP
1945 Inspector, German synthetic oil plants, Technical Oil Mission for the Petroleum Administration of War

Universal Oil Products Company
1945 Coordinator, Cracking Research Division
1951 Director of Refining Research
1960 Director of Process Research
1964-1972 Vice President, Director of Research
1972-1979 Vice President, Science and Technology

1980- Professor of Chemical Engineering, University of Massachusetts, Amherst

Honors

1944 Chicago Junior Chamber of Commerce Award
1952 Precision Scientific Company Award in Petroleum Chemistry
1957 Professional Progress Award, American Institute of Chemical Engineers
1965 Modern Pioneers in Creative Industry Award, National Association of Manufacturers
1967 Chemical Pioneer Award, American Institute of Chemists
1967 Perkin Medal
1971 Member, National Academy of Sciences
1973 National Medal of Science

1974	Member, National Academy of Engineering
1977	Eugene J. Houdry Award in Applied Catalysis
1984	Chancellor's Medal, University of Massachusetts
1991	National Academy of Sciences Award for Chemistry in Service to Society
1993	Henry J. Albert Award, International Precious Metal Institute
1994	Chancellor's Outstanding Teacher Award, University of Massachusetts
1997	Charles Stark Draper Prize, National Academy of Engineering

ABSTRACT

Vladimir Haensel begins this interview by discussing his family life. Haensel, though born in Germany, spent parts of his childhood in Russia, Austria, and Germany. He attended a German gymnasium, where he had only a few science courses. However, family friends encouraged his burgeoning interest in chemistry. When his father was offered a faculty position at Northwestern University, Haensel's family moved to the United States. Haensel studied engineering at Northwestern, receiving his B.S. in 1935. He earned a scholarship for graduate school at MIT, where he studied polymerization under Edwin R. Gilliland. With the help of a family friend, Vladimir Ipatieff, Haensel was offered a summer position at Universal Oil Products (UOP). After earning his M.S. in chemical engineering in 1937, Haensel took a permanent position at UOP, and helped Ipatieff to set up a high-pressure laboratory (funded by UOP) at Northwestern. During this time, Haensel also earned his Ph.D. in chemistry from Northwestern, writing a thesis on the decomposition of cyclohexane. In the 1940s and 1950s, Haensel moved into research management. He was also integral in UOP's development of the Platforming process. Haensel concludes this interview with a discussion of the importance of instinct in research, the future of research and development, and his thoughts on winning the Perkin Medal.

INTERVIEWER

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

TABLE OF CONTENTS

1 Childhood and Early Education
Growing up in Russia, Germany, Austria, and the United States. Effect of World War I. Growing interest in chemistry due to family friends.

5 College Education
Engineering at Northwestern University. Graduate school at MIT. Studying polymerization with Edwin R. Gilliland. Summer job at Universal Oil Products.
Influence of Vladimir Ipatieff.

8 Career at Universal Oil Products
Working for Ipatieff. Setting up high-pressure laboratory at Northwestern. Ph.D. thesis on decomposition of cyclohexane. Development of the Platforming process.

35 Research and Development
Importance of instinct. Experiences in research management. Corporate support for R&D. Cooperation between chemists and chemical engineers. Importance of reading. Future of R&D.

46 Conclusion
Thoughts on winning the Perkin Medal.

49 Notes

51 Index

INTERVIEWEE: Vladimir Haensel

INTERVIEWER: James J. Bohning

LOCATION: University of Massachusetts, Amherst

DATE: 2 November 1994

BOHNING: I know you were born on September 1, 1914 in Freiburg, Germany. Could you tell me something about your mother and father and your family background?

HAENSEL: Yes. My father was professor of public finance at the University of Moscow for many years. My mother came from a Baltic country. My father was a Lutheran to begin with, and my mother was Russian Orthodox. When they were married, they both became Russian Orthodox. They lived in Moscow. Before the revolution, my parents had three servants and no children. After the revolution, they had three children and no servants. [laughter]

BOHNING: You were born in Germany, correct?

HAENSEL: Yes, by virtue of the fact that this was 1914. My parents and grandparents would go to a place near Freiburg which was called Baden-Weiler. It was in southwestern Germany, near France, near Switzerland. It was a beautiful countryside. My grandparents used to spend time there. It was a watering place from the Old Roman times and they would spend part of the summer down there.

Therefore, I was born in Freiburg, which is the closest city to Baden-Weiler. I was born just at the outbreak of World War I. Because my father was a Russian citizen, he was taken as a prisoner by the Germans; later on, he was released because the Germans realized the fact that the Russians do not conscript their teaching personnel at the University.

Shortly after I was born, we went back to Russia, which was not yet the USSR. The borders were closed and we had to go by way of Sweden. We stayed in Moscow until the revolution. Our home was a beautiful apartment overlooking the Kremlin on one side and the museum on the other side. It was a beautiful location. You couldn't think of a nicer location; it was on the top floor of a four-story building.

As the revolution got going, they were shooting from the Kremlin toward the communists that were moving in, and shooting back from the communists through our window. [laughter] So after a while, Father and Mother decided they were going to leave; this is how we

went to the Crimea. That must have been 1917. I remember nothing of it, except what I have heard.

At that point, Father became professor at the Tauric University in the Crimea. We were there until almost 1921. As the communists moved down and occupied more and more of the territory during the civil war that followed, they took over.

Father was the dean of the university in Simpeferol. Father made a mistake, as the dean of the university, giving a speech extolling the virtues of the defunct former regime, [laughter] and the next day the communists came. Father was arrested and the whole family was taken back to Moscow.

There was nothing to eat. It was an armored train, and somebody was going around threatening to kill everybody as soon as possible; something like that. It took about a week when they were taken back to Moscow.

BOHNING: What year was that?

HAENSEL: That must have been 1921. It took a long time for the communists to take over. Father was taken prisoner, but he was released by orders of [Vladimir] Lenin, who said he needed all the people who knew economics; and father taught public finance. When Father became associated not just with the university but also with the commissariat of finance, he wrote the first five-year plan for the Soviet Union. That was really incredible, to be able to do that sort of thing. It was one of the many five-year plans.

When we came back to Moscow, we picked up our apartment again, where we were before. It was fine, except there were two more inhabitants by that time. [laughter] Father went back to teach. He taught in two places and he also was associated with the government in this activity, with respect to the prospective five-year plan or financial planning, whatever it is you call it.

We stayed in Moscow until 1929, at which time Father received an honorary degree from the University of Munich, and he was allowed to leave the country. My mother had some problems such that she needed to see her own doctor in Germany, so she got permission to leave. However, they took Father's permission away again, because they always wanted to keep a hostage. [laughter] That was something! It's amazing what we went through, with that regime.

BOHNING: What about the children?

HAENSEL: The children always go with the parents, so there was no problem.

So they took Father's permission away, and then after quite a bit of time, Father's permission was granted, and we left. We took nothing with us, because we were all coming back.

In the meantime, my cousin was living in Moscow at our apartment, and he notified Father surreptitiously that there was a notice about the fact that he was no longer going to be permitted to teach at the university.

You see, Father was really very outspoken, and he said you cannot teach public finance without mentioning capitalism. [laughter] Apparently, it was too advanced for them, and they decided against it. In fact, his successor was sent to Siberia two years later. So Father escaped a very important fate.

When we left, Father went to teach at the London School of Economics. My older brother went with my father, and we stayed in Germany in Düsseldorf. I had to learn German, which I did. You know, after a while, it becomes very easy to learn something. At that point, it was 1929, so I was fifteen years old.

We spent a year or so in Germany, in Düsseldorf, staying with my uncle. Then Father received an appointment to teach at the university in Graz, Austria. So we became Austrian citizens, went there to live, and had a wonderful time. It's a beautiful location. Have you been to Graz, by any chance?

BOHNING: No, I haven't.

HAENSEL: All of Austria is just beautiful. We were there for a year, and then Father was invited to come and give lectures at the University of Chicago, at Northwestern, and the University of Wisconsin. Northwestern offered him a permanent position. So the whole family moved here to this country in 1930. I've lost track of whether it was 1930 or 1931, but I guess it was 1930.

I came along, obviously. My older brother went to Northwestern, my younger brother was in grade school, and I was enrolled in Emerson Township High School. My parents took me there. I knew no English, and they said, "You're now on your own" (there was this Russian conversation going on). [laughter] I joined the freshman English high school class during the summer session. By the end of summer school, I had to read *A Tale of Two Cities* by Charles Dickens. At that point, I went to the Northwestern library to get a German translation of *A Tale of Two Cities*, [laughter] which was thicker compared to the normal version. I passed the test, etc.; I got a grade of 67 on that, as I remember.

In looking back, I'm so delighted that I had parents who were smart enough to say, "None of this bilingual business. You're going to learn the language of this country." Which I did. They also sent me to the School of Speech at Northwestern. I went to see this lady, and she said, "Let me hear you talk." So I talked. She said, "You talk down here. We talk here." She said, "Keep your mouth open." [laughter] I've been keeping my mouth open ever since. [laughter]

This is how this started. Then the following year I went to Northwestern. I was interested in chemistry.

BOHNING: I'd like to back up for a moment. You had your earlier education in Russia, in Germany, and in Austria.

HAENSEL: Yes, that's right. How did I get into chemistry? Would you like to know that?

BOHNING: Yes, but I would like to know something about your educational experiences before you came to this country, and how they played into your developing an interest in chemistry.

HAENSEL: Before I came to this country, my experiences really were very, very good. The school in Russia was very nice. They had a very nice teacher, and the only problem with him was that he said, "You're left-handed; from now on, you're going to be right-handed." [laughter] My mother was completely appalled about that.

What did I have? I had really good training at the high school in Germany, except of course I had to learn German much better than I had already picked up. I think all the way through, I remember the professors and teachers that I had, even in high school, who were extremely good. Also in college. I remember the names of the very few I did not really care for. You probably found the same thing. I remember the teacher in Düsseldorf; I admired him. Those people were really good; they were dedicated teachers.

The experience in Germany was really largely to learn German and to learn some math, etc. It was the regular procedure, as you go through high school. By that time I was turning fifteen. When I came to this country, I only had to learn English plus pick up some math courses.

BOHNING: Did you have any science courses in Europe?

HAENSEL: Very little; some, but not too much. I'll tell you how I got interested in chemistry. My parents had a great many friends. One of them was a physical chemist or an inorganic chemist. One time when he came he said, "I want to show you people something interesting." They brought out two glasses; he took one glass, poured some stuff in it and in the other, mixed the two, and then turned it over; it had gelled. [laughter] I imagine it must have been a salicylic acid precipitation. Everyone agreed that was a good example.

From then on I was really hooked. I felt this is for me, and I like this. [laughter] Did you have a similar experience, Jim, or not?

BOHNING: I had a high school teacher who—

HAENSEL: —influenced you?

BOHNING: Yes. In chemistry. He was outstanding.

HAENSEL: This was really where you learned. The giants came later on. But these earlier people were just as much giants in their own way; they elevated you, they made you curious. I think that's the most important part that happened.

I had a little science in Austria, and some in Germany. But those are just individual, single years, because of our moving. Then I went to Northwestern. It must have been 1931 when I enrolled there, because I graduated in 1935. I went to the School of Engineering; they didn't have any chemical engineering, so I took lots of chemistry. There were outstanding professors there. I mentioned one of them yesterday that you remembered—Charlie [Charles D.] Hurd (1). Ward V. Evans was another one. They were absolutely marvelous teachers!

Now, it is a little more difficult to find people like that. I find this not as a fault, but as a sort of a side impression. Our teachers, nowadays, are much more impersonal, with respect to their students. I think it's wrong. But of course, they have huge classes also. That's one of the big problems today.

BOHNING: What was your brother majoring in? He was a year ahead of you.

HAENSEL: My older brother was two years older, and he went into the financial area.

BOHNING: Like your father.

HAENSEL: Like my father. Except that later on in life, my brother was down in South America working for an oil company. During the war, he was a Captain. He always specialized in technological sales, to a large extent. He was down in South America for a number of years.

My younger got his degree from Northwestern. Everybody got their degree from Northwestern. My younger brother also specialized in sales. My younger brother died about twelve years ago, something like that. My older brother is alive in Dallas. He has a very nice wife. He likes what he's doing; he is now eighty-two, so he's doing very little. [laughter] So that's where we are.

BOHNING: My notes say that your degree at Northwestern was in chemical engineering.

HAENSEL: No, it wasn't chemical engineering. It was general engineering with a major in chemistry. They didn't have any chemical engineering then. This is where I really became more and more interested in chemistry. After that, I had a scholarship offer from Columbia and from MIT; I was delighted that I chose MIT. I went there in the fall of 1935 and spent two years in chemical engineering working for [Edwin R.] Gilliland. He was a very famous name in chemical engineering. That is where I really met the giants, like Harold Weber and Gilliland and Tom [Thomas K.] Sherwood. I remember Sherwood particularly for very spectacular sort of a thing, where he plotted the negative log of the concentration against the price of a product. The points all fall nicely on a line. [laughter] He curved the line a little bit. On one side, you might have radium way up and the price will be in millions of dollars, and the bottom part you'll have copper. This is the negative log of concentration. Do you want me to draw it for you?

BOHNING: That's all right; I think I understand it.

HAENSEL: Okay, because I've copied some of that someplace else.

BOHNING: Well, that would be interesting to see.

HAENSEL: Okay. I'll find it a little later on.

BOHNING: All right.

HAENSEL: At Northwestern I had reasonably good grades, and I thought I was really pretty much at the top of the class by the time I got through. This is how I got the scholarship to MIT and Columbia.

As I said, I selected MIT. It was really fascinating. You said you interviewed [Hoyt] Hottel (2). I never had anything from Hottel, but I certainly had W. K. Lewis and Thomas Sherwood, and Gilliland, who was my thesis advisor.

I wanted a master's degree, because I really wasn't sure if I wanted chemistry or chemical engineering. I vacillated a great deal; I think, for obvious reasons. At that stage, I really did not know what I wanted. I did not know until I started to work. [laughter] I still don't know, so I cover both. To me in retrospect, the thing that makes it important for me here [University of Massachusetts] is the fact that I know a great deal of chemistry that fits into the chemical engineering concept.

Chemical engineering has become much too mathematical in treatment, and it has taken the chemistry out of it. I have a very good friend on the faculty, Phil [Phillip R.] Westmoreland; I will give you a copy of his presentation (3).

So, where were we? We got to MIT.

BOHNING: Yes.

HAENSEL: That was the time, when I went there for graduate school, that although I did very well at Northwestern, when you go to another place for graduate work, you're no longer expected to be at the top of the class. [laughter] Indeed, at Northwestern I was the star pupil in freshman English, as long as we talked about Chaucer, which was very Germanic in character. [laughter]

I did a thesis on polymerization with Gilliland; it was interesting in character, but it was nothing sensational. To me, the other part was the learning from the giants, which really was the most spectacular thing I ever had, education-wise. This was probably the biggest boost that MIT had during that time. After that, I found that they became more conscious of the fact that they could form their own companies and things like this. As a result, the place goes downhill. Right now, it's doing very well. It has plenty of graduate students and a pretty good faculty, but the *esprit* is not there.

The chairman is Bob [Robert A.] Brown. A very good man. You probably know Jimmy [James] Wei. They have very good faculty, so it says. They're good people to get; some of them come in here and give beautiful lectures; but I don't see the same strength that I've seen before. Other schools like Minnesota and Wisconsin have taken over. Also Caltech [California

Institute of Technology], to a smaller extent, because Caltech is a combined chemistry and chemical engineering program, which is a little difficult to put together.

As I said, I remember the names of some of the giants, but I do not remember the names of the mediocre ones. The one person who really gave me quite a boost was Harold Weber. I also had a course in chemistry from James Flack Norris, another very famous name. Again, these were giants. To me, Harold Weber was an extremely ingenious person. He said, "You know, I went to MIT and I flunked thermodynamics six times. Then I had to teach it." [laughter] He was a consultant to UOP. He was extremely good.

BOHNING: Had you thought at this point about what you were going to do beyond MIT? Did you have any career ambitions at this point?

HAENSEL: Career ambitions? Really, I just thought that things would probably come along. I had the chance to work in the laboratories at UOP one summer, because of my parents. My father and [Vladimir] Ipatieff got together, being both Russians and very close friends, and I had a chance to work at UOP in the summer.

BOHNING: While you were at Northwestern?

HAENSEL: While I was at Northwestern. That's how the association with UOP came about.

[END OF TAPE, SIDE 1]

BOHNING: Did your father and Ipatieff know each other in Russia?

HAENSEL: Not much; apparently, they only knew of each other. Ipatieff was a Lieutenant General in the Russian army. You've gone through his background, so I don't need to tell you about him (4). After he came over here, and when I became associated with him, he was like a second father to me. Number one, because he could speak Russian, [laughter] and I could speak Russian with him. Number two, because of the family association. As I said, he was like a second father. I've known him until the day that he died.

BOHNING: So he visited your home?

HAENSEL: He visited our home; he visited really more with my parents. I got married in 1939. I don't know how my wife stood all the Russian talk. My parents and we would drive down to the Pearson Hotel in Chicago, right next to the water tower. We would have something to eat there, and we had lots of good conversation; my wife could not participate. But somehow she tolerated it; she was a very tolerant person. [laughter]

During these visits the conversations with Ipatieff were largely discussions of the past life in Russia and the life here. I think that Ipatieff assimilated himself extremely well, because he had his work.

Every summer Ipatieff would take one or two months off to go fishing up in Wisconsin. He loved it. He found a place full of birch trees that was just like home. Russia is very famous for its birch trees. He would spend his time thinking; he was a great thinker. When he came to work at UOP, he had an office. We had this whole building that we're going to try to dedicate as a shrine [laughter] of catalytic knowledge or something like that.

When I got through MIT, I had an interview with Shell. A very interesting man came in and said, "We're looking for chemical engineers, which you are. You've got a master's degree." I said, "Yes, I will be getting it." He asked me a bunch of questions, which were not very difficult. But there's one question, he said, "How much do you expect to earn in fifteen years?" I said, "Fifteen thousand dollars." He said, "You'll never make it." [laughter] Later I started to pay fifteen thousand dollars in taxes. [laughter]

I got an offer from Shell. I went back to UOP and said, "I would really like to work for you." They said, "We would like you to work for us." They said, "We'll give you one hundred and forty dollars a month." I said, "I got an offer from Shell for one hundred and fifty dollars." It was a lot of money at that time. When I was at MIT, we used to walk across the bridge and go to the Café de Paris on the other side in Boston, and for forty-five cents you could have dinner. [laughter] Those were the days.

But I said, "Fine, I'd love to," because I wanted to work for Ipatieff. That was really the basis for it. That's where the association with Ipatieff came in.

Shortly thereafter, it must have been 1939 or thereabouts, Ipatieff was told by UOP that, as he would like, he was to start the high-pressure laboratory at Northwestern. They said that they would designate me to set up the laboratory, while I could get my Ph.D. at Northwestern, with Ipatieff. Boy, I said, "Yes." [laughter] That's a deal you can't refuse, right? I was getting paid by UOP all the time. That is how I spent the next three years.

BOHNING: Why did UOP set the lab up at Northwestern?

HAENSEL: The connection with Northwestern for Ipatieff came as follows. Ipatieff would not have been able to go to the States without a university association. That's how he came here. Ward V. Evans is famous for his Grignard reactions. He's the one who made all the arrangements. You see, Ipatieff was solicited by UOP to come here, through Gus [Gustave] Egloff. (You may have heard of him too, I suppose, old Gus.) I admired Gus a great deal, because Gus was asked by Hiram Halle (the president of UOP) to go to Europe and find somebody world renowned in catalysis. Gus went over there. This was Hiram Halle, financier, having heard that catalysis is the coming word; he's the one that sent Egloff. Nowadays, we have too much watching of the bottom line. Those people were thinking ahead. This is a financier who really said, "I think I smell something about catalysis [laughter] and I'd get the best material I can get." Get the very best; always shoot for the best.

That is what turned out, that Gus went over to Germany. At that time Ipatieff has been going to the West, based on Lenin's orders to bring back as much technology he possibly could. He went there eighteen times; the nineteenth time he took his wife with him and he never came back. [laughter] Yes, it's an amazing story of these people, isn't it? [laughter]

BOHNING: Yes.

HAENSEL: It's incredible what they went through, all the revolutions and all the other things. Somehow they survived, and they rose to the occasion.

Ipatieff's name was very well known already. If it weren't for the fact that so many of his publications and his work were not published in English or German, but largely in Russian, he would have gotten a Nobel Prize for his work in high pressure and in chemical reactions. As he said, "Nobel gave me praises but no prizes," [laughter] or something like that. He worked for Nobel; he had a consulting arrangement with Nobel in Sweden.

My relationship with Ipatieff was, number one, a common language, so that we could communicate very well. It was difficult for him. He came over here at the age of sixty-five, but he really was instrumental in not only putting UOP on the map as a great catalytic center, but also in developing processes. The catalyst that he developed, for example, which was known as solid phosphoric acid catalyst (SPA), is still being used very extensively for all sorts of purposes, such as the alkylation of aromatics, for example.

So he really was the guiding genius at UOP, and he did extremely well, despite the fact that communications were difficult.

BOHNING: Did UOP fund everything at Northwestern?

HAENSEL: Yes. UOP funded my salary at one hundred and fifty dollars. [laughter] They might have given me a little raise in the meantime. Of course, they supported the laboratory, which was really very, very good. It was a marvelous arrangement, because, as I said, he had to be associated with the university in order to come over here. So that's how Ward V. Evans engineered the whole business to get him over here. It's an amazing feat, isn't it?

BOHNING: Yes, yes. You were talking about foresight on Halle's part, but here you have Evans doing the same thing.

HAENSEL: Yes, Evans had the same idea. Exactly. Evans was really instrumental in bringing him there. There were strange moments. Evans was a great football fan, so he invited Ipatieff to go to the football game, Northwestern against Minnesota. That was probably a good game. [laughter] Evans said, "Would you come?" He said, "No, I've seen a football game once." [laughter] Sometimes, I think he's right. [laughter]

For him to really become so well known in his work in this country, after the age sixty-five, is very unusual. Most of our famous chemists are natives. There are very few who have come over, but not chemists. [Albert] Einstein came over here, if you realize.

Incidentally, have you seen the statue of Einstein in Washington?

BOHNING: Oh, yes.

HAENSEL: Isn't that amazing?

BOHNING: Yes, it is. I saw it in August at the ACS meeting, went I went to a reception over there.

HAENSEL: Oh yes, at the Academy?

BOHNING: Yes.

HAENSEL: You know, it's magnificent!

BOHNING: Yes, it's quite something! I had seen pictures of it, but that was the first time I saw the real thing.

HAENSEL: It's fun to visit it again. When we go there, it is always fun. It's a beautiful spot for it.

BOHNING: One of the other people who I understand that was brought over around the same time was Tropsch of the Fischer-Tropsch reaction.

HAENSEL: Hans Tropsch, that's right. Hans Tropsch came over, but Hans Tropsch did not last too long because he had cancer, and he went back to Germany to die. I also met him at UOP. When you talk to them, you say, "What is the thing that you really need to know in connection with catalysis?" Both Ipatieff and Tropsch said, "You've got to know an awful lot of chemistry; you've got to understand the chemistry. This is they way I feel about chemical engineering. You've got to know an awful lot of chemistry to be able to put the two things together.

They're going to bring to you that little booklet that Phil Westmoreland has on "Putting the chem back into chemical engineering," and we'd like you to have that (3).

BOHNING: That would be fine; thank you.

HAENSEL: Good. So, what else? Where did I leave off?

BOHNING: Well, you've gone to Northwestern, and UOP is paying you to get your Ph.D. and help Ipatieff set up the high pressure lab.

HAENSEL: Exactly, which I did.

BOHNING: What did you work on for your thesis?

HAENSEL: For my thesis, I worked on the decomposition of cyclohexane. It was nothing sensational, but it was interesting. I don't remember whether we had a publication on that or not. We also worked on other things, but that started later on with UOP.

If I may go ahead just a little bit, to both Ipatieff and to me, if you do something unusual, which is completely contrary to the previous concepts, you really get excited about it. It was really very simple. We had a simple compound, like 2,2-dimethylbutane, and when you react it with hydrogen in the presence of nickel catalyst you form exclusively neopentane and methane. Ipatieff just loved the nickel catalyst, because he developed it; nickel and kieselguhr was his favorite catalyst. NiK, he called it. This reaction is completely irreversible. Now, the interesting about it is that it's so neat, and such a clean thing. Ipatieff was never a man for mechanisms. He said, "Mechanisms are cheap. They're a dime a dozen." [laughter] In a way, I think it's true.

Actually Herman Pines, who worked with Ipatieff for many years, was always a mechanism person. Ipatieff had a little disdain for mechanisms, because you could claim that all sorts of things were happening. But to Ipatieff, the fact that it occurs was really the most interesting part.

Then we went one step further. You can make 2,2,3-trimethylpentane by alkylation and polymerization. When you react that with hydrogen, again, nickel on kieselguhr, you form triptane, plus methane, of course. That is <u>the</u> highest octane number paraffin that you can have. There are only two that are really outstanding. One of them, in the aromatics series (I mentioned it yesterday), is mesitylene. This is about 130+, and triptane is also about 130 or thereabouts. But this is a nice paraffin compared to the aromatic, which you might have some problems with.

To Ipatieff, this was just great. One of the papers in this bibliography is about the dimethylation reaction (5). Somehow it hit what he was thinking about. Me, too. Fascinating. Why does it happen? How does it happen? There are all sorts of theories as to why specifically it absorbs here. In a way, it just seems logical, in retrospect, that that's the way it should go because it was the weaker bond right there that you're breaking. But for Ipatieff, he didn't give a damn about the mechanism. [laughter] He said, "I want results!" That's what he wanted.

BOHNING: This may be too simple, but would you classify his techniques as mostly trial-and-error?

HAENSEL: No, not trial-and-error ever! He always had a nose for chemistry. That was really it. Amazing! In all of the things that he did, he always had a feeling that it would work. After this triptane work was originally published (6), it came to be known that two people from Northwestern, or from UOP (for that part) made triptane, which was a super gas that would help with aviation gasoline and was necessary for the war effort. This never got to the war effort. [laughter] It was publicized that it could. It probably needed a little help, with respect to getting going. There was despair, and people said, "We need something that will really fly our airplanes."

It would have. But at the same time, you could take this material (2,2,3-trimethylpentane), which was not far from it anyway, and you don't have to lose one group. [laughter] But it was a

boost to the spirit, that here we have Avgas. They had all sorts of publicity about it; it never got that far. [laughter]

BOHNING: So you're saying Ipatieff had an instinct.

HAENSEL: Exactly. He had a gut feeling, an instinct, and he was usually right. But he did do an awful lot of thinking. I saw in the paper, a picture of Ipatieff during a snowstorm, walking down Lakeshore Drive. Can you imagine this guy [laughter] bundled up and walking in the snow? It said, "The Thinker." That's really what he was. He had an incredible gut feeling with respect to chemical reactions! To me, it was amazing. At the same time, I never had any word from him of discouragement or anything else, despite the fact that he had gone through terrible personal tragedies in his life, with his sons.

His granddaughter visited us here in this country from St. Petersburg. I went back there and visited her when we celebrated the 125th anniversary of Ipatieff's birth, which was a year ago in St. Petersburg. She just wanted to visit the graveside of her grandparents.

His wife felt very badly about the fact they lost one son in Africa. When Ipatieff defected, they took him out of the Russian Academy of Sciences, although they reinstated him later on. When I was there a year-and-a-half ago, they told me there were only three major people in Russian chemistry. One was [Dimitri] Mendeleev, then Ipatieff, and the third one—I can't think of his name right now. Only three people, they said, were big names in Russian chemical history.

BOHNING: To be classified in the same mold as Mendeleev is quite an accomplishment.

HAENSEL: Exactly! Yes. Ipatieff admired Mendeleev, because he created an order. You see, Ipatieff was an extremely orderly person. Remember, we talked about Jerry McAfee. It's the same sort of order in thinking that Jerry has, and that Ipatieff had, which I don't have. [laughter]

BOHNING: Have you ever met anybody else that you would classify as having the same instinct that Ipatieff had? The same nose for chemistry?

HAENSEL: The same nose for chemistry? Not really. I think that Hans Tropsch had some of that. But I saw very little of him, because he went back to Germany to die, and that was the end of Hans Tropsch. I really don't know of anybody else. I do feel, for example, that some of my professors at Northwestern, like Ward V. Evans, had a nose for it. Ward V. Evans was not only a football fan but a wonderful human being and a very good chemist in his area. The others were

also very, very impressive! There was Bob [Robert K.] Summerbell. You probably remember his name.

BOHNING: Yes. Charlie Hurd talked about him (1).

HAENSEL: Fred Basolo is another of the new breeds. Have you ever talked to John Turkevich?

BOHNING: No.

HAENSEL: Do you remember Hugh Taylor?

BOHNING: Oh, yes. I had his brother H. A. Taylor for physical chemistry at NYU.

HAENSEL: I think that Hugh Taylor was really an outstanding person; there was no question about it. But the people that I admire are like [Glenn] Seaborg. These are the people that I really regard as the greats. I think Linus Pauling really was a tremendous person, a tremendous chemist, but I think he went overboard [laughter] in some respects. It was a fact. The guy who I really admired more than anything else was Paul Emmett. Paul Emmett was probably one of the best catalytic chemists. I would rank him close to Ipatieff, in his knowledge and his understanding, and his being such a wonderful person. He was certainly the guy! So those are the biggies; those are the giants.

[END OF TAPE, SIDE 2]

BOHNING: You finished your Ph.D. in 1941?

HAENSEL: Yes. Then I went back to UOP.

BOHNING: I'm a little uncertain; the high-pressure lab continued at Northwestern, but did Ipatieff go to the UOP laboratory?

HAENSEL: Oh, yes. He was both places. He spent three days at UOP and two days at Northwestern. The incredible part about him, to me (it's just an interesting sideline, perhaps) is

how human the guy was. He came in the lab one time and said, "You know, it is time for you to make mountain ash brandy." I said, "Really?" [laughter] He said, "Do you know any place around here where that tree grows?" I talked to my wife Mary and said, "You know, there is a mountain ash tree right here in the neighborhood, a huge tree. All we have to do is to gather the berries and give them to Ipatieff." It's just amazing; here we come in the lab and the professor said we're going to make some brandy. [laughter] It was probably illegal.

We went over to this place which both Mary and I thought had a beautiful tree. We went to the door and told this man that we were making a chemical experiment, which was true, and could we pick some of his berries? He was so pleased about pushing the frontiers of science [laughter] that he even brought us a ladder. So we were out there climbing, and collected all the fruit. Our reward was a number of bottles of beautiful mountain ash brandy that Ipatieff made, on his own. He did it at home. I don't know whether he did it at the lab or at home; I didn't care about that part. [laughter] This is to show you the human side of a person.

There is one thing that bothered me afterwards. I went over to Seagrams and said to them, "You know, one of the great things that a company could do is to put out a new kind of brandy. What about a mountain ash brandy?" The guy said, "It won't ever sell." Just like that. This is one of our problems. We look at the bottom line. If we could only get away from that.

Did you ever interview Lou [Louis H.] Sarett?

BOHNING: We haven't, but he has been interviewed by someone else (7).

HAENSEL: He's one of my idols. His father was a professor of poetry at Northwestern, and I've known him for a number of years through the Academy. He had a very good write-up in the Proceedings of the Academy dealing with the changing times in industrial problems (8), which says that we have gone away from the people who manage research and who had a technical background, to people who watch the bottom line. That started about thirty-five years ago or something like that. That is what really hit us very hard; it hit us as an industrial empire.

When I look at the number of people who are elected to the Academy from various places like IBM and Bell Labs; that number is reducing. Why? Because, for example, Bell Labs has now sent some of their talent to work in their subsidiaries, which are really the producing end. That's the kiss of death. This is something I'm going to write up for *Chemtech* one of these days, because to me it is really a very critical point.

Do you agree with that? That we're going downhill in this sort of thing?

BOHNING: Yes. Let me share a story that somebody at Dow told me, who was a former research director. He said that when Willard Dow ran the company, they had one cash register. But when

business people took over and each unit had a cash register, that's when things started to go downhill. Because with one cash register, one group can carry another group that's still developing.

HAENSEL: Yes!

BOHNING: While one group's producing, they carry the group that's developing something new.

HAENSEL: Sure. That's right.

BOHNING: But when you have multiple cash registers, that doesn't happen.

HAENSEL: You can pinpoint, and you can kill the one that's just developing.

BOHNING: Yes.

HAENSEL: That's right; that's a very good point.

BOHNING: I've always kept that in mind, and it's often true. That's part of what is on this agenda list that I sent to you that I'd like to discuss—your experiences with research management. But before we do that, could we work through the events around the platforming development. I know you've written about this in several places and other people have described it (9).

HAENSEL: Okay, we can go right to it.

BOHNING: I've pulled a number of quotes out of the new UOP history (10).

HAENSEL: I haven't looked at the new one; I've looked at the old one (11). I like the old one better.

BOHNING: You are quoted extensively in the new one.

HAENSEL: That's good.

BOHNING: There are a couple of things that I read in there that I wanted to bounce off of you.

HAENSEL: Okay. Do you want to bounce off first or do you want me to tell you how it came about?

BOHNING: I think we should work that through first.

HAENSEL: How it came about? Okay. It was really very, very simple. It went something like this.

I was assigned to do work in catalytic reforming. The thing that bugged me was the following, and it still bugs me that things like that happened. Catalytic reforming really means taking a fraction boiling at 40 or 50°C up to a 250°C straight from crude oil, with an octane number of about 35, and produce a new gasoline out of it which has at least an 80 percent yield and 80 octane number. In other words, 80/80. That was the aim.

The thing that people went after was modifying the old work horses, like chromium and aluminum catalysts. People had known about them for years, and they were just modifying them. There was no ingenuity associated with it. I thought, "My gosh! What are we really looking for? What do we have in there?" We knew that we had napthenes, which are now called cycloalkanes.

Let's say there is 45 percent napthenes, 35 percent paraffins, and 10 percent aromatics; that is the makeup of a midcontinent gasoline as it comes out of the ground. Okay, so what can you do with this? The combined paraffins, if you're able to separate them, will have a negative octane number, because it is just entirely too low. With the napthenes or cycloalkanes, if you were to separate them, it would be something in the order of maybe 50 or 60 octane number, something like that. Of course, the aromatics are 100+.

So what do you do with the material? I thought, "Why aren't we able to look at these materials and see what we can do, with respect to converting them into something more exciting than just going through the same old catalysts that we've used, and getting 80/80. I don't want an 80 percent yield, 80 octane number; I want 100 octane number. [laughter] Why not? Always go for the best." That was my motto. I think I got it ingrained into me, through all the MITs and Ipatieffs, to do your best. Do much better than anybody else is doing. [laughter] I don't mean competitively; I mean just for your own chemical soul, if you want to call it that. Are you with me?

BOHNING: Yes.

HAENSEL: Okay. What do we have there? For napthenes, we have some cycloparaffins. We have all these rascals:

BOHNING: Yes.

HAENSEL: That goes on—long chains, short chains, boiling all up to 100°C. Okay? Now, what can you do? I did know from the literature, from way back when, work that was done, I think, in Russia—on using a 5 percent platinum on charcoal catalyst as an analytical tool, to be able to discern the differences in the Baku oil field products, between C_6 ring compounds and C_5 ring compounds. I thought, "Now, that is interesting." At that time, I was very much impressed by the fact that aluminum was an extremely interesting material. How I got into that, I really do not recall properly. But I do know that one of the early experiments we did was to take this mixture of materials and dehydrogenate them in the presence of a 5 percent platinum—maybe it was on carbon, I'm not sure—catalyst, and the result was a disaster. The octane number went up from about 35 to about 45. [laughter]

I thought that was very strange. What is it that was doing it? Well, remember that there are these straight-chain paraffins plus a few aromatics. What else do we have in there? We have sulfur compounds in there. So I thought to myself, "My gosh! We'd better set up a little desulfurization unit."

I had one assistant working with me. One side of the lab, we were desulfurizing the material; on the other side, we were processing it over this very sensitive platinum catalyst. We did this largely because we were so impressed with the fact that platinum could be poisoned by sulfur.

You get that ingrained into you. The worst thing that could happen to you. My gosh! It's obvious. Let me erase this from the board. (It's so nice to have a blackboard, I just got this one here. I had another one, a synthetic one, and I got rid of it.) [laughter]

Anyway. We had a good size lab. One side was devoted to desulfurization; the other side was processing, using platinum catalysts that we made up. It was not too difficult to make them. It's fairly easy. Invariably, we had to desulfurize before we processed it, because we knew that platinum was being poisoned very easily. What happens is

$$Pt + H_2S \Leftrightarrow Pt_xS_y + H_2$$

That was the end of this catalyst, obviously. So we'd desulfurize and desulfurize.

One time, in the middle of this thing, we ran out of desulfurized stock. We were still making only about 45, 50, 55 octane number, which was interesting. So, we plugged in the undesulfurized material because we just didn't have that much stock. When we looked at it, sure enough, the catalyst died. Died abruptly! Was killed. Kaputt, as the Germans say. [laughter] What did we do?

I had a gut feeling (this is where you talk about gut feelings) that, after all, if that's what's happening, then maybe the reaction is reversible. So why not increase the hydrogen partial pressure? That was really the beginning of platforming, to recognize the fact that you could overcome some of these problems. It was not that we couldn't desulfurize, it was the fact that, somehow, we got a handle on the ability of the catalyst to perform. Only later on did I really realize the fact that I could apply that same thing to many other parts of what eventually became the platforming process; we'll come to that.

We had to make up these catalysts. How do you start? How do you do it?

We started out with aluminum nitrate as a substrate for making the alumina. You precipitate this with ammonia, and you form this gob of precipitate:

$$Al(NO_3)_3 \bullet H_2O + NH_4OH \rightarrow Al(OH)_3 + xH_2O + NH_4OH$$

Now this is where some serendipity comes into it. As you precipitate this, you're left behind with a whole gob of ammonia nitrate, which stank up the place. [laughter] It was in those days, even then, an environmental hazard. [laughter]

So I set up a little preparation unit, and we started out with aluminum chloride. It was a very nice material. We made exactly the same thing; we made a precipitate, and everything was fine. I looked at it, and I said, "Now this is very strange." Any time that I made a catalyst out of aluminum chloride before, we got a higher octane number. Not much, maybe five points. But it was visible. We were now in the 65 to 70 range. You see, we're coming right along into the higher octane numbers.

Sure enough, this gave us a higher octane number. I thought, "Now, why is that? What is there that is different from the aluminum nitrate outside of the stink we were getting [laughter] from the nitrate?" We actually had a little tiny pilot plant, and we could test the exhaust gas from

the unit. Sure enough, when we looked at the gas coming off the unit, there was acidity in it. In other words, we were getting some HCl coming off.

$$AlCl_3 \bullet xH_2O + NH_4OH \rightarrow Al(OH)_3 + xH_2O + NH_4Cl + HCl$$

That told us that here was something special. We were getting octane numbers this time, about a ten-octane-number-higher product, and still a very good yield. The yield was out of the 80/80 business. That was really the beginning of the understanding, that the element we needed was a bifunctional catalyst. That was really it. Did I make myself clear?

BOHNING: Yes, absolutely.

HAENSEL: To a certain extent, what it really says is, what do I have to go on? I have to go on not just serendipity, but I have to be in the lab, all the time, to smell the products. I remember Ipatieff was going in the lab; he would look at this test tube or something, and he would smell the product. That was the first analysis. [laughter] What the giants really taught me, mostly Ipatieff, in that respect, is how to examine what you're getting. Don't sit in the office and let some flunky (as we called them at that time) bring the stuff to you. No, you go right in there; you work with it. This is what really taught me. In later life, when I became vice president and director of research, I always spent two hours a day going through the laboratories, going through the pilot plants, talking to the people who work on the front. That is what made the difference. This is how I learned my chemistry. [laughter]

I was absolutely delighted with this, because all of a sudden, we not only had higher octane numbers, we must have been doing some different chemistry. What happened was that we were not just dehydrogenating, because there wasn't enough there, but we were also converting all of the five-membered rings. We had this potful of paraffins to work with. What did they do? Somehow, they were dehydrocyclizing to make aromatics.

I showed Ipatieff. I said, "Look at it!" When I started doing this work, he did not in any way interfere. That was his motto. He said, "You've got to get someplace on your own. I'm not going to bother you, but I'd like to know what you're doing." It was fascinating to see his eyes sparkle when one of his students got something.

The most important part that came out of this, was the fact not just that we had come up with this whole business, but the fact that we were using a catalyst that nobody thought of using. Everybody was still working on chromium on alumina, and molybdenum on alumina, modifying them. We gained an awful lot of time. Nobody would use it. In fact, there was one guy within our own company who was most objecting to our using platinum. Who do you think it was? It was not Mr. [David S.] Harris, who was the head of the company at that time; he was all in favor of it. It was the financial person. [laughter] He was a financial officer.

BOHNING: That's just what we were saying earlier. [laughter]

HAENSEL: That's right. I've forgotten his name. But, platinum—my gosh, can we afford it? [laughter] Sounds like "Lucky Alva" all over again (12), something like that. Really, this is where you separate the men from the boys. I realized I was really the man at that point; I've got something. So we went ahead.

The man who I really admired very, very much, was Larry [Clarence G.] Gerhold. Larry was the director of the laboratories, and he would come around. He said, "Val, you know what I think you might to do at this point? As much as I admire your work, which you have done beautifully, we've got to cut down on the platinum concentration." I said, "I'm working on that." He said, "Well, work a little bit harder." [laughter] Which I did.

At that point I did something else, which was an obvious thing, and I talked to a lot of the people around. I said, "Now, after all, we have the whole periodic table to choose from, but as far as the halogens are concerned, there's only fluorine and chlorine, and fluorine might do very well." In fact, I put the fluorine in, and it gave a very, very high octane number. But, the yield was lousy, because the fluorine, as an additive to the catalyst, was much too acidic in character.

Just in class the other day, someone asked, "What about bromine and iodine?" I said, "Look at the periodic table." You can see that these are much weaker halogens. Which they are. The periodic table was the most wonderful thing in the world. [laughter]

At that point, the connection with Gerhold was extremely important, because he not only supported the work, he was very much interested in the work, and did an awful lot for me in that respect. He protected me from all the people who said, "Platinum? Bah!" He also had vision.

He had a master's degree from the University of Illinois in chemical engineering. Actually, a wonderful guy. I called him the other day to come to this celebration (13). He said, "You know, I'm now four score and seven years." [laughter] "I don't know whether I can make it, and I'd like to do it. It would be nice if somebody could come with me." At that point, it would have been very, very difficult. I want to send him all these things, and send him a copy of the article in *Chemtech* (14). Have you seen it?

BOHNING: Yes, I have a copy. That's the one from September.

HAENSEL: Yes, that's right.

Larry was, to me, a great friend, a great advisor, and a great pusher in important places. To me, he was the guy who was close to the top management. He was advisor to Mr. Harris. Mr. Harris was one of the best presidents we ever had. Larry is the one who would go to Mr. Harris—nobody ever called him Dave, I guess. David Harris was a marvelous guy. Larry said, "Mr.

Harris, you know, I think we're getting someplace here. How would you like to put some more manpower in this business?" Before long, when things really got hot, half of the staff at UOP was working in platforming. That is incredible! Try and think of it now, in an industrial environment; you'd never get anyplace! [laughter] "What about my project? That's more important!" This was the ruling from headquarters, who understood the potential for these things.

Larry was an extremely smart guy; he still is a very smart guy, at four score and seven. But he came around and said, "You know Val, I'm worried about one thing."

[END OF TAPE, SIDE 3]

HAENSEL: He said, "You know what I was thinking about? If I considered Mr. Harris," who was an extremely kind person, "and if I analyzed him, what is he worried about? What worries him?" The only thing we could really think of jointly, was what if we lose this marvelous process to somebody else? What if somebody else gets in on the act? I said, "I don't really think so, because nobody wants to use platinum; that's just too expensive." [laughter] He said, "No, you'd be surprised. Let's just worry about it. Has anybody left the company to go someplace else?" I said, "Yes, we had one guy who left the company." He said, "That's what I need." [laughter]

So he went to Mr. Harris. He said, "You know, this is serious business. We don't want to lose it; this is our baby." So we doubled our effort in the whole thing. As I said, half of the staff was really working in this area. First of all, we had to make the catalyst on a larger scale. You can't just go into a filling machine.

Have you seen the catalyst?

BOHNING: No.

HAENSEL: Take a look at this. This came from UOP. Beautiful, isn't it?

BOHNING: It sure is.

HAENSEL: The reason it works up to the top [in the bottle] is because it's very dry in there. This is the platinum alumina catalyst; UOP has been supplying me with this for various purposes. I have a whole bottle of alumina over there, around the corner; it is perfectly white. This is probably about 40.4 percent platinum on the catalyst.

So that was the approach to Mr. Harris—has anybody left the company? [laughter] I said, "Yes, he really did. The guy left and went to some other company." He said, "That is not good. We've got to just get going."

Then the question was, how do you announce this process? One of the vice presidents of the company was a man by the name of Ed [Edwin] Nelson. Ed went down to a meeting of the Western Petroleum Refiners Association (WPRA meeting), something like that. I did not go; I had no reason for it. Ed Nelson gave sort of a general talk on cracking reactions and catalytic cracking, which was just coming in. Then he said, "I just want you to learn something else. We have a new process and we call it platforming."

The name platforming did not come from the public relations people. We had a very nice public relations office that worked on everything, including names. The guy who coined the name was Horner Eby, who worked with me for a number of years. Horner Eby said, "Look, what did we do here? We used platinum, and we elevated the octane number. Don't we really reform the gasoline? So let's combine that to platforming." That's how the name came up. [laughter] The guys in public relations were mad as hell, because they didn't come up with this name. [laughter]

So this is how it started. I think they broke all the records, with respect to trying to make the right amount of catalyst. We used to pill these damn catalysts (pardon the word), and to pelletize it and everything else; it was an absolute pain! Besides, it's such an abrasive material.

This catalyst [referring to sample bottle] came as a result of work by James Hoekstra, a graduate of a parochial school in Michigan. (I can't think of the name of it right now.) Jim Hoekstra was a guy who worked for me. I went to him and said, "Jim, it would be awfully nice to be able to make this catalyst by some method other than pilling. Pilling is such an old thing. That's how you make aspirin! You don't want to make aspirin pills. That's what you want for your headaches. [laughter] How could you make a catalyst which did not require all the pilling?"

Jim Hoekstra, with a degree from a parochial college in Michigan, as I said, was part of the group. A wonderful guy! He said, "You know what I think? We precipitate aluminum hydroxide. It would be nice if we could sort of make droplets of it, wouldn't it?" I said, "Well, you'd better think hard about how you're going to do it, because I certainly don't know." [laughter] He came back within a couple of days and said, "You know what we need?" We already had the idea of something being dropped in spheres. If you drop them through oil, all you get is mush; this dark terrible stuff is just collecting on the bottom. He said, "I have an idea. What if I were able to premix, in the cold, the aluminum sol." (You can make a sol from aluminum metal, which we did later on.) This came about the same time, making the aluminum sol, which is deficient in chlorine. If you were to drop this material and mix it in with something, it would set within this sphere, as it was going through the hot part. It's very clever, isn't it? It's damn ingenious, really.

It wasn't any Ph.D. who was doing this. This is a guy who had the practical experience and somehow, was just trying to think. He said, "We're going to use HMT" (hexamethylene

tetramine). I said, "What are you going to use that for?" He said, "Because HMT decomposes." After all, what is HMT but a product of interaction of NH_3 and HCHO. Then you get this structure of HMT, which I can never write out properly. I'll have to learn how to do this before I give this lecture. [laughter]

HMT decomposes on heating because it is a product of these two materials. But it stinks something awful when it decomposes. (Boy, that was something we had to worry about later on.) So he set up this little tower, with hot oil inside of the tower and droplets. You take a mixture of alumina sol, deficient in chloride, obviously, and HMT; you drop them into this oil, and as it goes through, the HMT decomposes inside of the droplets. Anytime you do it outside, everything goes to mush! So that was really the key. It was just amazing! It does not take a Ph.D. to do this; it just takes damn smart thinking, to see what you can do under duress. So he set this up, and beautiful spheres came out at the bottom. You could take them, even though they're still gelled, and bounce them on the floor.

You've seen the vitamin E spheres that you can buy in the store. That's just what a bottle of it looks like. Beautiful spheres! So he really had a wonderful time with that. This got around all the pilling that we had to do. The first catalyst that went into Old Dutch [Refining Company], as I remember, was not the spherical catalyst. It was a pill catalyst, because we just couldn't set up fast enough for this new thing.

This was really the result of a guy who graduated from a little parochial college in Michigan, who had an idea. The reason he had an idea was because he was given a chance to have ideas. None of this business of big chiefs, sitting in their offices and writing out little orders. "Will you do this next? Let me know what the result is." We had a working organization, where everybody was on the firing line. [laughter] That was really it. That is really what made UOP great.

Of course, the tremendous cooperation, at that point, that we had, particularly through Larry Gerhold. He got all the engineering staff excited about it. There were some who said, "Oh, come on, we're not going to be using platinum as a catalyst." But you've got to fight these battles. Internally. The hardest sell, as somebody said, is within your own company, which is, as you well know, very true. But despite all this, this thing came through later on to make this catalyst, that you see here.

BOHNING: During this development, what role did Ipatieff play?

HAENSEL: Very little. Ipatieff died in 1952. We started the work in platforming in 1949. But he also knew about the work that I was doing before then with the catalyst, and he was thrilled. But he was so careful, really, to be interested, but not directing. He said, "This is your baby." I admire that part very much because he might simply have said, "Look, I want to participate in it." He did not. He was very, very astute. "You're doing it; more power to you."

BOHNING: In this regard, I know of instances where research directors insisted that their name be on every paper and every patent that went out.

HAENSEL: In fact, it would be very bad if their names were on every patent, because that's illegal. Unless you participate actively in the creation of a patent, that's it!

BOHNING: Well, they may have been, as you say, going into the lab.

HAENSEL: The early papers that Ipatieff and I had, look at the list. My name comes first on the demethylation thing, because I was the one who was doing the work in it. He participated very actively in it, but he wanted to push me. As I said, he was like a second father to me. I was one of his prodigies, [laughter] if you want to call it that. You see, when platforming came in 1949, I had been born in 1914.

BOHNING: You were very young then.

HAENSEL: Very young. That's right, yes. The demethylation and all those other things that we talked about before, they came much earlier. That was really because I had the best training, and I was just absolutely enamored of this stuff that I was doing. Sometimes I would get home and Mary would say, "Are you still there, or are you here?" [laughter] My elder daughter Maryanne would say, "You know, Daddy, I'm Maryanne, I'm not Kathy." [laughter] That hurts.

BOHNING: Were there any times during the development of platforming that you were discouraged?

HAENSEL: I'll tell you when I was discouraged—when Old Dutch first started up. You read of some of that.

BOHNING: Yes.

HAENSEL: The feeling was that here we had spent all this time and all this money, and all of the sudden the guy who was in charge of the unit pulled the switch; the reactor was overheating. "Look what I've done here!" [laughter] It was my doing, you see. There are two Nebeck brothers.

Howard Nebeck was one of them, and he was there at the time when this unit, the Old Dutch unit, started up. It's only fifteen hundred barrels. They were luck to find somebody who would buy the process. Actually, we financed a lot of the investment in it, to make it a showplace.

It was desperate. The thing shut down. I went back to have a drink in the restaurant or in the hotel. Howard Nebeck said, "Excuse me, but I'm going to do a little thinking about this." He went back upstairs and redesigned the shell of the unit, because we had hydrogen leaking into the outer shell. It was a carbon-sealed outer thing; therefore you had to protect it, where there was obvious leakage in there. We thought we'd protect it enough by the presence of the catalyst in there, but it was just a matter of time when it would leak out. He redesigned it. Within a week or two weeks at the most, he had it operating again. From then on it ran so successfully that the owner, Elmer Sondregger, started scouring the neighborhood for more fuel to be put into the unit. One thing that he failed to look at was that some people had cleaning fluid, [laughter] even the gasoline that was being used for cleaning. That unit really took off, because you can imagine the excess chloride pushing this reaction to a fare-thee-well. That was one of the things that was bad about it.

Did I tell you about something that happened before the unit started up? I wrote about it somewhere (15). Mr. Harris called me and said, "Val, I hear that you are going up there to the startup." I said, "Yes, Mr. Harris." He said, "I'm looking forward to hearing about it, but I will go out there later on. In the meantime, would you make up a sample of catalyst?"

I said, "Now, that's strange." I didn't want to say, "Mr. Harris, we got a couple of tons [laughter] of this catalyst." I said, "Yes, Mr. Harris, I'll do that." Then I started to think, what does he want it for? He said, "Oh, incidentally, get a sort of half-gallon jar of this catalyst, make it up, don't fill it all the way, and don't close the lid. Have it out in the control room." [laughter] What do you do? So we fixed up a catalyst. We did the right thing. We had sufficient materials on hand to really fix it up. We had everything we could think of. So we had a catalyst that looked just like the normal catalyst, and put it out there in the control room, and didn't seal the cap.

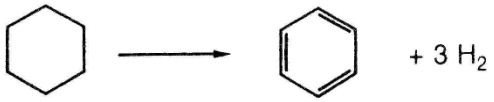
So there it was, the UOP catalyst. He said to put any number on it that we wanted; we used lab number 5. So we did that. Later on, after Old Dutch was running pretty well, Mr. Harris called up and said, "I want to congratulate you on how well the unit is running. Congratulations to you and your coworkers." "Thank you; everybody has been in on it." [laughter] He said, "Incidentally, the catalyst level in that jar is down. Would you fix up another batch?" [laughter]

That was typical of Mr. Harris. That's how Gerhold analyzed him—always very careful. He said, "Val, have you tested this catalyst?" [laughter] I said, "No, Mr. Harris, I have not." He said, "What if it works?" [laughter] I said, "Mr. Harris, if it works, it will set the science of catalysis either forward ten years or backwards ten years, but I don't know which." [laughter] He said, "That's good enough for me. Thanks, again." He was an extremely polite person and just a wonderful guy to work for.

So that's how this thing came about—just a recognition of the problems. Later on, as we realized that sulfur still had an influence in the system, we desulfurized again, in order to reach these high octane numbers. We also recognized, as time went on, that, we couldn't run at very high pressure.

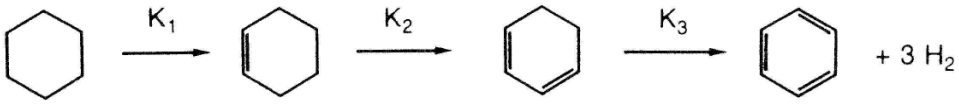
The thing that we talked about (and I talked to the students about this, Jim) is that we started off with a simple reaction, like cyclohexane. What does it go through? How does it get all the way to benzene? Most likely, it does this:

That was one of the things that I asked Ipatieff about. I said, "Do you believe that it goes like this, all the way to the aromatic?" He said, "Do you know who talked about this?" It was a very famous Russian chemist. Ipatieff had people he really didn't trust too much; like the Russians. [laughter] He said, "I really don't think that it does go like this:"



He said, "I really think it must have some intermediate points in it." I said, "I think so, too."

Much later, I would say seven or eight years later when we really worked on these things, this is where we got the idea that we could probably study this sequence, and if we ran it fast enough, in other words, an extremely short contact time, perhaps we could find indications of cyclohexane and relate it to the formation of final product [benzene]. After all, K_1, K_2, and K_3 apply to each step:



That is what made it fun.

We did this. Believe it or not, we made a run at 32,000 liquid outer space velocity—that means 32,000 ccs, going over one cc of catalyst per hour. We diluted the catalyst so that we would not have too much of an endotherm problem, and we diluted the feedstock also. In any event, it was 32,000 space velocity. That was the time where we found, sure enough, that the ratio of the cyclohexene to incremental benzene was increasing all the time, as we increased the space velocity.

This was the subject of a lecture about two or three days ago (16). Here is a run using cyclohexane over a platinum catalyst, 300 pounds pressure. One thing we said from the very beginning, Jim, was that we would simulate conditions as much as we could to commercial conditions. People who worked at extreme vacuums, they don't understand catalysis. [laughter]

Have you ever interviewed Michel Boudart?

BOHNING: No.

HAENSEL: Michel Boudart was one of the few people who had appreciated the fact that there is a tremendous gap between all the high vacuum work and atmospheric pressure work. In our case, we ran it essentially at the temperature and pressure required in a commercial unit. On this diagram (16) this is space velocity, 32,000 space velocity, and then we ran a blank with no reaction. We watched for increasing amounts of cyclohexene, and then we applied the ratio of cyclohexene to incremental benzene. Here is our incremental benzene; this is a conversion of cyclohexane to products, and there is a little MCP being formed.

So you see, this ratio is increasing. This was real fun.

Right here, you see what happens (16); the ratio is increasing. In other words, so this bird here or this bird right here is showing that you're really stopping the reaction. When our friend George Olah talks about stopping reactions, he does not know how to stop the reaction. [laughter] You do it by really understanding the chemical engineering behind it.

[END OF TAPE, SIDE 4]

HAENSEL: Most organic chemists really do not appreciate the importance of chemical engineering; and most chemical engineers don't understand the need for chemistry. That is really the key. My success is precisely because I've been in both areas. [laughter] I try to understand what needs to be done and try to understand the mechanism of how it occurs.

I talked with Ipatieff about this conversion, shortly before his death. I asked him about it, and he said he thought it went through the intermediates. He said, "I just can't visualize how a molecule can sit down on the surface of a catalyst, and all of the sudden lose six atoms of hydrogen." He couldn't! He had a gut feeling. I said, "I'm going to find out," but unfortunately, he died before we did these experiments.

BOHNING: One of the things I was struck by when I saw the statistics for Old Dutch, was that there were relatively large amounts of hydrogen formed.

HAENSEL: Exactly.

BOHNING: Does that present any problem when you're dealing with that?

HAENSEL: Well, on the contrary. One of the greatest things that goes with it, is the use of the hydrogen. The hydrogen is recycled. We run in the presence of hydrogen to minimize polymerization.

But hydrogen is produced, and hydrogen is used very extensively. From all the reforming units, they collect the hydrogen, and they then hydrocrack the oils and they also desulfurize the oils. There is a huge industry in hydrogen denitrogenation and desulfurization. All of the countries require that you have a minimum amount of sulfur and nitrogen in your products. Japan is particularly difficult about that. That's why this hydrogen is tremendously important. We just recycle it back to keep the coke off the catalyst. Okay?

BOHNING: Yes.

HAENSEL: We published some of this at one of the student congresses. There was an outcry, that this could not happen. I said, "Fellows, I do know this happens, and that's what it is." [laughter]

BOHNING: I'm wondering whether at this point I could get your reaction to some things that I pulled out of different publications.

HAENSEL: Shoot.

BOHNING: At one point [1950], there was a World Petroleum Congress at The Hague, and you got into a verbal battle with someone.

HAENSEL: That was [Eger V.] Murphree.

BOHNING: Okay. Since my reading didn't say who it was, I was curious about that.

HAENSEL: As far as I remember, that was Murphree. We announced platforming at that point, more or less, when we were sure what was going on. We were doing 90/90 percent, a 90 octane number or something like that. It was a tremendous improvement. He said, "Well, we have our own workings with our own special catalyst," which was a chromia catalyst or a moly catalyst at that time. "We can do it. We can do these things." They never built the unit. [laughter]

Bob [Robert C.] Gunness, who was my professor at MIT and a Practice School director—later on he became the president and chief executive officer of Amoco—wrote to me and said, "Val, I want to tell you that you spoiled a lot of effort for me. When you came out with platforming, you spoiled all those beautiful plans that we had for catalytic reforming." [laughter] I said, "I was glad to do it."

The thing that I resented, more than anything else, is the adage of those biggies. I'm a little guy, and I worked for a little company. That was extra pleasure to be able to do it. Do you understand what I mean?

BOHNING: Oh yes, absolutely.

HAENSEL: Of course, it has its problems, because the biggies liked to run all over you.

BOHNING: This is a quote, I believe (17). You said, "Any technical development is only as good as the interest you can arouse in people. Once the critical people are sold, the input on their part is tremendously important for the final technological success."

HAENSEL: You're quoting me?

BOHNING: Yes.

HAENSEL: That's good. I think that's correct. [laughter] I think that's basically right.

BOHNING: But how—

HAENSEL: How do you get to them?

BOHNING: Right. What techniques do you use to arouse people to get that interest going? You talked before about the subterfuge you used with Mr. Harris. [laughter] But, in general, how do you arouse people to get that interest going, to sell your idea?

HAENSEL: I think what you really have to be, Jim, is tremendously enthusiastic about a thing. I think it's your own enthusiasm. It's the same thing that the students tell me. They said, "You know, we love your lectures; you're so enthusiastic about these things. We sort of live with you." So I think you have to do the same thing. I don't want to appear to be a sort of a salesman, but I have something to sell.

Right now, I'm on a committee here, which deals with problems that we have with minority education. I have a terrible feeling in that area. What I really want to know is how are we getting the minorities educated? Largely, particularly for Blacks, their parents say, "Look, we don't want you to go into engineering." "Why not?" the kid says. They say, "It's too complicated for you. Why don't you take African studies instead?"

This is really what it is. We get an awful overload of African studies. [laughter] They get jobs where they teach African studies. Before long, you're all over the country. But the whole thing is wrong, because I want to educate. People say, "What about teaching in the other languages that come in?" I say, "I came in and I had to learn English; that's all. [laughter] I'm an American—of Russian origin. I do not want to teach anybody in Vietnamese or any other language. I want to be myself, but I want to get someplace." It's a very competitive spirit in that respect. I think they should have the same thing.

BOHNING: What was Ipatieff's attitude towards the United States, once he was here?

HAENSEL: He was very happy to be here; very happy. When he went to be naturalized, the examiner asked him a number of questions about himself. He came out of a communist country, so he might be subversive or something like that. [laughter] He asked him, "Do you go to church?" Ipatieff said, "Yes." The guy said, "Which church?" Ipatieff said, "Any church; God is everywhere." [laughter]

That is one of the things that I have felt very strongly about. I would say I'm a religious person. But you have to accept certain things. There's no question about that. You have to accept the difficult things, the tragic things, with the wonderful things. It is really that acceptance. I gave a talk at the church, on religion and the life of a scientist. I said, "Being a scientist does not mean that you have to be agnostic, an atheist, or anything else. All it really means is that I have to accept the fact that in my own mind, I have no way of reconciling everything that we have with somebody

pressing some buttons way back here in evolution. Evolution is probably the best example of God's will." You see what I'm driving at?

BOHNING: Yes.

HAENSEL: To me, it has been a tremendous help to feel that way. I never say, "Thank God it's Friday!" [laughter]

BOHNING: I have here a number of quotes.

HAENSEL: You must have done your homework.

BOHNING: Well, I did a little.

Oh, before I go to these quotes, we mentioned this briefly last night that in 1945, you were with the Technical Oil Mission [for the Petroleum Administration of War].

HAENSEL: Exactly.

BOHNING: You were in Germany and you said that you had a chance to interrogate [Karl] Ziegler.

HAENSEL: Yes.

BOHNING: Could you tell me a little more about that experience?

HAENSEL: He was a rather haughty person, and did not take to being interrogated by some little twerp coming into his private domain. He had done all this work. I said I just want to know what he did, *auf Deutsch*. He was very civil and very polite, but I didn't get much out of him. He talked in generalities, about polymerization, etc. But there was nothing that I hadn't learned from other contacts.

BOHNING: Did you speak to him in German?

HAENSEL: Oh yes, yes.

BOHNING: Because he couldn't pull anything by speaking in German. [laughter]

HAENSEL: He didn't pull anything. But, as I told you, we had troubles with our British counterparts. [laughter] It was a very interesting experience. In a way, it was a very sad experience, seeing all the cities that were just devastated. The Major in charge of our group said to me, "Val, I need your help." I said, "What do you want me to do?" He said, "We have a place here, where we keep all the Russians. They were prisoners of war held by the Germans. We can't get rid of them; we can't send them back at this point, because there is no communication here. Would you like to go and talk to them in Russian?" [laughter]

These things were very sad but very interesting. One of the problems we had is that the Russians would break into the plants and would drink wood alcohol. They were looking for anything! But people get pretty desperate in wartime.

BOHNING: Let me go to these quotes, which I pulled out of the UOP history (10).

HAENSEL: My gosh! Are these words going to haunt me?

BOHNING: Well, they come out of this new UOP history.

HAENSEL: Oh yes, okay.

BOHNING: I'd just like to get your reaction to some of these. In talking about your beginning work on platforming, the authors say, and I quote: "Haensel showed little enthusiasm for the project, at first. He tended to work differently from many others on the staff."

HAENSEL: Good.

BOHNING: "He had a restless, impatient mind, and scant tolerance for a slow, meticulous evolutionary approach to science."

HAENSEL: Good. I learned that from Ipatieff. [laughter]

BOHNING: "He preferred to rely heavily on instinct and take quantum leaps from one research avenue to another, until he hit on something that his gut feeling told him would be productive." We already discussed that. "Working on reforming would be shear drudgery."

You're quoted as saying that; meaning you were reluctant to start on this project.

HAENSEL: To start on the project, which means repeating the same old cobalt catalyst or chromia catalyst, or moly catalyst. Rehashing old stuff—forget it! That was the idea for the quantum leap. Wouldn't you do the same thing?

BOHNING: Well there's also a description of an argument you had with Gerhold in which you said you wanted to work on something else, and he said—

HAENSEL: "You will work on this." That's what he's quoted as saying.

BOHNING: Yes.

HAENSEL: That's not quite correct.

BOHNING: That's why I wanted to ask you.

HAENSEL: Yes, that is not quite correct. I wouldn't quote it that way, because Larry never talked to me that way. Never! He was the most polite guy and we had wonderful discussions. He was the first one to champion the whole damn thing. That quote is not really very proper. I think they said [Chester J.] Giuliana was involved there too, but it's hard to say what he said because he's now dead. [laughter] Giuliana was a very nice guy and a very, very good patent lawyer.

I hope they say nice things about Mr. Harris.

BOHNING: Oh yes, yes.

Here's another one with Gerhold. Let me continue. "Gerhold and Haensel clashed on research tactics. Gerhold quarreled with Haensel's tendency to cut broad corners of the scientific method to move the work ahead."

HAENSEL: You mean, I was in favor of it?

BOHNING: The author states that Gerhold quarreled with your tendency to cut broad corners of the scientific method. This is the author's quote.

HAENSEL: No, I never said that, but that's all right. This must be this guy Hal Higdon who wrote that.

BOHNING: But Gerhold conceded that, "Haensel has an uncanny chemical instinct." It's interesting, we're coming back to that instinct again.

HAENSEL: Good.

BOHNING: "His approach saves time, the most expensive thing in research and development."

HAENSEL: I'll buy that. I'll take credit for that.

BOHNING: It's interesting. We're talking about innovation and discovery, but I've never really thought before about instinct in that context. Yet it's come up several times already.

HAENSEL: This is really I think what I learned from Ipatieff. I would say that I saw instinct in W. K. Lewis, who certainly was the greatest chemical engineer that MIT ever had. W. K. Lewis had a certain gut feeling about things. We were talking and he said, "I want to talk about a piece of equipment. This is what it's doing." He described it, without the floor diagram and everything else. Then he said, "You know, I've never seen this, but this is the way it should be." [laughter] That is really where the gut feeling thing came from. It grows in you. I think this is the contribution of the giants, more than anything else, developing this instinct in you. I think you can put that into your thinking, because it is an important part. We don't pay much attention to instincts. Someone's instincts can be bad; they could be all wrong. But in general, it is very, very helpful.

BOHNING: I also understand (and we touched on this a little) that there was considerable opposition to platforming from the sales department.

HAENSEL: Yes. You know why, don't you?

BOHNING: Well, they related it to the established processes; they weren't thinking in terms of something new.

HAENSEL: That's right.

BOHNING: They were thinking in terms of modifying the old process.

HAENSEL: They were sold on selling the same old stuff. This is why one of my pet statements is that, industrially, if you want to talk about something new, you never talk to the marketing people. Never! Because what do they know? They only know the past; they don't look into the future.

You may have heard the name, Don Broughton? He was the man who developed the Molex process. Don Broughton, to me, was one of the most distinguished members of the Academy. He came from MIT; he taught at MIT and then came to UOP. An absolutely wonderful guy! He developed the Molex process. It was great. I think he was another guy who probably had the right instinct. This is why there was so much admiration between Gerhold and Don Broughton.

I was elected to the Academy of Engineering, and the following year I nominated Don Broughton. He won on the first round, which is very unusual. You know, I vote on these things for the Academy of Engineering, and I see people's names come up for fifteen years or so; [laughter] it's terrible. But this guy came in immediately, because they could see what he did. He had the right instinct for it. He had a revolutionary idea. So that is the way instinct really works. You obey those instincts. It doesn't go away, either; you keep on thinking about the darn thing.

There are many instances. There was a guy who was talking about the intricacies of the atom. A Britisher who got the Nobel Prize. I can't think of his name right now. He said the idea about the construction of the atom came to him on a solitary walk in Cambridge Woods, or something like that.

BOHNING: Was this [Sir Ernest] Rutherford?

HAENSEL: It could have been.

BOHNING: Or [Joseph J.] Thompson.

HAENSEL: One of the two. The thing that is really interesting is that I found that myself. We have a greenhouse. Our house has a beautiful entrance. Hertha does a beautiful job on that. We have a bougainvillea that's blossoming right now. Out of this world!

Anyway, working with plants, working with your hands, and thinking nice thoughts about chemistry are very compatible. There are two things that I have told to my employees, people who work for me, and my friends. You never take your troubles from work to home, and you never take your troubles from home to work. One of my associates came in the other day and said, "I listened to what you have to say about this. I think that's very true. You must never do that because you're no good at either place. That's what happens." [laughter]

So those are some of the less famous quotes that I have of my own. [laughter]

BOHNING: Marvelous.

You've already discussed the experiences of yourself and others within the company in selling research management.

HAENSEL: Yes.

BOHNING: Well, at one point, you became the research management.

HAENSEL: Yes.

BOHNING: What was your attitude towards those who were trying to sell you on new processes?

HAENSEL: That's a very good question. I think what I had to do really is to be very, very careful. You have to give a fair shake to any idea that a person has. I think the way it has worked out is that the results were excellent. For about a ten- or twelve-year period, we were churning out one new commercial process every year. People like Herman Bloch were full of ideas. I had wonderful discussions with him. I was in charge, no more than he was in charge with me, because it was such a great friendship.

I could not tolerate people (and I still can't) who are too pompous, who think their ideas are the best in the world. You have to be very careful how you handle them, because you may want to keep them, but not too much. I had one guy, for example, who I had to fire.

The door was always open, except when I really had to have some private discussions with somebody. You gained the respect of the people by doing that. As I said, I spent two hours each day talking to the people who were on the firing line, and appreciating their comments and getting their inputs. Some of these guys in the pilot plants had no college education at all. They came off the street. You and I have a Ph.D., but we must never let it show. This is the thing; you must never do that! You never have to show that you're the boss.

In that respect, my attitude was really to realize that there's a difference between my position as vice president and director of research, and the people who worked for me. But never have I said, "Look, I'm in charge, and this is what you're going to do." With respect to hiring people, we always had a group of people interviewing people. I think the present interviewing system is actually for the birds. I may have written something about it in one of the *Chemtech* articles (18). I'm not sure if I had it in that article or not.

[END OF TAPE, SIDE 5]

HAENSEL: In other words, what you want to know is, how do I behave myself as a boss? You don't have to take yourself seriously. You don't have to be very important. You have to react to management wishes, which sometimes are a little obscure. But, in general, I have had very good experiences with everybody that I worked for, going all the way back to Mr. Halle. As I said, I was amazed how astute some financier like that could be in looking at the future. That's where we have lost out now, in this bottom line profit.

That's my great fear in this country—the tendency to watch the bottom line. You see it all the time. I think somehow we lost, Jim, the frontier spirit in science. People might say there are wonderful things going on. They are wonderful things, but they're results of an awful lot of work by people who still believe in science as a wonderful occupation.

I wouldn't have traded it for anything. You probably feel the same way, don't you?

BOHNING: Oh, yes.

Were you ever in the position, when you were in charge of research, to kill an idea? If so, what criteria did you use to make that judgment?

HAENSEL: To make that decision? That's a very difficult decision. I don't think I've ever really killed anything. I merely indicated that it was just not worthwhile from all standpoints, but normally, from a technological standpoint, you explain that it isn't so hot. Besides, these guys weren't very sure either. [laughter] That was part of it. So you see, research management, if you want to call it that, is a very pleasant occupation, and it's not subject to rules *per se*. You played by the seat of your pants, for each individual person that comes to you with an idea. Also, it comes with ideas as to how people react. Let me give you an example.

We had a symposium, which was by the ACS, which dealt with hiring practices and interviews. In fact, Hertha and I now hold sessions with the students, with respect to interview guidelines, et cetera, which I think are very critical. After that ACS session, this guy came up to me and said, "I'm going to interview at DuPont tomorrow. What do you think my chances are?" I said, "You have no chance at all." He said, "How come?" I said, "Look at yourself in the mirror. A beard, sideburns? [laughter] You look awful." He said, "But that's me." I said, "That's not DuPont." That is the real difference. This is where you really have to pull the plug on these guys.

The same thing applies in research. There are some things that are so far out. The thing that really got my goat, more than anything else, was continuing to work on something which had already been worked on and then merely add a little something else to it. It isn't worth it. But you have to remember, Jim, that UOP has all the way through been extremely set on licensing, which is so different from actual production. This is where we got so far ahead of all the other companies—by virtue of the fact that we have no commercial operation per se, except making catalyst, detergent, etc.

We were selling technology. This is what that book actually is called, isn't it? *Selling Ideas*.

BOHNING: *Ideas for Rent* (10).

HAENSEL: *Ideas for Rent*. I think that's a very good title. We're so far ahead of others because to the others, you had to justify what you've done within your own company. The most difficult sale is within the company. As we found out within UOP, it was difficult to sell inside, despite all the efforts on the part of people like Larry Gerhold and Bob [Robert E.] Sutherland. Bob Sutherland is another very, very good person in connection with pushing platforming. We really had some champions; this is what you need. You've got to find yourself an entrepreneur who's close to the top. Larry was certainly one like that.

BOHNING: There's a story of a group of researchers coming to Willard Dow asking for support for a project and they said to him, "Willard, we know this will work; this is the way DuPont does it." Willard's response was, "If we can't do it better and differently than DuPont, we're not going to do it." [laughter]

HAENSEL: That's very good. Exactly.

You must get an awful lot of sayings from these different interviews, don't you?

BOHNING: It's interesting; I hadn't thought about that until just as you were describing this; it fits in perfectly with what you're saying.

HAENSEL: Yes. Another thing I want to emphasize is that in all cases, in any research management, you never denigrate anybody. For example, I know some professors who would say to some student who has asked a question, "It's a damn foolish question, isn't it, if you think of it?" You never do that. There's another thing that you never do. We have one guy here, right now, who's a very good professor. I'm a member of the personnel committee, and I have to listen to lectures. The question he raised was, "Is there anybody here who doesn't understand this?" [laughter] You never do that; you can't, because you never trespass on the personal feelings of that person. Never do that, because that's denigrating; let your guy stay honorable.

I can't do that to the students. Hertha says I'm just a soft guy, a softie. [laughter] A girl would come around, and she would cry, because she feared an exam. I just let her cry, and then I said, "That's fine. Let's look at what happened, and let's see if we can do better next time, but the grade remains the same." [laughter] But I can see some reasons. For example, a girl came in yesterday. I didn't realize she was in an automobile accident. Her head is still not very good; she still has a nervous reaction. It's tough to take an exam under those conditions, and I can see it then. She said, "Do you believe me?" I said, "You tell me, I trust you. I have no reason not to believe you." But I didn't say, "If you try it again..." [laughter]

BOHNING: What about scientific teamwork, especially in an industrial setting? What are your experiences with that? You started out on platforming with one or two assistants.

HAENSEL: Two assistants. That's right, yes.

BOHNING: Then it built and built and built.

HAENSEL: Yes, it started out with two people, then it got up to about ten, then it got up to thirty, then I got to three-hundred. You talk about teamwork. I think the real answer to it is how you pick the people. If there is a brand new idea, you've got to involve the inventor in it as much as possible. Let him still be the most important part of the whole picture. You must not turn it over to

somebody and say, "Let's see if you can do all of it." Keep him or her in there, whoever it is. That is really the spirit behind it.

For example, this is why UOP let me go on to Old Dutch. They didn't have to. But I would have been very sad if I couldn't have gone. But it was also this spirit of Mr. Harris that was doing that sort of thing; that you allow people to show their very best. As I told you about this man with fifty patents who came from a two-year college, give him a chance!

There is something new going on, and somebody told me about it on the telephone yesterday. Do you know Marcia [Dresner]? She is one of the editors of *Chemtech*.

BOHNING: Yes, I've talked to her; I know her.

HAENSEL: Yes. A very nice lady. I never met her. She said there's a new division in ACS, which deals with technicians. She said you should investigate that. I think that is a wonderful idea.

BOHNING: They've had a preliminary, or temporary status for some time.

HAENSEL: That's right. Now it has been legalized. I think that's a very, very good idea.

There is this other guy who wrote to me about this project of making the Riverside facility as a shrine, [laughter] this historical business. I think it's a good idea.

BOHNING: Absolutely. As I said last night, I encourage you to have him talk to me, and we'll see what we can work out.

HAENSEL: I'll do that, yes. I'll get a hold of him because I have your card here.

BOHNING: While all of this was going on, what kind of contact was there with other companies? You talked about the discussion with Murphree.

HAENSEL: That's right.

BOHNING: Did you work isolated or were you aware of your counterparts?

HAENSEL: For example, I would participate in visits to Shell and to other companies. Whether I was sort of a show thing (you know what I mean?), I wasn't quite sure. But they wanted somebody who knew the business, I guess. They wanted to me to reinforce things. At the same time, the company was very clever. They wanted to keep their own people in place at the same time, but they didn't overdo the sale.

I've gone to many other companies, and it was a very interesting experience. It was really an eye-opener. Some of the companies were really not terribly interesting. But others were very much so; very, very interesting. We got something out of it every time.

I've done some consulting, after having been here, and those contacts have also shown me, right away, how to behave yourself. [laughter]

BOHNING: We've talked a little about this, but let me ask it again. Because you had such a long time period with UOP, what changes did you see happening in support for R&D over your career, and why?

HAENSEL: The changes that I have seen, which I think have not been dramatic, is the tendency to form one group or the other to be dominant. In other words, the whole idea is that there was a beautiful relationship, a hands-off relationship in a way, between chemical engineers and chemists. That's what we had in the past, and I want to push it, because basically it was both. That's what my education was about, and I could see both sides. But I could not see dominance by one or the other because that would be deadly. This is what you got to watch out for. We went through a period where there was too much dominance; too much pressure from the chemical engineers. You do this or else. Whatever it was. It was not good.

Now, what's next on the agenda? You brought up something before.

BOHNING: We were talking about company R&D and support; we've already talked about the changes in company attitude. You mentioned that earlier.

HAENSEL: Yes. That was really the best company. One of these days I might write something about it. It has to be a completely cooperative effort between chemists and chemical engineers. One side or the other must not dominate because they're completely different people. I think the only reason for any success in that respect, as far as research management is concerned, is that I could see beneath both sides. But never one oppressing the other one.

I'll give you an example. One of my employees or coworkers, as I would call them, came to me and talked to me about something. I said, "Are you sure it's going to work?" He said, "Trust

me, I know it will work." [laughter] That puts you in a difficult situation. I said, "Look I can only trust you as far as I can throw a grand piano." [laughter] But this is the question that you have to be very, very careful with. The best relationship that we have had at UOP, as far as people that work for you, was between Herman Bloch and myself. I was in charge, but I never considered myself to be in charge, in that respect. I think any emphasis on being the boss is deadly, because it creates a feeling of insecurity on the part of the employees because you have the right to fire them. You do have the right, but it's an unwritten situation.

The worst examples are these insecure bosses. Frankly, I never felt insecure. I think this is really the way people should feel. I could never denigrate.

BOHNING: What do you think is important for the future vitality of research and development in a company setting?

HAENSEL: In a company setting. You mean in companies dealing with R&D. Vitality? Maybe, to a large extent, Jim, it should be the ability for the company to sponsor independent thought, and support independent thought, more than anything else. But also, there is a very important part, which I failed to mention before, and that is the origin of ideas. If you live in a sort of a vacuum, there will be no way for you to know what is needed in the marketplace. In other words, you simply can't say we need this or that. As I said, the places I stay away from are the marketing people.

Consider the perfect vacuum to begin with, if such a thing exist. Somehow, you must have an input. Where do the ideas originate? How do you sponsor ideas? I think where UOP prospered and made a lot of progress was by supporting the attendance at technical meetings for all employees. There used to be a pecking order. Management thinks it's a picnic. It's not a picnic. Hertha and I don't go to the Petroleum Division meetings, because I think we still know it pretty well. We'll go to some other meetings that are much more interesting and they elevate your viewpoint.

The general attitude on the part of the major oil companies has been that they don't want to send people to the meetings for two reasons. Number one, they might give away things. Number two, it's too expensive; we're losing time from them. A week at an ACS meeting? Forget it! We can only send three people. It's ridiculous; it's absolutely ridiculous.

During my time, I could have sent anybody to Timbuktu, as far as that's concerned. I had a certain budget, obviously; it was not endless. But the idea is that the places where you get ideas is from contacts with other people and <u>not</u> necessarily the marketing department, because the marketing department is doing what it has to, to sell in the market. [laughter] Do you see what I mean?

BOHNING: Yes.

HAENSEL: But you raised a very important point. Where do these ideas come from? It comes from a fair amount of reading of really good journals. I don't mean necessarily *Scientific American*, because that's terribly narrow. I don't know whether you've come across *Discover Magazine*?

BOHNING: Oh, yes.

HAENSEL: Are you a subscriber?

BOHNING: I used to be.

HAENSEL: It's wonderful.

BOHNING: My problem was that I had too many subscriptions and I couldn't read them all. [laughter]

HAENSEL: You couldn't read them all; I had the same trouble. Somebody told me, "Why don't you start *Discover*? I said, "I really don't have time to read it." But *Discover* is very good. *Discover* is good because they speak the language of the layman, and I think that's an important part. I think reading is important. I get an awful lot of ideas just reading. I don't necessarily mean something like *Chemical and Engineering News*, but there are even some very good things in there. The place I really get an awful lot out of, strangely enough, is *Science*. It comes out every week and there are some marvelous things in there. Absolutely fascinating. I think this really is a sort of mind-broadening thing, that you get ideas from. Some of the things that they discuss are so close to what the things that you're looking for.

I wrote a letter to *Science*, Jim (19). I think it may be on the list. It was October 8th of last year. Did you see it by any chance?

BOHNING: No.

HAENSEL: It says, "Transportation costs and the national debt." Believe it or not. What it really dealt with is the fact that we should look at how much we spend per mile every time you drive your

car. Driving up here cost you 43 cents per mile. Believe it or not. That is the cost of transportation. If you realized that it's really costing you that much, you would think twice before going to get the paper around the corner or something like that. [laughter] The concept is that when you measure the cost of owning a car and put all the expenses together, it amounts to approximately 43 cents per mile. Do you believe that?

BOHNING: Yes.

HAENSEL: It's incredible, isn't it? In fact, I'll get you a copy of that. I think we probably have a copy. It was in *Science* of October 8th of last year. I have a number of requests for it, and a number of questions. I gave it to the class without my name on it. I gave it to the class and said, "See what you think of it." Many of the students really enjoyed it. Others said, "Absolute trash! I would never do this!" [laughter] To him or her, it really meant, "My car is my kingdom." But that was part of it. How did I get into that subject? I forgot. [laughter]

BOHNING: We were talking about where the future of R&D is going and what is necessary for it.

HAENSEL: To me, the most incredible thing in the world, Jim, is that the internal combustion engine was developed after the fuel cell was developed. The fuel cell, in effect, is a low-temperature operation. The most difficult part about our current gasoline engine that we have is that it has 16 percent efficiency. You buy three gallons of gas. One gallon goes out of the exhaust gas; one gallon goes to keep the engine cool; and the other gallon goes to the wheels, where unfortunately the efficiency is only 50 percent. So by then, you have 16 percent efficiency. [laughter] This is age-old. In other words, where do these ideas come?

[END OF TAPE, SIDE 6]

HAENSEL: In other words, just think, where do ideas come from? Isn't it ridiculous, that we, in this day and age, can put a man up on the moon, but we can't improve our mode of transportation, which results in all this pollution that we have? Even a diesel engine, instead of being 16 percent efficient, is probably about at the best, 23 or 24 percent.

So where do the ideas come from? Ideas, I think, to a large extent, come from realizing—and I don't mean the problems of the world with respect to relationship with other people or anything like that—what are we doing that it's so very inefficient? We have many inefficiencies in the actual chemical plants that we use. Separations are not terribly efficient.

You've met Mike [Michael F.] Doherty yesterday, didn't you?

BOHNING: Yes.

HAENSEL: He's our chairman. I think one of his forecasts (if you want to call it that) is really being able to create systems that provide for much better separations, where the investment in energy is much reduced. If you look at a commercial plant right now or an oil refinery, probably about six to eight percent of the total heat availability within a barrel of oil is used up within the refinery, which is perhaps not too bad, because you know you got an awful lot of high-temperature processes going on.

In other places where we are very wasteful. We're wasteful in the basic idea of energy in and energy out, and the concepts that go with it. There is too much work being done in areas where there is less energy out than the energy you put in. We have all these things coming around, Jim, such as where people say, "Now look, let us convert manure into energy." The most important point that people do not realize is that the gathering of all of the waste products, such as manure, or gathering of wood for converting into energy is very, very wasteful. The most important concept is, what is the energy balance? We do not recognize that.

I think this is where the ideas come to people. How can you do a better job? In that case, it doesn't mean a better catalyst, it means, really, a recognition of what we do and what we do wrong. Does that make sense?

BOHNING: Yes.

HAENSEL: Does it answer the question to a certain extent? I think that covers that.

As I said before, when we go to meetings, we go to meetings that are sort of alien to us. But that's where we learn. As I said, *Science* is a good source magazine. *Discover* is a very good way of scientific entertainment. *Scientific American*, I'm going to stop the subscription to it, because it's written by professors, and professors are not good.

What other questions do you have?

BOHNING: I have one more question. What effect did winning the Perkin Medal have for you?

HAENSEL: Pleasure. Is that good enough? I mean, it was nice.

BOHNING: Did it have any effect on your career?

HAENSEL: No. Do you know what really had a tremendous effect upon me? I don't mean to mask that point. But very strangely, I had a man who was working with me in automotive exhaust control. We had a substantial project in that. His name is Martin Perga. I had a call from Martin Perga about five or six years ago, and Marty said, "Doc, I have a favor to ask of you." I said "Marty, what is it?" I was thinking to myself that it had been so long since we worked together. He's a very good man. He's now in charge of a refinery out west. He said, "My first son was born yesterday, and I would like to name him after you." I said, "Marty, I've gotten many awards, but none of them has brought tears to my eyes like this one. I'm delighted." When Hertha heard about it, she said, "Does anybody want to be called Vladimir in this country?" [laughter] Vladimir Alexander Perga. When you asked what the Perkin Award meant to me, the call from Marty Perga was much more because it went straight to the heart.

BOHNING: That's marvelous.

HAENSEL: Yes, it was just great.

BOHNING: Well, we've been going for over three hours.

HAENSEL: You said it!

BOHNING: I appreciate the time you spent. Is there anything else you think we should cover at this point?

HAENSEL: No, I'm in your hands, really. I gave you everything that you needed to have. Did I give you all the stuff?

BOHNING: Yes.

HAENSEL: You've got everything.

BOHNING: Let me just finish then by thanking you for spending the time with me this morning; I really enjoyed it.

HAENSEL: Boy, it was a pleasure to me. Yes, yes, come back; we'll talk about something else.

BOHNING: I'm sure we could have quite a lot more to talk about.

HAENSEL: Before you turn the recorder off, Jim, I think it would be very important for us that you come and visit the department and talk to some of the people here. There are some really new wonderful ideas circulating around here. It's part of our chemical heritage, right?

BOHNING: Yes, it is.

HAENSEL: It says right here—Chemical Heritage. [laughter]

[END OF TAPE, SIDE 7]

[END OF INTERVIEW]

NOTES

1. Charles D. Hurd, interview by James J. Bohning at Evanston, Illinois, 28 February 1991 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0090).

2. Hoyt Hottel, interview by James J. Bohning at MIT, 18 November and 2 December 1985 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0025).

3. Phillip R. Westmoreland, "Putting the *Chem* Back into Chemical Engineering," 21 October 1994; typescript in Chemical Heritage Foundation oral history research file #0115.

4. Vladimir N. Ipatieff, *The Life of a Chemist*, ed. by X. J. Eudin, H. D. Fisher, and H. H. Fisher; trans. by V. Haensel and R. H. Lusher (Stanford University, California: Stanford University Press, 1946).

5. V. Haensel and V. Ipatieff, "Selective Demethylation of Paraffin Hydrocarbons," *Journal of the American Chemical Society*, 68 (1946): 345-346.

6. V. Haensel and V. Ipatieff, "Selective Demethylation of Paraffi Hydrocarbons: Preparation of Triptane and Neopentane," *Industrial and Engineering Chemistry*, 39 (1947): 853-857.

7. Lewis H. Sarett, interview by Leon Gortler in Viola, Idaho, 6 September 1990 (Whitehouse Station, New Jersey: Merck & Co., Inc., Public Affairs Information Center).

8. Lewis H. Sarett, "Research and Invention," *Proceedings of the National Academy of Sciences*, 80 (1983): 4572-4574.

9. Vladimir Haensel, "Platforming," *Petroleum Refining*, 29 (April, 1950): 131-135; Haensel, "The Development of the Platforming Process: Some Personal and Catalytic Recollections," in B. H. Davis and W. P. Hettinger, Jr., eds., *Heterogeneous Catalysis: Selected American Histories* (Washington, DC: American Chemical Society, 1983), pp. 141-152.

10. Charles Remsberg and Hal Higden, *Ideas for Rent: The UOP Story* (Des Plaines, Illinois: UOP, 1994).

11. "The corporate history of UOP has been nearly 16 years in the making. It was begun by Charles Remsberg in 1978.... Unfortunately, the manuscript was never published." See Note 10, p. xii.

12. Vladimir Haensel, "Lucky Alva," *Research Management*, 10 (1967): 135-139.

13. Chemical Engineering Department Symposium honoring Professor Vladimir Haensel at the University of Massachusetts, 3 October, 1995; for complete program see Chemical Heritage Foundation oral history research file #0115.

14. Vladimir Haensel, "Creativity: Is Anyone Listening?" *Chemtech* 24 (1994): 10-13.

15. See Note 9b, p. 148ff.

16. For a copy of these lecture notes, see Chemical Heritage Foundation oral history research file #0115.

17. See Note 9, p. 152.

18. See Note 14, p. 13; Vladimir Haensel, "Lucky Proteus Or How Not to Hire a Genius," *Research Management*, 14 (1971): 44-48.

19. Vladimir Haensel, "Transportation Costs and the National Debt," *Science*, 262 (1993): 163.

INDEX

A

Academy of Engineering, 11, 14, 16, 37
Alkylation, 10, 13
Alumina, 20-21, 23, 25
Aluminum, 18-19, 24
Aluminum chloride, 20
Aluminum hydroxide, 24
Aluminum nitrate, 20
American Chemical Society (ACS), 11, 40, 42, 44
Ammonia, 20
Amoco Corporation, 31
Aromatic, 10, 13, 18-19, 21, 28
Avgas, 14

B

Baden-Weiler, Germany, 1
Baku oil field, 19
Basolo, Fred, 15
Bell Labs, 16
Benzene, 28-29
Bloch, Herman, 38, 44
Boston, Massachusetts, 9
Boudart, Michel, 29
Bromine, 22
Broughton, Don, 37
Brown, Robert A., 7

C

California Institute of Technology (Caltech), 8
Carbon, 19, 27
Catalysis, 10, 12, 27, 29
Catalyst, 10, 13, 18-25, 27-31, 35, 40, 47
Catalytic cracking, 24
Catalytic reforming, 18, 31
Chaucer, Geoffrey, 7
Chemical and Engineering News, 45
Chemtech, 16, 22, 39, 42
Chicago, Illinois, 9
Chicago, University of, 3
Chlorine, 22, 24
Chromium, 18, 21
Columbia University, 6-7Copper, 6

Cycloalkane, 18
Cyclohexane, 12, 28-29
Cycloparaffin, 19

D
Demethylation, 26
Denitrogenation, 30
Desulfurization, 19-20, 28, 30
Dickens, Charles, 3
Dimethylation reaction, 13
Discover Magazine, 45, 47
Doherty, Michael F., 46
Dow Chemical Company, 16
Dow, Willard, 16, 40
Dresner, Marcia, 42
du Pont de Nemours & Co., E.I., Inc., 40
Düsseldorf, Germany, 3-4

E
Eby, Horner, 24
Egloff, Gustave, 10
Einstein, Albert, 11
Emerson Township High School, 3
Emmett, Paul, 15
Evans, Ward V., 5, 10-11, 14

F
Fischer-Tropsch reaction, 12
Fluorine, 22
Freiburg, Germany, 1

G
Gerhold, Clarence G., 22-23, 25, 27, 35-37, 40
Gilliland, Edwin R., 6-7
Giuliana, Chester J., 35
Graz, Austria, 3
Grignard reactions, 10
Gunness, Robert C., 31

H
Haensel, Vladimir
 cousin, 3
 daughter, 26
 doctoral thesis, 12
 father, 1-3, 5-6, 8, 16
 Hertha (wife), 38, 40-41, 44, 48
 Mary (wife), 9, 16, 26
 mother, 1, 4
 older brother, 3, 5-6
 uncle, 3
 younger brother, 3, 6
Hague, The, 30
Halle, Hiram, 10-11, 39
Harris, David S., 21-24, 27, 32, 35, 42
Hexamethylene tetramine, 24-25
Higdon, Hal, 36
Hoekstra, James, 24
Hottel, Hoyt, 7
Hurd, Charles D., 5, 15
Hydrogen, 13, 20, 27, 29-30

I
IBM, 16
Illinois, University of, 22
Iodine, 22
Ipatieff, Vladimir, 8-16, 18, 21, 25-26, 28-29, 32, 35-36

K
Kieselguhr, 13
Kremlin, 1

L
Lenin, Vladimir, 2, 10
Lewis, Warren K., 7, 36
London School of Economics, 3

M
Massachusetts Institute of Technology, 6-9, 18, 31, 36-37
 Practice School, 31
Massachusetts, University of, 1, 7
McAfee, Jerry, 14
Mendeleev, Dimitri, 14
Mesitylene, 13

Methane, 13
Minnesota, University of, 7, 11
Molex process, 37
Molybdenum, 21
Moscow, Russia, 1-3
Moscow, University of, 1
Munich, University of, 2
Murphree, Eger V., 30-31, 42

N
Napthene, 18-19
Nebeck, Howard, 27
Nelson. Edwin, 24
Neopentane, 13
New York University, 15
Nickel, 13
Nobel Foundation, 10
Nobel Prize, 10, 37
Norris, James Flack, 8
Northwestern University, 3-16
 School of Engineering, 5
 School of Speech, 4

O
Olah, George, 29

P
Paraffin, 13, 18-19, 21
Pauling, Linus, 15
Pearson Hotel, 9
Perga, Martin, 48
Perga, Vladimir Alexander, 48
Perkin Medal, 47-48
Petroleum Administration of War, 33
 Technical Oil Mission, 33
Pines, Herman, 13
Platforming process, 17, 20, 23-26, 31, 34, 37, 40-41
Platinum, 19-25, 29
Polymerization, 7, 13, 30, 33
Proceedings of the Academy, 16

R
Radium, 6
Russian Academy of Sciences, 14
Rutherford, Sir Ernest, 37

S
Salicylic acid, 5
Sarett, Louis H., 16
Science, 45-47
Scientific American, 45, 47
Seaborg, Glenn, 15
Seagrams, 16
Shell Oil Company, 9, 43
Sherwood, Thomas K., 6-7
Simpeferol, Crimea, 2
Solid phosphoric acid catalyst (SPA), 10
Sondregger, Elmer, 27
St. Petersburg, Russia, 14
Sulfur, 19, 28, 30
Summerbell, Robert K., 15
Sutherland, Robert E., 40

T
Tale of Two Cities, A, 3
Tauric University, 2
Taylor, Hugh, 15
Thompson, Joseph J., 38
Triptane, 13
Tropsch, Hans, 12, 14
Turkevich, John, 15

U
Universal Oil Products (UOP), 8-13, 15, 17, 23, 25, 27, 34, 37, 40, 42-44
Old Dutch Refinery, 25-27, 30, 42
Petroleum Division, 44

W
Washington, D.C., 11
Weber, Harold, 6, 8
Wei, James (Jimmy), 7
Western Petroleum Refiners Association, 24
Westmoreland, Phillip R., 7, 12
Wisconsin, University of, 3, 7

World Petroleum Congress, 30
World War I, 1

Z

Ziegler, Karl, 33