CHEMICAL HERITAGE FOUNDATION

RUDOLPH PARISER

Transcript of an Interview
Conducted by

Arthur Daemmrich and George G. Cremer

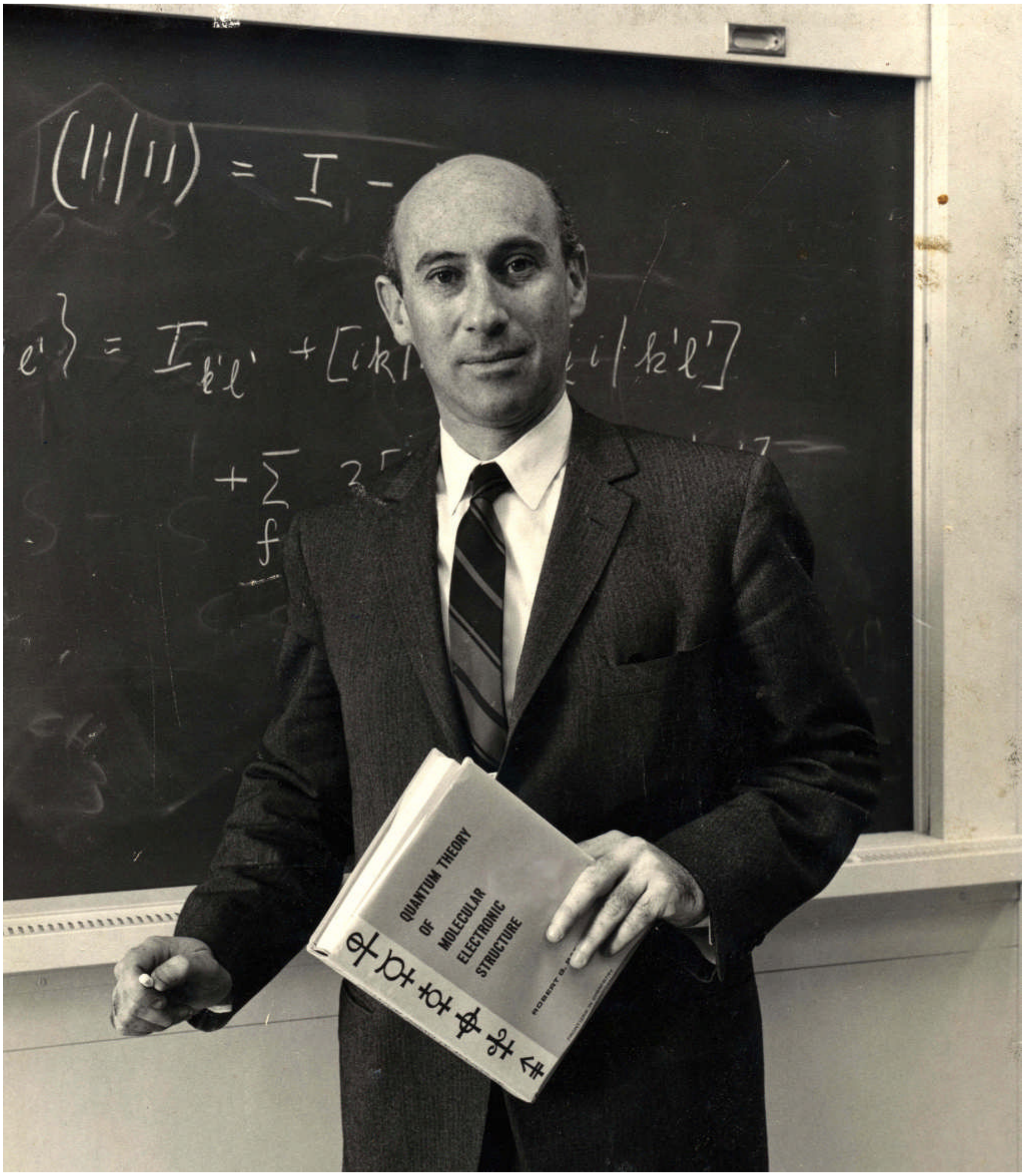
at

Chemical Heritage Foundation
Philadelphia, Pennsylvania

on

28 October 2005

(With Subsequent Corrections and Additions)



Rudolph Pariser

ACKNOWLEDGMENT

This oral history is one in a series initiated by the Chemical Heritage Foundation in honor of E. N. Brandt, an outstanding practitioner of oral history and a leader both in its methods and broader historical context. The series documents the personal perspectives of entrepreneurs, scientists, and managers who have made outstanding contributions to the growth of the American chemical industries during the twentieth century.

This series is made possible through the generosity of The Rollin M. Gerstacker Foundation.

CHEMICAL HERITAGE FOUNDATION
Oral History Program
FINAL RELEASE FORM

This document contains my understanding and agreement with Chemical Heritage Foundation with respect to my participation in an audio recorded interview conducted by Arthur Daemmrich and George Cremer on 28 July 2005. I have read the transcript supplied by Chemical Heritage Foundation.

1. The audio recording, 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 audio recording(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.

Please check one:

a. ✓ **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, Pennsylvania.

b. ______ **Semi-restricted access.** (May view the Work. My permission required to quote, cite, or reproduce.)

c. ______ **Restricted access.** (My permission required to view the Work, quote, cite, or reproduce.)

This constitutes my entire and complete understanding.

(Signature) Signed release form is on file at the Science History Institute
Rudolph Pariser

(Date) 04/22/06

Revised 10/11/05

This interview has been designated as **Free Access**.

One may view, quote from, cite, or reproduce the oral history with the permission of CHF.

***Please note*:** Users citing this interview for purposes of publication are obliged under the terms of the Chemical Heritage Foundation Oral History Program to credit CHF using the format below:

> Rudolph Pariser, interview by Arthur Daemmrich and George G. Cremer at Chemical Heritage Foundation, Philadelphia, Pennsylvania, 28 October 2005 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0320).

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

RUDOLPH PARISER

1923	Born in Harbin, China on 8 December

<u>Education</u>

1937-1941	American School in Japan [ASIJ]
1941-1944	B.Sc., chemistry, University of California at Berkeley
1946-1950	Ph.D., physical chemistry, University of Minnesota

<u>Professional Experience</u>

1944-1946	Infantry and Signal Corps, United States Army
	E. I. du Pont de Nemours & Co., Wilmington, Delaware
1950-1954	Research Chemist, Organic Chemicals Department
1954-1959	Research Supervisor, Organic Chemicals Department
1959-1963	Division Head, Elastomer Chemicals Department
1967-1970	Assistant Laboratory Director, Elastomer Chemicals Department
1967-1970	Laboratory Director, Elastomer Chemicals Department
1970-1972	Director, Exploratory Research
1970-1972	Manager, Research and Development, Elastomer Chemicals Department
1972-1974	Manager, Market Research & Market Development, Elastomer Chemicals Department
1974-1979	Director, Pioneering Research, Elastomer Chemicals Department
1980-1981	Research Director, Polymer Products Department
1981-1986	Director, Polymer Science, Central Research & Development Department
1986-1988	Director, Advanced Materials Science, Central Research & Development Department
	R. Pariser & Co., Inc.
1989-present	President

<u>Awards</u>

1957	Delaware Section Award, American Chemical Society
1976	Outstanding Achievement Award, University of Minnesota
1990	International Journal of Quantum Chemistry, April Issue, in honor of Rudolph Pariser, Robert G. Parr, and John A. Pople

2001	Fellow, American Association for the Advancement of Science
2001	Fellow, World Innovation Foundation
2002	Honorary Fellow, World Innovation Foundation
2003	The Lavoisier Medal for Technical Achievement
2004	Emeritus Certificate, Rubber Division, American Chemical Society
2004	Fellow, International Union of Pure and Applied Chemistry

Advisory Boards and Steering Committees

National Research Council, NAS/NAE

1979-1981	Co-chairman, Panel on Polymer Science and Engineering
1979-1982	Committee on Chemical Sciences
1984	Co-chairman, Panel on High Performance Composites
1986-1989	Committee on Materials Science and Engineering
1996-1998	Committee on Fire Suppression Substitutes and Alternatives to Halon

National Science Foundation

1986-1989	Materials Research Advisory Committee
1994	VPI evaluation and site visit
1999	Small Business Innovation Research Program [SBIR]

Chemical Heritage Foundation

2002-present	Executive, Program and Membership Committees, Joseph Priestley Society
2003-present	Executive Committee, Robert Boyle Society
2005-present	Board of Overseers

University Advisory Boards

1980-1997	University of Delaware Research Foundation, Research Committee
1986-1991	Massachusetts Institute of Technology, Department of Mechanical Engineering, DARPA/ONR Program on Structural Polymers
1987-1997	University of Wisconsin, Department of Chemical Engineering
1987-2003	University of Florida, Gainesville, Department of Materials Science
1988-1995	Lehigh University, Department of Materials Science and Engineering
1989-1991	University of Delaware, Chairman, Advisory Committee, Materials Research Symposia
1990	University of Wisconsin, Chairman, Advisory Committee, Department of Chemical Engineering
1990-1996	University of Pennsylvania, School of Dental Medicine
1996-2004	University of Delaware, College of Engineering
2000-2003	North Carolina State University, Raleigh, Department of Chemistry

2000-present	University of Delaware, Department of Materials Science
2001-2003	University of North Carolina, Chapel Hill, Department of Chemistry
2005-present	University of North Carolina, Institute for Advanced Materials
2005-present	University of North Carolina, Chapel Hill, Department of Chemistry

ABSTRACT

Rudolph (Rudy) Pariser was born in Harbin, China on 8 December 1923, to Ludwig Jacob Pariser and Lia Rubinstein. His father was a German POW during World War I, who escaped from his Russian captives near Manchuria while being transported on the Trans-Siberian Railway. He made his way to Harbin, where he became a live-in tutor for the Shapiro family, and ended up eventually taking over their import/export business. Rudy's mother was a refugee of the Russian Revolution and a relative of the Shapiro family. She made her way to Harbin from Estonia after her family's leather factory was destroyed in the Revolution.

Rudy describes his childhood in Harbin as being quite pleasant. His family lived in a very nice apartment, with servants and a boat on the river. He attended a German school in Harbin, the Von Hindenburg Schule. His friends included children from the foreign business community in Harbin, which was reasonably large, as well as Russian refugees who had fled to Harbin from the Revolution. In 1936, Rudy was sent to an American Missionary School in Beijing, where he lived in the dormitories. The school had a rather strict, quite religious atmosphere. In the summer of 1937, the school was destroyed by the invading Japanese.

Rudy's parents next sent him to the American School in Japan, in Tokyo. It was a school of nearly two hundred students, and many of his teachers were recent American college graduates with advanced degrees who had relished the opportunity to come to Japan and teach. While there, Rudy reinforced his interest in chemistry, thanks in part to the influence of his chemistry teacher, David Misner, whose love for the subject was apparent in the way he taught. Rudy was also strongly encouraged by his father, whom he remembers saying, "If I were you, I wouldn't be a businessman. I'd be a chemist."

By the summer of 1941, tension between Japan and the United States was high and Rudy's parents knew that Rudy had to leave for the U.S. to continue his education. They decided to send Rudy to California with his mother, while his father stayed behind with the business in Harbin. The attack on Pearl Harbor, on 7 December 1941, prevented Rudy's mother from returning to China. Rudy attended the University of California at Berkeley and worked as a Russian translator, and later at the Richmond shipyards, to earn money for himself and his mother. He earned a degree in chemical technology from Berkeley in less than three years, where he was taught by renowned professors like Joel Hildebrand, William F. Giauque, Melvin Calvin, and Frank Oppenheimer.

Upon graduation, he began working as a chemist for Kaiser Permanente Mills. He held that job only briefly before his strong feelings about the War got the best of him and he enlisted in the Army. Although he had a college degree in chemistry and could speak multiple languages, Rudy was also very physically fit, and since fitness was of the highest priority in the Army, he was placed in the infantry. There he was trained for the invasion and occupation of Germany, but through a series of errors he was sent to the Signal Corps in Missouri instead of the front lines, and never actually made it to Germany.

After Rudy had completed his military service, he pursued his Ph.D. in chemistry at the University of Minnesota. He trained under Dr. Robert L. Livingston and did his thesis on chlorophyll photosensitized reactions in solutions. Rudy also met Robert G. Parr for the first time at Minnesota, where Parr was finishing up his Ph.D. in 1947, about when Rudy arrived.

Rudy Pariser received his Ph.D. in physical chemistry in 1950, whereupon he began looking for employment. He interviewed at DuPont's Jackson Laboratory, in the Organic Chemicals Department. Though he had been concerned that it would have a "plant" atmosphere, he found the lab to be of a very high scientific caliber. He was offered a Research Chemist position and accepted with pleasure.

One of his first assignments was to examine the stability of whitening agents, and after that work was completed he proposed to work on the relationship between a dye's structure and its color. He remembered that Robert Parr had done his thesis work on calculating the energy levels in ethylene and benzene, and he gained approval to engage Parr as a consultant to help with that project.

In his thesis, Parr pioneered a rigorous quantum mechanical procedure, which was extremely demanding especially in regard to computing inter-electronic repulsion integrals. However, the calculated energy levels were not in satisfactory agreement with experimental values. Soon after commencing their collaboration, Parr discovered the "zero differential overlap approximation" which dramatically simplified the calculation of the repulsion integrals; the same results for benzene as in Parr's thesis could now be calculated very easily. However, in order to achieve agreement with Parr's experiment, Rudy discovered a method for adjusting the values of these integrals, which was based on the use of atomic ionization potentials and electron affinities. Then, taking advantage of IBM's new computer technology, Rudy developed a program to perform such calculations for large molecules very efficiently. He produced results that were in very close agreement with experimental values, and that also predicted as yet undiscovered excited states, such as triplet states. In recognition of subsequent contributions by John Pople, the theory became known as the Pariser-Parr-Pople theory, or PPP theory. The theory continues to receive worldwide recognition.

With the development of PPP theory, Rudy had become well known in the scientific community. At DuPont, he began his rise through the ranks of research management. In 1970, Rudy was promoted to Director of Exploratory Research, Elastomers Department. Under his leadership, his group developed many important products for DuPont, including certain Viton products, and new products like Vamac, Hytrel, and Kalrez. At that time, also reporting to Rudy was Charles J. Pedersen, who was honored with the chemistry Nobel Prize in 1987.

Subsequently, as Science Director of Advanced Materials in Central Research, Rudy's organization discovered and helped to commercialize group transfer polymerization, novel high temperature superconductors, and other electronic and ceramic products.

Rudy's success with products was matched by his skills as a manager. During his long tenure in DuPont management, Rudy mentored many people who became future leaders and

Vice Presidents at DuPont and elsewhere, including Thomas M. Connelly, Uma Chowdhry, and James M. Meyer.

In 2003, Rudy was awarded the Lavoisier Medal, DuPont's highest award for technical achievement.

Rudy retired from DuPont, and in 1989 he formed R. Pariser & Co., Inc., consulting primarily for R&D management. He also remained active on various university advisory boards, such as at the American Chemical Society, the International Union of Pure and Applied Chemistry, and the Chemical Heritage Foundation.

Rudy was married to Louise (formerly Margaret Louise Marsh) in Bermuda in 1972. Louise hails from an old, established family in North Carolina. Although Louise and Rudy were brought up "on opposite sides of the world," their values are extremely compatible. Rudy and his wife reside currently in Hockessin, Delaware.

INTERVIEWERS

Arthur Daemmrich is the director of the Center for Contemporary History and Policy at the Chemical Heritage Foundation. He holds a Ph.D. in science and technology studies from Cornell University and has published on biotechnology policy and politics, the sociology of medicine, and pharmaceutical drug regulation. The projects he supervises at CHF bring long-range perspectives to bear on key issues in innovation, globalization, risk, health, and environmental policy. Daemmrich has held fellowships from the Social Science Research Council/Berlin Program for Advanced German and European Studies, the Kennedy School of Government at Harvard University, and the Chemical Heritage Foundation.

George G. Cremer is a program assistant in the biotechnology program of the Center for Contemporary History and Policy at Chemical Heritage Foundation. He holds a B.A. degree in history from the Richard Stockton College of New Jersey, and is working toward a B.S. in mechanical engineering at Drexel University. Cremer was a member of the oral history program for two years prior to joining the biotechnology program, where he is currently aiding in the research of commercial biotech in the United States from the mid-1970s to the early 1990s.

TABLE OF CONTENTS

1 Family Background and Youth

Growing up in Harbin, China. How Pariser's father and mother ended up in Harbin. Attending the Von Hindenburg Schule. A brief history of Manchuria. Experiencing World War II from the Far East. Attending American School in Japan [ASIJ]. Mother's work with World War II refugees. Jim Rasbury.

9 Moving to the United States

Enrolling at the University of California at Berkeley. Father's escape from Harbin. Working at Kaiser Permanente Mills. The Chemistry Department at UC Berkeley. Enlisting in the United States Army. Experiences in the Army. Ph.D. work on chlorophyll photosensitized reactions at the University of Minnesota.

17 Joining DuPont

Jackson Laboratory. Research on stilbene derivatives. Mentors. Research on the relationship between structure and color of dyes. Working with Robert Parr. Zero differential overlap. Using IBM computers. The Spectroscopy and Molecular Structure Symposium of 1952. Calculations with PPP Theory.

25 Managing at DuPont

Considering an academic career. Impressions of the IBM 701. Doing polymer-related work. Joining the Elastomer Chemicals Department. Neoprene research. Developing Viton. The Pariser Prize. Becoming the Director of Exploratory Research. Developing Vamac. Developing Hytrel. Developing Kalrez. Rewarding performance.

35 Later Management Positions

Working in the Sales Department. Departmentalization at DuPont. The affect of the 1970s oil crisis on DuPont. Becoming the Research Director in charge of Pioneering Research. Working on group-transfer polymerization. Charles Pederson. Crown ethers. Pederson's research. Central Research as "Purity Hall." Mentoring at DuPont.

42 Conclusions

Charles Overberger. Experiences at the NRC. On the ad hoc panel of Department of Energy chemistry research. Edel Wasserman. Halon fire suppression. CHEMRAWN VII and XIV. Meeting Margaret Louise Marsh. Thoughts on his legacy at DuPont. Winning the Lavoisier Medal.

47 Notes

50 Appendix - Illustrations

67 Index

LIST OF ILLUSTRATIONS

51 Lia Pariser, circa 1942.

52 Ludwig Pariser, circa 1940.

53 Pariser with friend Olga Guterres in Kamakura, Japan, 1939; and Jim Rasbury, as pictured in the 1939 American School in Japan [ASIJ] yearbook.

54 Pariser and his friends at the ASIJ, 1940; and Pariser's junior class at ASIJ, 1940.

55 Pariser's graduation photo, 1944.

56 Pariser at Jackson Laboratory, circa 1957; and Newspaper clipping from 1957.

57 The first instructional class for the IBM 701 Data Processing Machine; and Layout of the IBM 701.

58 Pariser as the director of Pioneering Research, 1957.

59 Pariser with Herman Schroeder and Charlie Pederson in 1987; and John Pople and Robert Parr in 1999.

60 Weitao Yang, Pariser, Robert Parr, and Paul Ayres, 2006

61 Thomas Connelly, Jr., Pariser, and Charles Holliday, Jr., 2003.

62 Group photo of "the Interlocutors."

63 Margaret Louise Marsh Pariser, circa 1970.

64 The Parisers in 1972 and 2003.

65 The Parisers skiing photos and holiday cards.

66 The Parisers for Louise's birthday, 2005.

INTERVIEWEE: Rudolph Pariser

INTERVIEWERS: Arthur Daemmrich and George G. Cremer

LOCATION: Chemical Heritage Foundation
Philadelphia, Pennsylvania

DATE: 28 October 2005

DAEMMRICH: This is an oral history interview with Dr. Rudy Pariser, on 28 October 2005, at the Chemical Heritage Foundation [CHF]. Rudy, please begin by telling us about your family and early childhood.

PARISER: I was born in Harbin, China, which today is a major industrial center. It was a very interesting city. It was not really 100 percent Chinese, since it had a large Russian population—maybe around fifty to one-hundred thousand Russians. My nanny was Russian, so Russian was one of my early languages. I also learned German early on because my father [Ludwig Jacob Pariser] and mother [Lia Rubinstein] primarily spoke German at home. Today, I don't remember too many words in either language, but when I visit Russia and Germany many people think I am a native because I don't have an accent! [laughter]

My father was a Lieutenant in the German Army during World War I. He was wounded on the Russian Front, taken prisoner, and was moved eastward when the Russian Revolution started. I don't know what motivated the Russians to preserve the prisoners, but they did.

DAEMMRICH: They were probably motivated to use them as labor.

PARISER: That's true. At that time, the prisoners were transported on the Trans-Siberian Railway. As my father told me, when the train was near the Chinese border, he and two others decided not to get back on the train when it made a rest stop, and they made their way into Manchuria, which is very near to Siberia. He made his way to Harbin. There he got a job with a Jewish family—the Shapiro family—teaching their kids because there were no schools other than Chinese schools. He was essentially an in-house tutor.

My mother arrived in Harbin shortly after my father. She was actually related to the Shapiro family, though I don't know how exactly. Her parents had owned a leather factory in Estonia that was destroyed during the Revolution, so she made her way across Russia on the Trans-Siberian Railway as a refugee. She made it to Harbin and reported to the Shapiro family where she met my father, and eventually they were married.

DAEMMRICH: What was your father's educational background?

PARISER: My father attended a university in Germany for a couple of years. He was also a very well known athlete. He played soccer and was on the All-Germany Team, where he was either the goalkeeper or the substitute goalkeeper. They played for the European championship, possibly against Holland, and lost. It so happened that my dad was the goalkeeper in that big game, and he never quite got over that loss.

In Harbin, my father was eventually promoted to the office of the Shapiro family, which owned quite a large importing and exporting business by the standards of the time. In due course, he took over the entire business and expanded it. I know he created an office in Shanghai in addition to the one in Harbin, and created a connection in Tokyo as well.

DAEMMRICH: So, he was first an in-house tutor, and then a businessman in the import/export trade.

PARISER: Yes. I believe he was importing textiles from Europe and exporting furs from Manchuria.

DAEMMRICH: Why do you think he didn't return to Germany after the War ended?

PARISER: His brother was running a retail store in Germany, which had been established by the Pariser family. I guess my father liked it in Harbin. He had a good position, and I don't believe returning ever really occurred to him. And of course, when [Adolph] Hitler appeared on the scene it was a given that he wouldn't go back. In fact, his brother left Germany and joined us in Manchuria around 1937.

DAEMMRICH: Was your mother's family originally from Eastern Europe, or were they Germans who had moved there for business?

PARISER: I'm not sure where they're from, but they're not from Germany. If anything, they might have had Polish or Russian roots.

DAEMMRICH: Did you ever visit Germany as a child?

PARISER: Not that I can remember. I believe they might have taken me there when I was around two years old, so I don't remember.

DAEMMRICH: Tell us a little about growing up in Harbin.

PARISER: I attended the German school in Harbin, the von Hindenburg Schule. It was one of very few non-Chinese schools there. In fact, it was really the only school that had any kind of stature among the European community in Harbin. Harbin had a couple of Russian schools as well—and Chinese schools, of course. The Russians in Harbin were mostly refugees of the Russian Revolution, so the schools had a strong Russian Orthodox influence. Also, a good number of the refugees were actually Russian nobility that had escaped the Revolution. Some were pretty well off. There were quite a few Russian students in the German school, and I would say there was more Russian spoken there than German.

DAEMMRICH: Were the courses taught in German?

PARISER: Yes. They were taught in German by German teachers.

DAEMMRICH: Did you interact much with the local Chinese community growing up?

PARISER: Not very much. The local Chinese community was divided into two classes: there was a large, extremely poor population and then there were a very few very rich Chinese. The very rich Chinese wouldn't have anything to do with us, and the very poor were mostly uneducated. Of course, we had two or three Chinese servants at home, but they all spoke Russian. So I could get by speaking Chinese, but I was not strong in that language. We communicated in Russian.

CREMER: How did the warlords in Manchuria affect your family?

PARISER: As a child, I don't think I was especially perceptive of them. I think my father was much more apprehensive because it was not a very lawful place. You never quite knew what might happen next. [laughter] I know my father employed a number of Chinese people in his office who provided some inside information on who was in charge at the moment, how to get by, who to bribe, and that kind of thing. But, our life there was a very good one. We had a very nice apartment, servants, a boat on the river, and so forth. There was a very nice Russian-operated hotel, and good restaurants. So it was a good place.

CREMER: How did the Japanese affect the atmosphere in Harbin after they took over in 1931?

PARISER: Before the Japanese took over, we lived in a very uncertain atmosphere. You didn't really know who was in charge. When the Japanese came in, I remember that it was a big bloody affair. As a child, I remember looking out our window and seeing bloody bodies all over the street from the battle. The Japanese, as occupiers, were never very gentle. But afterwards, they created a police force and I think things became more secure.

In the nineteenth century, China was very weak. Both Russia and Japan had tried to gain control over Manchuria, an area over 500 square miles rich in forests, agriculture, minerals, furs—which my father exported—and a climate like Minnesota. In fact, Minneapolis is the sister city of Harbin. Russia gained the upper hand in late 1890s. Many Russians settled in Harbin, and the city became known as "the largest Russian city outside Russia." Then, during the 1904-1905 war with Japan [the Russo-Japanese War], Russia lost and Japan became the primary foreign influence in Manchuria. They stationed an army in Kwantung, but did not control all of Manchuria.

The Chinese Revolution began in 1911, led by Sun Yatsen and Chang Kai Shek. They failed to take control of Manchuria, and it remained under the control of the warlord Chang So Lin. In 1931, Japan struck and occupied Manchuria. They made it a puppet state and renamed it Manchukuo in 1932. Then, in 1934, China's last emperor, Puyi, was proclaimed emperor of Manchukuo.

In the meantime, in Germany, Hitler was appointed Chancellor by President [Paul] von Hindenburg in 1933. By a series of maneuvers—he set the Reichstag on fire and blamed it on the communists—Hitler became the dictator before the end of 1933. Less than a year later, anti-Semitism was enacted into law in Germany. Many refugees from Hitler's Germany, particularly Jews, left Germany via Russia to Harbin, which became a major staging area. My mother worked tirelessly for years relocating those refugees to Shanghai, Australia, and South America. She was even able to relocate a few lucky refugees to the United States, but most were turned away because the United States' immigration quota system was not particularly open to them at that time.

DAEMMRICH: How did you experience the beginnings of World War II from the Far East?

PARISER: Adolph Hitler had become pretty dominant in Germany by 1934 or 1935. At that time, I was attending the German school in Harbin, and his long arm reached all the way to that school. A Hitler Youth group was created, and all that. Of course, my parents didn't go for that—being that they were Jewish—so they took me out of that school. They engaged a tutor, an Englishman—I don't know where they found him—and I was schooled at home by that fellow in the British style. That was done in English, and was probably around 1935 or 1936.

DAEMMRICH: Did your father ever talk about Hitler at home?

PARISER: Yes. As a matter of fact, before Hitler came to power, my father was a big wheel in the local German community, being a decorated war hero and so forth. Ironically, he got another decoration from Hitler after he came to power. Maybe he didn't realize who my father was. My father was very friendly with the German consulate there, and so on, and most of the German people living in Harbin were not enamored by Hitler.

DAEMMRICH: Were people in Harbin paying close attention to what was happening in Europe?

PARISER: Of course. I remember my father actually went to Germany and brought back a shortwave radio. I remember listening to Hitler's speeches on that shortwave radio. I also remember September of 1939 when Hitler said, "Today we're marching into Poland." I might have been the first person in Harbin to know that. My father was at work. He didn't know until I told him.

DAEMMRICH: What did you think of Hitler's speeches as you listened to them on your shortwave radio?

PARISER: His speeches were not calm deliveries. He screamed. [laughter] But they were highly emotional, and I can see how they could stir people up. He had a lot of charisma for people who were open to that kind of thing.

DAEMMRICH: You had a British instructor for some years, and then you were put back into a formal school, correct?

PARISER: That was probably around 1936. My parents—I don't know how—discovered an American missionary school near Beijing, and we went there. It had a dormitory, and I was enrolled into the eighth grade of that school, I believe. I was there for only one semester. During that summer, the summer of 1937, the Japanese entered Beijing and, in the course of the fighting, destroyed the school. I had very mixed feelings about that. I liked the school in a certain way, but it was also very strict. We went to chapel every day, and it was horrible on Sunday—it went on for hours! [laughter] I also remember that the seniors were the only students allowed to date a girl. Dating consisted of asking a girl to walk around what was essentially a quadrangle at the school for an hour while a teacher sat in the center. Anyway, the school was demolished in the fighting, and I wasn't too unhappy about that.

Then we started looking for another school. The candidates were the American School [American School in Japan (ASIJ)] in Shanghai and the one in Tokyo. We selected the Tokyo school because we didn't want the same thing to happen in Shanghai.

DAEMMRICH: You knew where the Japanese were going next?

PARISER: We could guess.

CREMER: Tell us more about your mother's role in helping Jews relocate during the War.

PARISER: My mother was a big wheel in an organization in Harbin that served as a center for collecting refugees. And, there were a lot of them. My mother and the group were finding homes for those people. Other than Harbin, there was no real future for them there, but I'll tell you about one or two instances of that sort. A lot of those refugees were sent off to Shanghai, which, although it didn't provide a permanent solution either, there was at least a larger operation where people could find homes in Australia, South Africa, and some South American countries. A few lucky people got into the United States, but it wasn't easy. They had a quota system in the U.S. based on the ethnic backgrounds of local populations, and the German quotas were pretty darn full because of those refugees.

There was one interesting person that came through as a refugee. He was the trainer, or maybe the assistant trainer, of Germany's boxing champion Max Schmeling. When he came by, my mother hired him to teach me boxing. I'll never forget that. I enjoyed it a great deal, and it's something that came in very handy at least once. He was a good teacher. I remember he kept emphasizing the left jab and the right cross. He kept saying, "The shortest distance between two points is a straight line. Just go straight. No swings, just straight." I remember years later when I was in college in California, and I happened to be in a bar when some big bully took a swing at me for some reason. He was much bigger than I was, but somehow I connected with his chin very quickly and knocked him down. [laughter]

Max Schmeling, as you may have heard, was a good man, basically. He never kowtowed to Hitler, and he became a very close friend of Joe Louis. He took care of Joe Louis financially in his later years, when Schmeling was a very successful businessman and Louis was not.

DAEMMRICH: What impact did the Japanese invasion have on your father's business?

PARISER: It did not seem to have any strong negative impact. I guess he just continued. He moved a good part of his activity from Harbin to Shanghai, because foreign concessions were still in Shanghai. There was a French concession, and a British one, all of which were kind of an appendage to Europe. You could operate within those and not be strictly subjected to the laws of the country that had the concession. I think he lived in the French concession and operated from there. My father was very lucky to be in Shanghai when World War II started because, by that time, the German government had got to the Japanese and explained to them that many of the Germans in Harbin were not real Germans—they were "bad people," i.e. Jews. The net result was that my father's employees at that time, two German refugees, were taken into concentration camps and did not survive.

DAEMMRICH: Tell us about the American School in Tokyo.

PARISER: I went to the American School in Tokyo in 1937 as a freshman. That was a delightful school. The American School in Japan is still going strong today. In fact, Louise [Margaret Louise Marsh Pariser] and I attended a school reunion about ten years ago in Seattle. There must have been nearly five hundred people there. It was a small school when I attended, with probably less than two hundred students from grade one through seniors, and more than twenty nationalities.

CREMER: Did you have any mentors at that school?

PARISER: As a matter of fact, I think I got into chemistry because of one of the teachers there, and also because of my father. Those were the days when plastics were just emerging and chemistry looked like the thing of the future. My father told me, "If I were you, I wouldn't be a businessman. I'd be a chemist."

DAEMMRICH: Was he pushing you?

PARISER: Yes. I lived in the dormitory of the American School in Japan, and the teachers, for the most part, lived there also. They were young recent graduates from the U.S. Most of them had master's degrees, and some of them had Ph.D.'s. They were not trained teachers, but in that school, you didn't need education credentials. They just went there for a couple of years, usually, for the experience of living in Japan. That was before World War II, of course.

My chemistry teacher had a master's degree in chemistry. His name was David Misner. He had a love for chemistry, and it showed. So he influenced me.

CREMER: You also had a physical education teacher there named Jim Rasbury, correct?

PARISER: Yes. Jim Rasbury had a law degree from [University of] Michigan, and he went to Japan to teach physical education. He was a tall fellow, and he lived in the dormitories. I liked him, and we became close. Now, while I was in Japan, we were given two or three weeks of Christmas vacation, but there was no airplane traffic. So it took about three days to get from Japan to Harbin by boat or train, which would chew up most of the vacation. As a result, my parents would come over from Harbin and we would spend vacation in some resort in Japan, which usually was very pleasant. Over the course of several visits, they got to know Rasbury because he lived in the same dormitory as I did.

Sometime in late 1939, Jim Rasbury disappeared from the school, and rumors spread that he had been fired. The rumor was that he had been carrying on with some Japanese geisha girls, or a bit below that probably, which was unbecoming of a teacher. The school had been and still was a missionary school, but it had become very secular because many of its supporters were the embassy and business crowd in Tokyo whose children were students at the school. There was a certain degree of religious influence, but it wasn't a strict missionary school. There was no chapel and so forth as there had been in my previous missionary school near Peking.

Anyway, Mr. Amos, the school's principal, was approached by the students and asked about Mr. Rasbury. Mr. Amos didn't give a clear answer, so we assumed the rumors were true. And then, that summer—I guess it was in 1940—Mr. Rasbury appeared suddenly in Harbin and made a call on my father. He stayed with us for about three or four days, and the two of them had long private discussions. Then he left and my dad would not talk about it. A year or two later, he told me that Jim had been an American spy in Japan and that the Japanese had caught on to him. They expected him to try to escape back to the U.S. by ship, so Rasbury decided to run the other way, into Manchuria. He took a chance calling on us, because he had met my parents. My dad helped him go from there to Russia. At that time, Russia was not yet in the War. I understand he got to Vladivostok, a Russian port, got on an American freighter, and went back to the U.S. As you probably know, I ran into him again.

DAEMMRICH: Tell us about that.

PARISER: Tension had been growing between Japan and the United States, so that by the summer of 1941 there were very few students left at the American School. The American ambassador, Joseph [C.] Grew, had urged people to leave. We had a quickie graduation in Ambassador Grew's embassy, since by that time there were only five or six of us left—I was class president. [laughter] At that point, my parents decided to send me to the U.S. with my mother. The plan was that she would place me into some kind of college and then return to China. But while we were in California, Pearl Harbor was bombed and she couldn't return. We had seven thousand dollars, which was probably worth near one hundred thousand dollars by today's standard. It was substantial. My mother bought a small apartment house with it, and we rented out apartments. We lived in one of them, and I enrolled at the University of California at Berkeley [UC Berkeley], and spent the first semester living in the International House, or I House. It was a wonderful experience! The International House is still going strong.

The strange part was that we were classified as enemy aliens because our passport was German, and to the U.S., a German passport was a German passport. [laughter] We had curfews, restrictions, and the like, and it was not very pleasant. Of course, the United States government had already deported and relocated all the people of Japanese descent even though they were American citizens. At the same time, my mother and I were seeking some kind of employment because we needed money. Even though I was going to school, I wanted to help. I saw an ad in the *San Francisco Chronicle* that said, "Russian translator needed." So, I thought, "This is perfect."

I reported to the place, which happened to be the FBI [Federal Bureau of Investigation] office in San Francisco. They put me into a waiting room, and as I sat waiting for my interview, I noticed the name, "J. T. Rasbury" printed on the door. I thought, "My god. Is this the same guy?" I asked the secretary, "Has this fellow ever been to Japan?" And she said, "Yes, sir, he is our Chief." I said, "I knew him in Japan." She replied, "I'm sure he'd want to meet you." She got him out of the office, and it was Jim! That was a godsend because I got the job and later on he helped to get me a much better paying job at the shipyards in Richmond, California. He got us special I.D. [identification] cards too, so we didn't have to obey the curfews and restrictions. He also helped my mother get a very good job at a swanky store in San Francisco, which is now a Neiman Marcus. [laughter] At the time, it was called City of Paris, and she was in charge of the jewelry department.

DAEMMRICH: That's great. Let's return to your experience of coming to the United States. You came by ship at the time?

PARISER: Yes.

DAEMMRICH: Do you remember the crossing?

PARISER: Yes, I do remember the crossing. The highlight of the trip was our stop in Hawaii. It was the first time I set ground on U.S. soil. It was beautiful there, and I remember staying at the Royal Hawaiian Hotel for two or three nights. I guess the ship docked there for a few days. It was one of the few hotels on Waikiki Beach at the time, which is very different today. [laughter] It's still there. Louise and I are planning to be in Hawaii next month and we will stay there.

CREMER: How long did the trip take by boat?

PARISER: It took about two weeks. It was a Japanese ship. It was the very last one that completed the voyage from Yokohama to the U.S., and docked in Los Angeles. I believe it was on 4 July 1941.

DAEMMRICH: When you entered the country, how did you go from being an alien to a student at UC Berkeley?

PARISER: I was a graduate of the American School in Japan, and I had fairly good grades. In addition, the school was recognized by the University of California. They had had students from there before. ASIJ had been around for such a long time.

DAEMMRICH: Describe the atmosphere of UC Berkeley when you first started.

PARISER: Of course, in the beginning it was just before Pearl Harbor. As I already mentioned, I stayed in the International House where I enjoyed life greatly! After 7 December, it became a very warlike atmosphere. It was not the happy-go-lucky college life by any means, and I could not afford to continue living in the I House. The University instituted all kinds of accelerated programs and students tried to get as much done as possible before being drafted. That applied to me, too. I got my degree in chemical technology in about two and three-quarter years. I took accelerated class schedules and worked on the night shift at the shipyards during much of that time, too.

CREMER: You said, “working” sarcastically. What did you mean?

PARISER: I worked either during the “swing shift,” that is from 4 PM to midnight, or on the “graveyard shift” from midnight until 8 AM. Fortunately, we didn’t have to work very hard. I had a job as a ship fitter’s assistant. The chief ship fitter called me as he needed me. I don’t recall working very hard. I also studied there.

DAEMMRICH: I guess you brought your homework with you and worked.

PARISER: Yes. But every now and then the chief ship fitter would call me out and I’d have to hold onto a chalk line while he made markings.

CREMER: When you lost communication with your father, how long was that for and how did your mother cope with that loss?

PARISER: When the Japanese occupied Shanghai during the War, my father managed to buy himself a Portuguese passport. That was common in Shanghai. Portugal was neutral in the War so the Japanese didn't bother him. He had blond hair and blue eyes, so he didn't look Portuguese and he didn't speak one word of Portuguese, but they bought it. [laughter]

I think there were one or two occasions during the War when Jim Rasbury got word to my mother that he was all right. I guess the FBI had connections in Shanghai, so my father made out all right. I do remember that when the Japanese surrendered, a Japanese officer sold a great big suitcase full of cultured pearls to my father for very little money. When my father came to the United States in 1946, he brought that suitcase with him. It was enough for him to buy an automobile agency in New York City, where he became a Ford dealer for about ten years. That was very good for me, because I got to drive a new Ford convertible every year, including a 1956 Thunderbird!

DAEMMRICH: Let's go back to UC Berkeley where you did your undergraduate studies. Did they very quickly slate you into a major, or did you choose to study chemistry?

PARISER: I chose to study chemistry. I got a deferment, at least for the first year or two, because I was studying what is considered chemical engineering today, which was important for the War. After graduating, I got a job with Kaiser Permanente Mills, not far from San Francisco. I reported to them as a chemist for a while, but it didn't feel right. I felt very strongly about the War and I enlisted.

DAEMMRICH: What was the curriculum like at Berkeley in that compressed timeframe?

PARISER: The chemistry at Berkeley was stupendous, and that was what really sold me on Berkeley. As a freshman in chemistry, my main lecture was delivered by Professor Joel [H.] Hildebrand, who is well known even to this day. In fact, the rule at Berkeley was that all the full professors had to teach freshmen. G. N. Lewis was the chairman of the department. He was like a little dictator. My lab instructor was Professor [William F.] Giauque. He got the Nobel Prize later on for adiabatic magnetization, demagnetization, and reaching down toward the absolute zero temperature. I had [Melvin] Calvin in organic chemistry, who also got a Nobel Prize later on. I had [Wendell M.] Latimer for inorganic chemistry, who should have got the Nobel Prize. Those guys were top notch. In physics I had Frank [F.] Oppenheimer, [J.] Robert Oppenheimer's brother, who substituted for Robert.

DAEMMRICH: Robert was out in the desert working on the Manhattan Project?

PARISER: He must have been busy with that because we did not see much of him, and his brother took over his courses.

We also had [Glenn T.] Seaborg. He was an excellent teacher. So, it was very inspiring, and I was very fortunate. At the time, it probably was the best chemistry department in the country, or it must have been very close to it. Berkeley was very, very good, and it was all hard but inspiring work.

DAEMMRICH: Do you remember any of the laboratory work? Did you ever work as a research assistant for any of the professors?

PARISER: No. I just did the coursework and lab work. In fact, I think the stress of work and school was pretty hard on me and I got a case of pneumonia while I was studying. I was taken to the infirmary and remained there for nearly one semester. I studied mainly out of books, but I did very well. I got excellent grades that particular semester, probably because I did not have to work in the shipyards full-time.

Anyway, after graduating I did have a job for a short while with Kaiser, but then I enlisted in the [United States] Army, as I already told you. That was kind of interesting. I reported to the Presidio [of Monterey] in Monterey, California. They interviewed me and said, "You'll have a good job in the Army. You know all these languages. You have a degree in chemistry. You've got it made. You'll probably have a great job in intelligence or something." I thought, "Wonderful. I'm going to get a great job in the Army."

Every morning they would call the troops out of the barracks and make assignments. That morning they said, "Private Pariser, fall out. Infantry." I said, "What?" [laughter] So, I went back to the Lieutenant who had interviewed me and had told me what a great job I was going to have. And he said, "Your misfortune is they classified you as physically A1, the highest physical classification, and we really need people like that." So I was put into the infantry.

From there, I went through basic training at Camp Roberts in California. They were obviously preparing us for what later turned out to be the Normandy Invasion. One morning, near the end of our basic training, I remember the training field in Camp Roberts had transport planes lined up on it. All of us were being called out and they were loading people onto the planes. It turned out later that people from our group were being flown to England. Many of them participated in the Normandy Invasion.

CREMER: Did you take part?

PARISER: No. I'll tell you what happened to me. When they called my name they didn't say, "You're going to embark on plane number seven," they said, "Fall out!" So, I stood there by myself, thinking, "What the heck's going to happen to me?" [laughter] I wasn't anticipating anything good. Then after a while, another guy joined me; and actually, this fellow is still a very good friend of mine. We started talking and we discovered that we both spoke German. Then a third, a fourth, and a fifth guy joined us and we all had that in common, so it became obvious they had something else in mind for us. We never made it to the invasion of Europe, luckily, because there were many casualties in the group that had been sent over. Actually, the men selected from our infantry training were used in the invasion more than the soldiers that were already there, in Great Britain, because we had been better trained. The group that had been called out to stand aside grew to sixteen people. We were all sent off to Ohio State University, where we were to be trained for the occupation of Germany.

Things were very different back then. There was a lot more planning, obviously, because we were being trained at Ohio State for the Occupation of Germany. Each one of us was told to which village we would be sent. We were taught about that particular village; we got to know the mayor, the map of the town, and a little bit of the history, all of that. Actually, I never participated in that occupation, but some of my friends did, and they were well prepared.

CREMER: Had you gone would your objective have been to convey information from the U.S. troops to the German people?

PARISER: No. We would take over the local village government, temporarily. We were told that we would be the governing body of the village until something else more permanent was established. They had to get rid of Hitler's regime and put something else in its place, and we were it. My friends who took part in training for that mission were commissioned, and then they became the governments for these towns and villages. At least that's what I heard.

CREMER: How long was that training?

PARISER: We were trained intensively for three months, I recall. And we were just the first training group to arrive at Ohio State. Another group arrived after four weeks, and then another, and soon it became quite a large operation.

Just short of graduation, my group was shipped to the Signal Corps at Camp Crowder in Joplin, Missouri. When we arrived, nobody knew who we were. We showed them our papers, and they said, "It appears that you are supposed to be here." So they kind of put us aside and for a while they didn't know what to do with us. We did nothing for about a month. Later they said, "You're here and you've got to do something, so we'll put you into the Signal Corps." [laughter] One of the people in our group got very upset by all this and he started to investigate what happened. He informed us that someone made a mistake in Washington, obviously, and we were just shipped off to Missouri.

While I was there, I took the Signal Corps training, applied to Officer Candidate School [OCS], and was accepted. I was sent to Fort Monmouth, New Jersey, to the Signal Corps OCS, and the War ended while I was there. When it came time for me to graduate, my superiors said, "We're not so sure about you Private Pariser. We're worried that you don't have enough of a commanding presence. We suggest that you go back and take two more months of OCS, and then we'll re-evaluate you." I thought, "The War is over now."

There was a point system for being discharged, and if you had a certain number of points, which depended on your length of service and a few other things, you were eligible for discharge. I was not far from being eligible. So I asked, "If I were to become an officer, what would happen afterward?" They replied, "Afterward, you'd be committed to at least two more years of service." So I said, "No." [laughter] They replied, "In that case, we'll put you back into the Signal Corps." I said, "Fine." I was discharged shortly after that.

DAEMMRICH: What did you do after being discharged?

PARISER: I wanted to go back to California. I had heard of the G.I. Bill and thought, "I'll go back to the University of California, which I love, and continue my studies." I arrived there, and as a veteran, they had to take me, but they did so reluctantly. I was a resident of California, and they said, "It is our strict policy not to take our own graduates back into our graduate school." I said, "I have no choice." [laughter] They replied, "We'll see what we can do for you." So, they started looking around and they got me a fellowship at the University of Minnesota, which was too good to turn down. I had the G.I. Bill, and the fellowship earned me an additional two hundred dollars a month. It was an Office of Naval Research fellowship.

DAEMMRICH: So you entered the chemistry department. Would you describe your Ph.D. training?

PARISER: My professor was [Robert L.] Livingston, a photochemist. I did my research with him on chlorophyll sensitized reactions; photochemical reactions of in-vitro solutions. That was very interesting work. I was also influenced strongly by Professor Bryce [L.] Crawford—for whom Bob [Robert G.] Parr worked. Parr was doing a theoretical thesis at that time, and he was about three years ahead of me at Minnesota. William [N.] Lipscomb was another professor that influenced me a good deal. He won the Nobel Prize later on. Frank [Henry] MacDougall, who wrote the book on thermodynamics, taught me thermodynamics (1). It was a very, very good school.

DAEMMRICH: How many grad students were there at the time?

PARISER: A lot.

DAEMMRICH: So, you wouldn't have necessarily run into Parr a great deal.

PARISER: I knew him. MacDougall taught thermodynamics and we had to take more than one year, and Parr, who had graduated already, became an interim assistant professor. I happened to be a student of that course. Although Parr is a dear friend of mine, he may not be the greatest teacher in the world to some people. In fact, some people may have difficulty in understanding him, but I had a knack of understanding him—and I still do to this day—so I greatly enjoyed his course. Bob had a very different way of teaching, which "connected" with me. So that's how I met Parr.

DAEMMRICH: Please discuss your photochemistry research. What was happening in the lab? Were you grinding up plants to get chlorophyll out to do these studies?

PARISER: Yes. We extracted chlorophyll from the plants and purified it chromatographically. Then we studied oxidation-reduction reactions in solution, photosensitized by chlorophyll or related structures. In screening experiments, I found that pheophytin appeared to work as well as chlorophyll. As you may know, chlorophyll has a magnesium atom in the center of the porphyrin ring, and pheophytin is the derivative, whereby the magnesium is simply replaced by hydrogen when the magnesium is removed. This derivative has an absorption spectrum similar to chlorophyll. Pheophytin, however, is considerably more stable than chlorophyll under our experimental conditions. So in order to make things simple, we chose to study the reaction of dimethylaminoazobenzene, also known as butter yellow, with ascorbic acid (Vitamin C) photosensitized by pheophytin. The light absorbed by pheophytin energized ascorbic acid to reduce butter yellow to the amines in methanol solution. We studied the quantum efficiency in those reactions in depth as the main part of my thesis. Now, dimethylaminoazobenzene is recognized today as a strong carcinogen. I didn't know that back then. [laughter]

Anyway, this hopefully simple system turned out to be very complex, since we found that the reaction product, dimethylaniline, had a strong catalytic effect on the reaction. In retrospect, it might have been simpler to stick with chlorophyll, which is of course what plants choose to use.

Later on, I invented an apparatus by which we could run a multitude of reactions at the same time. It was sort of a carousel with sample reactions rotating around a central light source. We studied a good number of photosensitized reactions, involving both pheophytin and chlorophyll, and various dyes and reducing agents. A number of important concepts were supported by our research, namely the existence of a long-lived excited state in chlorophyll, which we believed to be a triplet state. Also, the quantum yield appeared very sensitive to the type of reducing agent employed.

DAEMMRICH: Did that become the basis for your thesis?

PARISER: Yes. That and the reaction with butter yellow which I mentioned. Three publications in *JACS* [*Journal of the American Chemical Society*] resulted from my thesis research (2). In retrospect, my thesis work provided very useful background for my initial research at [E. I.] du Pont [du Nemours & Company], as well as for my later collaboration with Bob Parr.

CREMER: Do you remember giving your first scientific talk?

PARISER: It was in Minnesota. My professor, Livingston, asked me to give a talk to a bunch of students about my research. I must have done a terrible job because he said, "Don't you ever, ever give any talks again in the future!" [laughter]

CREMER: Did you attend many scientific conferences?

PARISER: No. Back then, we finished our Ph.D.'s in about three years, whereas today it seems to take five or six, and it was not common for students to go to scientific meetings. We did not have the research support dollars available today.

DAEMMRICH: Why did that go so quickly? Was there less coursework?

PARISER: There was a lot of coursework, and a lot of research. I think that perhaps we just worked harder than students do today, and there was a very good incentive to get out. I had a fellowship, of course, but I knew it wouldn't last forever. In fact, I think it was supposed to terminate after three years, so I had a strong incentive to get my degree before then. We didn't have as many graduate fellowships as they have today, which seem to go on forever. Moreover, most of us were veterans so we were used to intensive work under pressure, I suppose.

DAEMMRICH: Would you describe your Ph.D. defense?

PARISER: As I recall, in attendance were Professor Alfred [O. C.] Nier, from physics, and Professors Crawford, [Izaak M.] Kolthoff, and Livingston, from chemistry. They asked good questions, but I did not have any trouble responding to them.

I was being interviewed for jobs around that time, too. I was invited to two interviews, one from Smith Kline and French [Laboratories] in Philadelphia, and one from DuPont. The interviewer from DuPont, Dr. Sam Scott, actually came to interview us at the university, but the one from Smith Kline never did. Sam Scott made a very strong impression on me. I think I was invited to interview at Smith Kline by mail. I went to Smith Kline first and that proved to be a bit of a disaster. I didn't really like them, and I don't think they liked me, so nothing came of it.

I interviewed at DuPont's Jackson Laboratory, which is located at Chambers Works. It's across the Delaware River from Wilmington, in [Deepwater] New Jersey. We had to travel to Jackson Laboratory by ferry from Wilmington, but the Delaware Memorial Bridge was under construction. I was not very enthusiastic when I heard about Chambers Works because I imagined it having a plant atmosphere. I thought I would be given some kind of plant work similar to the work I'd done at Kaiser years before, but when I arrived at Jackson Lab, I was very impressed. Those guys were real chemists. The person that really interviewed me a lot was Herman [E.] Schroeder. I liked Herman instantly partly because he appeared to understand my thesis, and obviously he liked me, too. John [M.] Tinker, the Lab Director, also interviewed me and that also went well. I was offered a good position at Jackson Laboratory and I accepted with pleasure.

DAEMMRICH: Did Jackson Lab feel at all separated from DuPont Central R&D [Research and Development]?

PARISER: Yes, but there was some interaction. There was more frequent interaction with Central Research later on when I got into quantum chemistry, but Jackson Lab was a terrific research institution in its own right at that time. Charlie [Charles J.] Pedersen (who later on won the Nobel Prize) worked there, and a lot of new products had been created at Jackson Lab. Tetraethyl lead (TEL), Freon refrigerants, Neoprene rubber, Teflon, and various pigments had been developed there, followed by the development of Viton fluorelastomers, Hypalon chlorosulfonated polyethylene, as well as isocyanates and polyurethanes. When I started at Jackson Lab there was a major effort in dye chemistry, for which Jackson Lab was already world famous (3). That effort has now been redirected in view of the fact that DuPont was strongly getting into the synthetic fibers, like Nylon, Dacron, Orlon, and so forth. Jackson Lab had to develop ways of coloring those new fibers. There must have been more than one hundred chemists at Jackson Lab doing synthetic work with that objective.

One of my first assignments was to look at the stability of whitening agents that fluoresced in the blue and were used to neutralize the yellow color in the fabrics. The problem was that these whitening agents were not light stable, and after a while the whitening would go away because the fluorescence would die out. That was a very interesting problem.

I studied it, and I found out that those whitening agents were stilbene derivatives, with lots of other structures hanging on the central stilbene structure. In those days, at first we didn't have modern spectrophotometers. We still used the Beckman DU, so we had to run one wavelength at a time, by hand. However, the automatic Carey spectrophotometer had just become available, and that helped a lot. In fact, I was able to do our studies with the whitening agent adsorbed on, say, a cotton substrate, by measuring the reflection spectrum. I discovered that those stilbene derivatives lost their fluorescence because they started out in the <u>trans</u> form and the light made them isomerise to the <u>cis</u> form. The <u>cis</u> didn't fluoresce, but the <u>trans</u> did, so you got a photo equilibrium based on that. I found you could make the <u>trans</u> come back by irradiating in a wavelength that the <u>cis</u> would mainly absorb compared to the <u>trans</u>. So, I advised, "If you want to make these whitening agents more light stable, you would have to make a structure that did not easily change into the <u>cis</u> form." I told Professor Calvin from Berkeley, who was a consultant at Jackson Lab at that time, about my work, and as I recall, he proceeded to do research on stilbene itself in solution. He published his results and was recognized as discovering the phenomena (4). [laughter]

DAEMMRICH: Did he make the photostable version?

PARISER: No, that was not his objective. He just published a paper and described the full photo-isomerization of <u>trans</u> to <u>cis</u> stilbene, which was a new discovery at the time. I had not studied stilbene by itself. I studied the whitening agents based on the stilbene structure but there was little doubt that stilbene would behave in the same way.

DAEMMRICH: Who were some of your mentors early on at Jackson Lab?

PARISER: Herman Schroeder was one. I may have been in his group briefly at first before he was promoted. Al [V.] Willett took his place as my Division Head. He was very supportive, as was John Tinker. We had a lab of two hundred Ph.D.'s, but there were only two or three physical chemists, and one physicist. Herman and John Tinker understood us. In a way, they were very supportive of doing physical chemistry in that sea of organic chemists, some of whom seemed to feel that physical chemists were mostly useless.

After I finished my stilbene work, Al Willett asked me, "What do you want to do?" He didn't quite know how to direct me. I said, "All of these people are working on dyes, and I can't possibly analyze the spectrum of all those dyes, even with the Carey spectrophotometer—it would take forever and I'm not sure anything much would come of it. I would like to research the relationship between a dye's structure and its color." Al didn't quite know how to direct it, so he said, "Wonderful. You do that." [laughter]

DAEMMRICH: Please discuss the shift at DuPont into that new research direction.

PARISER: I had some regular graduate courses at Minnesota in quantum mechanics. Bryce Crawford and Bill Lipscomb, taught them, and I enjoyed them. I remember a quote from Peter [J. W.] Debye, which went somewhat as follows: "And hereby, most of the laws of physics and all of chemistry are completely understood and what remains is their application." I did not see the ending of the quote, which said that the equations were much too complicated to solve! Instead, I thought, "My god. It should be a cinch." [laughter] So I bought the book, *Quantum Chemistry*, written by [John] Walter, [George E.] Kimble, and Henry Eyring, and I started studying it (5). I was not really trained in the field, but I enjoyed teaching myself that material. I taught myself simplified Molecular Orbital Theory, but there was nobody I could interact with at DuPont. So, I went to Al Willett, my boss, and I said, "I need to have somebody to talk to about this. I need a consultant." He replied, "Fine. Who would you like to do it?" I said, "I know Bob Parr did his thesis work on calculating energy levels in ethylene and benzene (6). Maybe I could talk with him?" And he agreed.

Parr was at Carnegie Tech [Carnegie of Institute of Technology, now Carnegie Mellon] at the time. I visited him in June of 1951, and told him I wanted to hire him as a consultant. He was flabbergasted. He didn't expect anybody would ever want to hire him, a quantum chemist, as a consultant in industry. It didn't seem practical. I said, "I'm trying to understand the structures of dyes and how they relate to their color. I would like to be able to calculate the color of a dye from its structure." The great thing about Bob was that he didn't laugh at me. He said, "Good idea." [laughter]

I didn't know this at the time, but in his thesis and in his work on benzene he had calculated the energy levels of benzene in the pi-electron approximation—sort of *ab initio* from first principles—and there was absolutely no agreement between his results and the experimental spectrum (7)! [laughter] It took him a good part of his Ph.D. thesis just to do benzene, and here he'd get to do complex dyes? Benzene has so much symmetry that you can do a lot very quickly, and it almost gets down to a two-atom problem. But he went along with it with considerable optimism

Then I said, "Since you're going to be a DuPont consultant, my management would like to meet you, and then perhaps you could give a little talk about your work." He didn't feel very confident about that but he agreed to do it. Bob talked mainly about the hydrogen molecule and illustrated some basic quantum mechanical concepts. I remember he felt badly after giving his talk because he didn't think he got his point across very well, which was probably true. [laughter] He talked way over their heads.

I had not been trained in theoretical chemistry, so he helped me learn the matrix algebra, group theory, and math such as that. It was not too long after that, in November of 1951, that he called me and said, "You know, I spent a few years calculating benzene, and I think I can see a way of doing the same thing much quicker. All you have to do is neglect all overlap between the atomic pi orbitals when you calculate the repulsions between electrons (this was later named zero differential overlap, or ZDO)." I did benzene that way. It didn't take very long and I got answers that were almost identical to the full-blown treatment, as Bob Parr had claimed. The full-blown treatment requires the calculation of electronic repulsion integrals, which are very complex mathematical situations where orbitals overlap and you have to calculate the electrostatic repulsion between electrons in the overlap regions. Such integrals involve calculating the electronic repulsions between two electrons, which may be distributed over one, two, three, or four atomic centers including the overlap regions between the centers. If this sounds complicated and difficult, it definitely is!

It didn't take me very long to learn the method based on zero differential overlap, and I could do benzene very quickly on my mechanical calculator. Then I said to my lab assistant, "Let's try to do naphthalene this way." She spent at least a month on the calculator, but eventually she did naphthalene. As for benzene, the results had no relation to the experimental spectrum—that is the color—at all. [laughter]

At that point, my thesis work in chemical kinetics came to the rescue. In chemical kinetics, we measure reaction rates and develop a mechanism for the reaction, and there are rate constants involved in the equations. Then we go back to experiment. We try to fit the theoretical result to the experimental result, and in this way calculate the values of the rate constants. So, I thought, "Why can't we do that here?" We could take the zero differential calculation for benzene, which is easy to do, and assume we don't know the value of the integrals of which there are only four for benzene in the ZDO approximation. Then we could fit the experimental spectrum of benzene to determine the value of the integrals needed to make it come out right. Those integrals relate to the electronic repulsion between electrons on any two atoms in benzene, as well as to the one-center repulsion where both electrons are on the same atom.

For benzene, I made an assumption. I said, "If the electrons are far apart, such as in the *para* position, then the theoretical calculation should be pretty close." So we used the theoretical value for that repulsion integral. The "experimental value" for the next nearest neighbor (1, 3 position) came out to be almost the same as the theoretical value, although it was somewhat lower. So that gave me a lot of confidence and it made sense. Then we went to the nearest neighbor repulsion integral, and again, it was lower but not that far off the theoretical value of about 9 eV, although it was over by about 2 eV. Though those "experimental values" were reasonably close to the theoretical, one-center repulsion integral where both electrons are on the same atom was way off. The theoretical value for the one-center repulsion integral is about 17 eV, whereas the experimental value was only about 11 eV. I looked at that number, 11 eV, and I said to myself, "How come that's so far off and all the others are not that far off?" Then I realized that 11 eV is close to the ionization potential of carbon. So I thought, "Let's take two carbon atoms, one electron in each, and move an electron from one to the other." So we went from having two neutral atoms to a plus-charged and a minus-charged carbon atom. That's the net reaction. The change in energy for this "reaction" ought to equal the ionization potential of carbon, minus its electron affinity. That is, you take one electron away, and that's the ionization potential, and subtract the electron affinity, which puts the electron back on the other atom. Theoretically, that energy change in the pi electron approximation (which we were using) is also equal to the one-center repulsion integral. So, the one-center repulsion integral should be equal to the ionization potential minus the electron affinity, or a little less than 11 eV. That's when the "eureka" came in. [laughter] It was a breakthrough equation. That one-center electronic repulsion integral (1111) can thus be written as $(1111) = I – A$. Of course, the calculation for benzene now came out fine. Then we used the new "experimental" integral values in naphthalene and, to our surprise, it worked. [laughter] I remember Bob came to Wilmington, we spent most of an evening talking about our results, and we both agreed that our approach had to be right. The adjustment of the pi electron repulsion integrals was compensating for the changes in the core sigma electrons that are formally ignored in the pi electron approximation.

However, we were still a long distance from our goal of calculating the spectra of dyes. I realized we couldn't analyze dyes by having my lab technician run those calculations on the desk calculator. It was still much too time consuming, even with the ZDO simplification. At that point, I'd heard that IBM [International Business Machines Corporation] had larger computers, so I went to their facility on Madison Avenue in New York City. I found they had what they called a Card-Programmed Electronic Calculator. It used vacuum tubes and IBM cards, and there was practically no built in memory to speak of. The memory was created by punching IBM cards and feeding them into the computer. So when we tried to run the same naphthalene calculation, we had to use cartloads of cards.

I worked with a very helpful mathematician at IBM, John Sheldon. We worked there at night because that was the only time we could get to the computer with a cart full of punch cards. I remember one night we were pushing a cart when it tipped over and all of the cards spilled out onto the floor! [laughter] It was nearly midnight, and we said, "To heck with this!" [laughter] Then John told me about a new computer being built at IBM. He said, "They're working on a new computer here, a big one that doesn't use these cards for memory. It will use transistors instead of vacuum tubes, magnetic tape for memory, and it will have a faster, smaller electrostatic memory." In fact, the computer we had been using was vacuum tube-based. If a vacuum tube blew out before the computation was finished all of the calculations were lost. That was another problem with the Card-Programmed Electronic Calculator.

CREMER: How long did it take to run a cart full of cards?

PARISER: It was not something you did in seconds. It took hours. If everything worked, which was uncommon, you could probably do naphthalene in an hour or two. But we never succeeded.

DAEMMRICH: How many different compounds were you trying to run?

PARISER: We were just trying to go beyond benzene. [laughter] I think naphthalene was the next thing up, and then anthracene, and so forth, to tetracene and pentacene. So I said, "This new computer, this supercomputer, that's for me." That's when John Sheldon said, "Let's see if we can't enroll you in a class at IBM that will teach you how to use that computer." So I enrolled in the class, in which there were probably twenty-five other students. I was the only non-IBM person there. [laughter] We learned how to program the computer in machine language.

CREMER: Was that class for the IBM 701 Electronic Data Processing Machine?

PARISER: Yes.

CREMER: Had the computer been released publicly yet, or were you using a prototype version?

PARISER: I am quite sure that it was the first 701. You might call it a prototype.

DAEMMRICH: How did IBM react to having a chemist using their computer?

PARISER: They were very supportive. I think they liked the idea of finding applications for their new computer. Some years later, they asked me to talk at a symposium as the first chemist (to their knowledge) to have used a "modern," high-speed, IBM computer.

I assume you know the story of [Thomas J.] Watson, one of the founders of IBM? When they were developing the PC [personal computer], he was asked, "What do you think the market potential is for this?" And he replied, "Maybe five or six."

DAEMMRICH: I'd like to take you back to that first meeting in 1951, when you met with Bob Parr. In the report we found in our archives, you described that a few weeks previously Dr. Halverson of American Cyanamid [Company] had visited with Parr (8). You wrote, "His interest in Parr's work confirms our suspicion that Calco [Chemical Company] is engaging in molecular orbital work which is similar to ours." How were you keeping up with your competitors? Was there a bit of a race going on?

PARISER: There is always a feeling of competitiveness in any industrial lab. I believe we kind of latched on to that to help us sell the project to DuPont.

DAEMMRICH: Let's actually come back to the series of articles that begin defining PPP [Pariser-Parr-Pople] theory. Would you describe that development?

PARISER: Yes. The publications started to come out before we did major calculations at IBM. As soon as I discovered the one-center integral equation, Bob said we should publish. He said, "Both of us should write letters to the editor. I'll write about zero differential overlap, and you do this other one about the one-center repulsion integral." So we did, and the two communications appeared in tandem, as well as two comprehensive joint papers describing our theory (9). That was the first step. The next step was a paper on ethylene-like molecules (10). We worked on that paper together using our theory. We also pointed out in that paper that our approach should also be applicable to saturated molecules; that is, not only pi-electron systems. I think the work at IBM must have started around that time. Incidentally, during the first few years after our initial publications, our theory was referred to as PP, rather than PPP; John Pople's "P" joined ours later.

DAEMMRICH: Do you remember any of the initial reviewers' comments when you first wrote the joint article?

PARISER: The reviewer's comments came later, when we started using the IBM computer. The reviewers from the common chemical community, at that time, were—how should I put it—somewhat negative. Basically, they said, "You guys are not playing fair using your computer." [laughter] But they didn't say it in so many words.

PARISER: We presented our work at a Spectroscopy and Molecular Structure Symposium at Ohio State University in June of 1952, which was really the first major publication of our work. The Ohio State symposia were the utmost at that time for presenting original work in spectroscopy and molecular structure. Robert [S.] Mulliken, later a Nobel Laureate, was in the audience when we presented our findings. There was total silence after our presentation, and then, finally, Mulliken stood up and said, "This is very interesting," and then he sat down. Mulliken was a man of very few words, but each word counted for a lot. His few words were great because we were not at all sure how well we would be accepted. Some people must've been disappointed, or even jealous, to some extent, because they had spent much of their working lives trying to do something with Molecular Orbital Theory and hadn't got very far. Then, here we come with a new theoretical approach that appeared to work much better than the existing Huckel pi-electron approximations.

That work was followed by my calculations, using the IBM 701 computer, of the polyacenes: benzene, naphthalene, anthracene, tetracene, and pentacene. The calculations were done in a few sessions at IBM and my machine language program worked perfectly! I probably still have it at home somewhere. That work was presented at the Ohio State symposium in 1954. It was well received, especially by the experimentalists who appeared hungry to have something challenging to shoot for with their measurements, such as difficult to measure triplet states.

The numerical answers we got from the computer for the polyacenes were terrific. To this day, I don't know why they were so good. The real boost came when various experimental people read those papers. For example, I remember distinctly how we would predict the location of a triplet state of naphthalene. It was Professor Don McClure, an experimentalist, who did some beautiful experiments searching for the triplet state and found it, and then said, "It was exactly there where you said it was!"

There was also a certain amount of critique. They said, "Your work is all based on alternant hydrocarbons." An alternant hydrocarbon is one where you can put a star on every other atom with no two stars being nearest neighbors. An alternant hydrocarbon presents certain important simplifications in the theory of molecular orbitals. In fact, in one of my papers I derived many simplifying rules that would apply because of that (11). But that opened a door for critique, because people said, "Your theory probably works because you have applied it to alternant hydrocarbons and many things will cancel out because of that." I responded, "We'll try it on a non-alternant like azulene." Azulene is a fusion of a five- and a seven-numbered ring, so it's non-alternant. We did that calculation and it worked beautifully. Azulene, because of its non-alternant structure, has a dipole moment, and our calculation was in good agreement for that as well, as for the spectrum.

DAEMMRICH: Are most dye molecules symmetrical?

PARISER: Most of the best are fairly symmetrical. We did some work in applying our theory to dye molecules, but dye molecules are a lot more complex in structure. They have many non-carbon atoms, like nitrogen. In our basic publications, we described some of our early work on nitrogen containing compounds, such a pyridine. Also, during that period, we provided a certain degree of guidance to the synthetic chemists at Jackson Lab, which they claimed was very helpful.

In the academic community, we became quite famous very quickly with our publications. There were great many references to our work, even though we only spent about two or three years on that project out of my thirty-seven years at DuPont (12).

DAEMMRICH: I can see how it generated a lot of enthusiasm among the academic chemistry crowd. How were you describing it to your colleagues at DuPont?

PARISER: That's a good question. Jackson Laboratory was a very industrially-oriented laboratory. It was, in many ways, a basic laboratory. We had research reviews that were open to everyone in the plant. We had a lot of plant managers and development engineers who attended our reviews. The unexpected thing was that our work was very well received. Those plant managers, who were real practical types, said, "This is good stuff. Keep it up. Who knows where this will take us?" The people over at Central Research heard about it, and they were a little jealous that they hadn't found it first! [laughter]

In the course of our discovery, I forged a close relationship with Howard [E.] Simmons [Jr.], who years later became head of Central Research as well as my boss. Howie was essentially a general scientist. He loved our work. He asked me to teach him about it, and I spent a good many hours doing so. He was trained as an organic chemist, but he caught on. I gave him the programs from IBM and he used them to put out a number of publications on his own (13).

DAEMMRICH: Was DuPont using computers at the time, or were you the first chemist?

PARISER: I was definitely the first. My management at DuPont asked me to give a presentation to the Accounting Department, which was not too well received. Perhaps they were concerned about their jobs.

CREMER: You presented a paper on azulene at a quantum mechanics conference in Texas (14). Would you like to talk about that conference, how it encouraged you, and how your paper was received?

PARISER: That was some conference. It was a who's who in theoretical chemistry. I would say that my paper on azulene was received extremely well.

CREMER: Had you considered going on in quantum chemistry, at that point?

PARISER: Yes. That question came up. In fact, I was approached by publishers. They wanted me to write a book. I considered it fairly seriously. In fact, I discussed it with Simmons, who might have been a co-author. Simmons thought it was a great idea. I discussed it with Schroeder, who was sort of my boss's boss, at the time. He didn't say, "Don't do it," but he sort of implied that it was not necessarily good for my career at DuPont. At the same time, he was telling me I had a good future at DuPont. I think soon thereafter I was promoted to a supervisory position at DuPont, so I guess it went to my head. Later on, I was also offered a position at two or three universities. If I had to do it over again, I'm not so sure that I would've stayed at DuPont.

DAEMMRICH: Do you think you would have enjoyed stepping into a more academic career?

PARISER: If I had one life to live over again, I'd like to give it a try, yes.

CREMER: Describe your impressions of the 701 itself.

PARISER: The 701 occupied a large room, perhaps 50 feet by 50 feet. Almost all the memory was on tape. They had several big tape reel units in that room, as well as a huge printer console. There was not much fast electronic memory there, like the RAM [random-access memory] of today. There was some fairly fast electrostatic memory, but we had those tapes running back and forth, feeding into the computer locating information on the tape automatically.

DAEMMRICH: Was it very noisy?

PARISER: No, but it was somewhat noisy.

CREMER: Were you in there by yourself or were there other people waiting to use it?

PARISER: There was a whole staff with that computer all the time. I wrote the program, which was punched out on IBM cards, and then fed into the computer. That was the way the input was done. The output was numerical data on rolls and rolls of paper.

DAEMMRICH: Was your input in binary?

PARISER: Yes.

CREMER: How long did it take to create the cards?

PARISER: I did that in the IBM class that I took. It took about two or three months to write the basic program. The IBM staff helped me by punching out the cards. I had to devise the math to diagonalize matrices. John Sheldon helped me a lot with that. I also had to create cards to enter the geometry of the molecule.

CREMER: How did you identify errors in the code? Did the computer identify them for you?

PARISER: That's a good question. Yes. There were certain warnings when the computer identified an inconsistency. I was pretty proficient in binary arithmetic, but the computer helped a lot because it had a certain self-programming ability in the sense that you could enter a certain number in cell A, and then take the number in cell B and multiply the two, and put the result in cell C. That's how you would program the computer. You didn't have to identify by specific number where A, B, and C were because the computer would do that for you. If you wanted to keep repeating it, there was a way of having it run over and over again.

When I was doing these calculations there was another class going on at IBM. They were developing what became Fortran, which was the first language beyond machine language. I never participated in that.

CREMER: I've read that DuPont management, in the 1950s, was afraid they were focusing too much on fibers and wanted to move towards other polymers, which they thought was the future. Did that affect your PPP theory work at all?

PARISER: No. I was working on PPP theory when there was a lot of concentration on fibers. That was in the early 1950s. Of course, work on other polymers was going on as well, and basic discovery work in polymers was happening at that same time. In fact, that's a good introduction because the emphasis in polymers started to pervade into Jackson Laboratory. For instance, Teflon, Neoprene and other polymers had been discovered in Jackson Lab. Polyurethanes were being developed there, as were fluoroelastomers and polyolefin elastomers. Eventually, during the late 1950s, the emphasis on elastomers increased strongly, especially at Jackson Laboratory, and decreased in terms of providing support for the fibers business. So, that sort of jives with what you were saying. But it wasn't a sudden shift.

DAEMMRICH: After PPP theory was published, how did you go about looking for your next project?

PARISER: Next, I was promoted to a supervisory position. I led four or five people in my group. We started doing polymer-related work for elastomers. All of it, as a matter of fact, was on physics and physical chemistry problems. It grew into studies of the dynamic mechanical properties of elastomers. We had several publications in that area (15). We also did work on the cell structure of polymeric foams, in addition to other projects (16).

DAEMMRICH: Were you doing compression tests?

PARISER: Yes. In fact, I was one of the inventors of a test for measuring crosslink density by way of a compression test (17). I had a bunch of very good people in that little group.

DAEMMRICH: What were your feelings about moving from lab work to a supervisory position?

PARISER: I enjoyed it, basically. I don't know how good a supervisor I was. My managers, I think, thought I was pretty good. I didn't have any problem with the people that worked for me. I think it was about then that someone in management decided that I needed to broaden my horizons. You know, "Enough of this physical and theoretical stuff, let's get him into a real 'dirty' job." So they put me in charge of the Neoprene research. Neoprene was DuPont's first elastomer. In fact, it had been discovered in Wallace [H.] Carothers' group in the early 1930s, and it was an important rubber during World War II. My Neoprene group consisted of maybe fifteen or so Ph.D.'s. At first, I was a complete neophyte in this field. In fact, the lab director said, "You know, Rudy, before you get too deeply involved in doing this, I want you to run a Neoprene polymerization." I had never in my life done anything like that. [laughter]

DAEMMRICH: How did that work out?

PARISER: We knew about Neoprene in elastomers at the time. It was around the late 1950s that the top management of DuPont decided to set up a separate department for elastomers. At that time, we were still in Jackson Lab, which was part of the Organic Chemicals Department. And in 1957, in fact, they did create the Elastomer Chemicals Department. Many of us were given the choice of staying with Organic Chemicals or going to Elastomers. I had previously done work in both areas. Herman Schroeder, who was not only my boss but also a good friend, was made the assistant research director for the Elastomers Department. That kind of influenced me to go in that direction, so I chose Elastomers.

Charlie Pedersen did the same thing, even though he had never worked on elastomers. I remember talking with him about it. We were good friends. We both opted for Elastomers, and it was shortly before then that they had put me in charge of the Neoprene group. We moved into a brand new beautiful laboratory at the Experimental Station. That happened in 1960.

DAEMMRICH: After the move, were you surrounded by a much larger group of scientists?

PARISER: Yes. There were probably five thousand people working at Experimental Station. It was bigger than it is now.

CREMER: Please discuss the Neoprene research you did after moving to Experimental Station.

PARISER: On one of the projects in Neoprene research, we were working on making Neoprene more crystallization resistant, so that it would not stiffen up at low temperatures. I thought we could achieve this by polymerizing it at a slightly higher temperature than we did normally. 2-chlorobutadiene polymerizes mostly 1,4 <u>trans</u>, and if you were to add a little bit of <u>cis</u> it would disrupt the regularity of the 1,4 <u>trans</u> structure and make it more difficult to crystallize. Also, it would create more 1,2 polymerizations instead of 1,4. So, I told management, "We'll work on this and also make it cheaper," because normally chlorobutadiene is copolymerized with another monomer; in this case, it was 2,3-dichlorobutadiene to disrupt the symmetry. The management liked the idea that it could be cheaper.

DAEMMRICH: A shocker. [laughter]

PARISER: Yes. So my work was encouraged. I learned a lesson from all this, because when we finally produced samples and brought them to our customers, they didn't like them! [laughter]

CREMER: Why? Was it just because it was different?

PARISER: Yes. Also, it would involve a lot of work on their part to make sure that it would behave properly in applications, and if it didn't, then there would be a lot of headaches. There were too many unknowns. That's a lesson I'll never forget because the same problem has come up several times. Just because you think a product is better, or cheaper, or both, doesn't mean it's a pushover by any means.

DAEMMRICH: Viton became a successful product around that time, correct?

PARISER: Yes, very much so. Viton is a fluoropolymer. As a matter of fact, I think I was promoted to Assistant Lab Director at some point [in 1967], and in the course of that I inherited supervision over Viton. I was also in charge of an exploratory group. In Viton, we ran into serious competition from the 3M Company. Specifically, the competition was over compression set. When you compress an elastomer, heat it, and then remove the pressure, it's supposed to come all the way back. If it doesn't come back all the way, that is a compression set, and that's not desirable. Viton, a fluoroelastomer, is used at very high temperatures. It is also used in the making of o-rings and seals, which are compressed in use. Even though DuPont was first, some patents had run out and 3M was getting into it. They came up with a version of the fluoroelastomer that had a considerably better compression set than the DuPont product. And although I didn't invent that product, I believe I inspired my group enough to develop a competitive product, and they did. They did terrific work. They invented a product that beat 3M with regard to compression set, and I was very proud of that accomplishment. Marge DeBrunner and Dexter Pattison deserve a lot of the credit, as well as the others in the group. It was very much a team effort.

Let me digress a moment and tell you about my Welch lecture on fluoropolymers. [Carl S.] "Speed" Marvel, of the University of Arizona, consulted for DuPont—in fact he consulted for DuPont longer than any consultant, more than thirty years—and "Speed," who was on the Welch Foundation Lecture Committee, insisted that I present a lecture on fluoropolymers. I worked very hard on preparing that lecture and received much help from our experts in the field. We presented that talk and also published a paper (18). The paper was also well received, and I heard that it was used as a "text" for a course at MIT [Massachusetts Institute of Technology].

Let me go back to my work at DuPont. In those days, I was a bit concerned by the bonus system at DuPont. There was a "B" bonus for generally good work, and an "A" bonus for a specific accomplishment, but it was all very secret. Nobody knew who got what, and we were not supposed to announce it to anyone. I felt that system was wasteful in a certain way. I saw that people could be motivated by being more public about their bonuses. So I decided to run an experiment. I announced a prize called the Pariser Prize. I offered one thousand dollars of my own money, which was quite a bit at that time, for the first person that could synthesize an all cis-polychloroprene. At that time, there was a lot going on with stereo-regular polymerization based on Ziegler-Natta catalysis, and this included the synthesis of all cis-polybutadiene. So, that was the challenge, namely, to prepare cis-polychloroprene.

Apparently, people had tried to do that before, but now there was a prize connected with it, and wow if one of my chemists didn't do it! It was not a practical process, but he did make a few grams of it by a very convoluted synthesis and we got a patent on the structure. NMR [nuclear magnetic resonance] proved the structure. It was prepared by a very complex synthesis (19). You'd never use it commercially. The good news was that our tests on that small sample were sufficient to tell us that it wouldn't be a blockbuster.

I did present the winner, Carl [A.] Aufdermarsh [Jr.], with his prize of one thousand dollars. It got a lot of publicity within DuPont, and I was even invited to lunch at the Hotel du Pont with two or three DuPont vice presidents from the Executive Committee. They were nice and polite and asked me why I had done this. I told them, "I think you ought to have a bonus that is more public within the company and I was trying to prove a point." They listened attentively and complimented me on my initiative but did not really adopt my recommendation. They did give me a bonus to cover the one thousand dollars, plus quite a bit more.

I should mention that although I had become a manager in Neoprene research, I had not completely forsaken theoretical work. One paper that gave me considerable satisfaction was in collaboration with [H.] Karl Frensdorff on treating copolymerization as a Markov chain (20), which was very well received in academic circles as polymer science was gaining more prominence in universities.

DAEMMRICH: As research manager, how else did you encourage people to work a little harder to get the products through?

PARISER: One of my most exciting periods in Elastomers was when I was promoted to director of Exploratory Research. My group was extremely successful in discovering new stuff. In addition to the lower compression set Viton that I've already mentioned, the seeds were planted for three new ventures that today are very profitable for DuPont. First, there was Hytrel, which is a polyether ester product designed to compete with thermoplastic polyurethanes (21). There was Vamac, which is an ethylene methyl acrylate copolymer. And then there was Kalrez, which is a completely fluorinated elastomer, somewhat like elastomeric Teflon. I'm proud to say that we carried all three to commercialization.

Vamac is an ethylene methyl acrylic copolymer. Herman should get at least some of the credit because he was sitting in my office and he was telling me, "There are people in polymer products"—which is where they made polyethylene—"who've got these plants sitting there with much extra capacity. Do something." I thought, "That's a challenge."

DAEMMRICH: He's saying, "Put the plant to work."

PARISER: Yes. Now, I had some very good chemists in my group. We started talking about things you can copolymerize with ethylene. At the same time, there was concern about Neoprene, which is highly chlorinated. It is quite flame resistant but it does emit toxic products in fires. One idea for eliminating that toxicity involved making an oil-resistant elastomer, like Neoprene, without chlorine in it. So, the two ideas kind of merged, and we thought, "Why don't we copolymerize ethylene with a polar monomer?" Methyl acrylene is a natural one to think of, and it can be done in high-pressure copolymerization with ethylene. They had that kind of equipment at the Sabine River where they had an underused plant. So that's how we got started. I had a lot of terrific help bringing that one along. The funny thing is we got a lot of guff from those plant people when we finally had the product! [laughter]

DAEMMRICH: Then they suddenly had a use for the plant.

PARISER: Exactly. I understand they're very happy to have it today; it's a good moneymaker. Ironically, Neoprene is also very flame resistant, but no one expected the new polymer we created would be fire resistant. Apparently, they've learned how to compound it with carbon black, and other things, which has made it extremely fire resistant.

Hytrel was brought on to compete with thermoplastic polyurethanes, which, in a way, got started in Jackson Lab many years earlier but were never pursued. The competition, especially Bayer [AG] in Germany, was making great progress with these polyurethanes and developing attractive markets in that area. So our idea was, "Let's do something to compete in that market." I had a very bright chemist in my group, Bill [William K.] Witsiepe, who was also incidentally instrumental in the development of ethylene methylacrylate polymer. He and I had a little *tête-à-tête* I remember, about that polyurethane challenge. Almost everything we could think of, other than polyurethane itself, would require a polymerization in solution, which we knew would be too expensive to compete. So, I told Bill, "Think of something that you can do in the melt." I guess that got him to thinking, and he knew enough about the DuPont patent literature, especially the work on Lycra, to take off from there. Using processes similar to those used in Lycra, he created the polyether ester polymer that is called Hytrel today.

We had a sales development group at that time. They were in Sales and they would come to us with what they called "new product requirements." They would tell us, "This is what we want," and then we would present them with a candidate and most of the time it would go nowhere. A thermoplastic polyurethane, however, was not on their list of new product requirements, and so they gave that idea a bit of a cold shoulder. I don't remember what their reasons were, but they had their own reasons. I remember complaining to Herman Schroeder about that, saying, "I wish we could have our own salespeople." And he said, "I'll see what I can do." [laughter]

I guess he talked with the director of sales and they decided to put a couple of sales guys in with my group. Of course, we suspected that they had probably picked the people that were on the bottom of their list. As it turned out, those people had the characteristics we wanted because they were quite creative, and I now think they knew that when they picked them. Any way, we got a couple of very motivated sales development people and they started looking for potential markets for Hytrel.

At the same time, we were also looking for a way to make larger quantities. Hytrel didn't fit into any of the existing new processes, except Lycra—but those facilities were busy. They wouldn't let us in there. Finally, we found a place in Japan—I can't remember the company's name, it may have been Mitsui—we made a deal with them and together we started making test quantities in Japan. We developed everything within our exploratory research group—the initial sales development, the initial pilot-plant production, and so on.

Tom [Thomas M.] Connelly, Jr. was one of the young engineers on Hytrel back in 1975. Currently, as I tell this oral history, he is Senior Vice President, and Chief Science and Technology Officer at DuPont. He and I used to meet quite frequently when he was first appointed to his current position to discuss research organization. I told him, "One of the problems with Central Research is that they're setup like a head without a body. They don't have a sales or manufacturing capability. You remember why the Hytrel development worked, don't you? We got a couple of sales people, and we got engineers like you, and we got it all going within Research." I could see a light going on in his mind. Today, they have sales support within Central Research.

DAEMMRICH: Is this the same thing as heavyweight teams where you build a whole group around a molecule? Pharmaceutical companies do that sometimes, and it seems like you were setting that in motion back then.

PARISER: That's right. For example, Kalrez was not initiated in my group. It had been dribbling along, not going anywhere, before they moved it into my group. First we addressed the process, and here, Dr. Ali Khan in my group did fine work in defining a reproducible, practical process. We then applied techniques similar to those that we used for Hytrel. We used the sales guys we had already, and we also had a bunch of engineers—who were very inspired by their previous successes—look at it and move it forward. Today, it is another big success.

One of the biggest customers of Kalrez, and probably still is, was the Halliburton Company. They use it for oil exploration, mainly for all kinds of seals. Kalrez withstands very high temperatures and very toxic environments. Nothing comes close to it.

In any case, our customers were leery of Kalrez, initially. They weren't sure of the market, and the product was extremely expensive, so it was not their kind of thing. As a result, we suggested, "Why don't we make some of the parts ourselves?" Of course, there was a big howl from the sales people. They said, "Compete with our own customers? How can you do that?" However, Herman Schroeder, our Departmental Research Director, pushed for us.

We had a very courageous General Manager in charge of the whole Elastomers Department, Charlie [Charles J.] Harrington. I remember that he was distantly related to the DuPont family. He said, "Do it." We had to go all the way up to him for that decision. So we did. We started making Kalrez parts in my group. We took over a facility in Germay Park, in Wilmington, and started manufacturing Kalrez parts. DuPont still does that to this day.

DAEMMRICH: Describe how you would do annual performance reviews and evaluate people in the research group.

PARISER: We had a salary system that was supposed to reward people. Actually, I did a little bit of a Pariser-Parr on that one. The DuPont Company classified people in different levels; for example, level 5, 5-A, 6, 6-A, and so on. An employee was paid according to his or her level.

DAEMMRICH: Did the levels go from CEO [chief executive officer] down to the lowest clerk?

PARISER: Yes. The CEO I believe was at about level sixteen. The ironic thing was that even though they said they had that system, I don't think it really worked that way. As a result, I decided to plot the career raises of several randomly selected people against time, and then see when they moved from one level to another, and I marked that on that same chart. From that, I discovered that a person's raises were not necessarily related to his promotion to a higher level, but rather, to the rating he was given for his performance. That is, he got paid more if supervision rated him higher relative to others in the group. Other things determined the promotion to the level, such as how long he had been at the current level. And so, by studying what was actually happening, I proposed a modified salary system. I also proposed that a person's reward should be based on his current pay and the integral of his pay up to that point.

I presented that idea to my management. They thought enough of it that they assigned a computer expert to work with me to see how we could develop that system. It was fairly complex but the concept was quite simple. In the end, what really determined an employee's reward was his or her performance and not necessarily his or her level. We developed a salary system based on that, and then the DuPont hierarchy came in and asked, "Where are our levels? We've got to have our levels." So we put their levels in as well. In the new system, an employee could go from level 5 to 5-A in one month, as opposed to two years, provided his rating justified it. I believe that they may still be using that system today.
.

DAEMMRICH: In the early 1970s, you spent two years as Manager of Market Research and Market Development. Was that physically outside of Research?

PARISER: Yes.

DAEMMRICH: Why did you move over into that position?

PARISER: I guess they wanted to broaden my experiences.

DAEMMRICH: Tell us about your experiences in the Marketing Department.

PARISER: It wasn't always pleasant. The salespeople wanted to know, "What is this research guy doing supervising us?" I don't think they paid much attention to me, at least not at first. In any case, I did learn a lot and in retrospect it was very good for me.

DAEMMRICH: What did you do to establish your authority over them?

PARISER: In research, I didn't have to do anything to establish my authority. It seemed like people just respected me and that was the end of it. This was different. Still, they were nice to me. They tried to educate me. They did whatever they wanted, but they "reported" to me.

CREMER: How did it shape your view of the company?

PARISER: If anything, it had a positive impact because I realized there were a lot of smart people outside of research. I began to appreciate how difficult it was to sell something and develop a market for a product. While I was in research, I thought, naively, "All we have to do is make a discovery, get a patent, and 90 percent of the work is done." I learned that that is not at all the case.

DAEMMRICH: Did you go out on any sales calls?

PARISER: Yes. They took me along, perhaps a little reluctantly. [laughter] What bothered me, at the time, was the research program I had left seemed to have become stagnant while I was away. So, after a couple of years I was transferred back to my previous research job.

DAEMMRICH: Did you get them back in gear?

PARISER: Yes. In fact, we commercialized most of our projects after I got back. But my stint in Marketing was good because it allowed me to establish some very useful contacts over in the Marketing Division. When I went back to them after had I returned to Research, they didn't just throw me out, as they might have in the past. They listened to me because I was also "one of them." It was a very good experience, and I was glad it had happened.

CREMER: Let's move back for a second. In 1966, DuPont's stock briefly collapsed and the company lost 4.6 billion dollars worth of market value. Did that affect your division at all?

PARISER: Let me put it to you this way: DuPont was highly departmentalized in the 1960s, and each department—Elastomers, Polymer Products, Fibers, et cetera—was almost like a separate company. Within Elastomers, for example, we didn't get general information about how business was going—we were aware of our own department only. I don't remember a stock collapse specifically, but I do remember that Fibers was getting into some business trouble because they were trying to reduce their research force at the Experimental Station. Elastomers, however, was not in trouble. We were growing in the 1960s. Of course, we were only a few years old at the time. I remember picking up some of the research people that were let go out of Fibers. Most of them did very well after they came to us.

DAEMMRICH: How did the oil crisis of the 1970s affect the company?

PARISER: I do remember the impact on Neoprene, which was linked to the Vamac situation. I remember giving a speech to some of the DuPont management, in which I said we needed to create a less expensive substitute for Neoprene and polychloroprene because they were too energy intensive. That gave a boost to the Vamac project.

CREMER: The Elastomer Chemicals Department merged into the Polymer Products Department, and you were put in charge of Exploratory Research for that department in 1980. Would you tell us how that merger happened and how it was structured?

PARISER: The Polymer Products Department was considerably larger than Elastomers—several times as large. I don't know what prompted them to merge the two departments, but it might have been partially because Charlie Harrington, our VP [vice president] and General Manager, was retiring. I became a Research Director in charge of Pioneering Research in the merged department. As a result, my group increased in size, and a couple of divisions from Polymer Products were combined with my pioneering group from Elastomers.

Like any merger, a lot of rough spots had to be ironed out. We were strangers to the people over there, and they reported to a "new guy," so there was a good deal of uneasiness. I think the pioneering spirit that had prevailed in the Elastomers Department was diminished considerably, and I was not especially happy with that situation at the time. My friend Howie Simmons, who had made big advances over at Central Research and became their VP in charge of the Department, sensed my unhappiness. He said, "Let me get you over to Central Research." Then, when there was an opening in one of the director positions, he approached me with that, and I said, "Good." [laughter]

CREMER: Was that an even larger group?

PARISER: At first it was smaller, but eventually it became larger.

CREMER: How did you cope with that responsibility?

PARISER: It was very interesting because Central Research is a terrifically strong scientific organization. There were a lot of excellent scientists there, and they didn't readily accept transfers from an operating department. However, they accepted me, probably because of PPP theory. I think every one of their directors was a member of the National Academy [of Sciences], which should give you a flavor for the organization. On the one hand, it was great to be in very friendly surroundings and have so many talented people. On the other hand, I really missed the input from the sales and manufacturing arms of an operating department. I felt it was somewhat like a very smart head without a body. I was somewhat frustrated in that regard almost the entire time I spent in Central Research, although there was improvement in that regard towards the end of my employment.

CREMER: What kind of products did you develop in Central Research?

PARISER: We worked on group-transfer polymerization or GTP. That project had been initiated before I arrived, and I enjoyed helping to bring it along. One of the people I replaced, at least in part, was Burt Anderson, who had moved from Central Research into the Fabrics and Finishes Department as the Assistant R&D Director for that department. Group-transfer polymerization got started in his group while he was in Central Research.

I knew Burt from way back when we were in Elastomers. He was one of my supervisors and was also involved in the Vamac, ethylene methylacrylate development, which I described earlier. We have a close friendship to this day. In fact, I'm going to have lunch with him next week.

Because of Burt's high position in Fabrics and Finishes, he was very helpful in bringing the group-transfer polymerization project along. It was eventually commercialized in the Fabrics and Finishes Department. Today, it is a very successful product. It produces polymers that are used to stabilize the inks in inkjet printers. Hewlett-Packard [Development Company] was one of our biggest customers in that area.

Of course, while I was in Central Research, Charlie Pederson won the Nobel Prize in 1987 for work that he had done more than twenty years earlier.

CREMER: Please discuss your relationship with him.

PARISER: Charlie had already retired by that time. I knew him from way back in Jackson Laboratory. He was an excellent chemist, but he never got a Ph.D. He was a very "industrial" chemist. I already mentioned that he chose to work in Elastomers because of Herman Schroeder, who was also his good friend, and perhaps I had influenced him also. Charlie was a

Research Associate, which was an elevated title for a researcher. He "reported" to me while we were in Elastomers, meaning he essentially did his own thing.

He discovered the crown ethers, for which he won the Nobel Prize. By way of background, when Charlie moved to Elastomers in 1957, he became a member of my Exploratory Division.

When Charlie first told me about these new crown compounds, as he had named them, both he and I recognized that it was an extraordinary find. I remember telling Charlie, "Write this up. Publish it." He hadn't written a paper since coming to DuPont because he was a real industrial chemist. His objective was to get patents, not papers. He promptly filed for patents on his crown ether work, and it was a good time to publish that work. However, it took what seemed like a year to convince him. He had said, "I don't know how to write a paper." I replied, "Look, I will help you. I'll guide you through it." I had just published PPP theory not long before that, as well as a bunch of other papers in various fields (22). Charlie went ahead and wrote two monumental papers, and I helped him somewhat to write them, edit them, and so forth. He said, "I'm going to put your name on there as a co-author." I said, "No, you're not. This is your work. You can write a little blip at the end and cite me for my help if you wish."

CREMER: That was the Nobel Prize winning work, correct?

PARISER: Yes. I could have been a co-author. [laughter]

DAEMMRICH: Modesty can really hurt you sometimes.

PARISER: Charlie felt very sheepish about his work on the crown ethers. The whole thing came about because of a project in my group, as a matter of fact. Charlie had worked on stabilizing gasoline by creating chelating copper. Copper was a detrimental catalyst in the degradation of gasoline, so he'd try to chelate the copper in order to stabilize the gasoline, work that he had done earlier in Jackson Lab.

When Charlie started to report to me in Elastomers, we discussed a similar problem. When you polymerize ethylene propylene copolymers the native vanadium catalyst left in the polymer acts as a catalyst for its degradation. This seemed similar to copper in gasoline. He said, "Maybe I can invent a chelate that will chelate the vanadium." As he worked on that, he accidentally came across the cyclic crown ethers. He discovered how they did not chelate vanadium, but they did trap lithium, sodium, potassium, and cesium. He discovered that bigger crown rings trapped bigger alkali ions; that is, a small crown ether ring would be efficient for complexing a small ion—for example, Li^+—however, a larger crown ring would work best with a larger ion, such as Cs^+. It was the first direct demonstration of molecular recognition, which is what was cited in the Nobel Prize. Of course, a lot of academics jumped on that, and I'm certain it's because of them that he got the Nobel Prize. If it hadn't been for Jean-Marie Lehn and Donald J. Cram, co-winners of that Nobel Prize, who did a lot of the development and follow up work, the Nobel committee probably wouldn't have awarded it.

Incidentally, I did write up the histories of the discoveries of the crown ethers, as well as of PPP and group transfer polymerization, in a paper entitled, "Creativity at the Crossroads between Science and Technology (23)." It emphasized the role that industrial motivation can play in inspiring advances in basic science. This paper is based on a talk that I had given at the Chemical Heritage Foundation in 1996 as a part of their "Conversazione" series of lectures.

DAEMMRICH: There was a time in the 1980s when some companies began questioning the value of central research divisions. Did you have to fight some battles to keep Central Research the way it was?

PARISER: Yes. Simmons wanted to keep it as "Purity Hall." I think Central Research did serve a number of very useful purposes outside the realm of doing some fine research for the company. In fact, I wrote a paper on the subject (24). Not very many things that we did in my group became commercial, except for group-transfer polymerization and high temperature superconductivity. Uma Chowdhry, who was a Manager reporting to me at the time and is now a VP heading Central Research, was then in charge of the superconductivity project.

Anyway, the "Purity Hall" concept, in my opinion, had some great benefits for the company. It gave us the opportunity to hire very talented and smart people, many of whom advanced to very high levels in DuPont. It also helped to maintain valuable contacts with research being done in universities, and in general, it enhanced the reputation of DuPont as a forward-looking company. Central Research is uniquely positioned to fulfill these objectives.

DAEMMRICH: Let's discuss high temperature superconductors. Was that DuPont's first venture into materials for the electronics industry?

PARISER: No. DuPont has a long history of electronic materials with a great variety of products and applications—photo resists, for example, which I believe were developed in the Photo Products Department many years ago. Related technology is a big winner today for use in producing large, flat televisions screens. Central Research, of course, made their big hit with Carothers and the discovery of Nylon. The Carothers success is what influenced Central Research for the next fifty years. One might say that his discoveries have contributed strongly to making DuPont into the large company that it is today.

DAEMMRICH: Would you talk about some of the people you mentored over the years?

PARISER: One of them is Gerry [Gerald M.] Estes, an excellent engineer in my group in Elastomers. He's now Vice President at the LORD Corporation. He lives in Chapel Hill and is still a good friend of mine. He and I were in the Market Development group at the same time. I was the Manager and he had been sent there for "broadening" from my research group.

There is also Jim [James M.] Meyer. I hired him, and eventually he became VP of Central Research. So did Clay Smith, a black chemist, who also became a VP in Central Research. [F. M.] Ross Armbrecht [Jr.], whom I had hired in Elastomers, held many leadership positions after leaving DuPont, including President of the Industrial Research Institute. There is Uma Chowdhry, who is currently Vice President overseeing Central Research. I didn't hire Uma, but she came into my group at Central Research very shortly after she was hired. Steve Freilich is currently the Director of Advanced Materials Research, which is a position I had held at Central Research. I hired him. Hal Snyder is currently director of fluorochemicals research. I hired him. There is Jim [James A.] Trainham, who is now the CTO [chief technical officer] at PPG [Industries]. And, of course, there is Tom Connelly, hired in 1975, the current Chief Science and Technology Officer and Senior VP for DuPont. There are quite a number of others as well who have reached high positions, such as Lou Manring at DuPont and Bill Hsu, who recently left DuPont for a high position elsewhere.

DAEMMRICH: Let me ask you a broader question then. When you think about how you managed them, did you have an overall guiding philosophy or did you deal with them individually?

PARISER: I think a number of them came to work for the Elastomers Department because I was somewhat famous. Connelly said so. He's an engineer, so that's pretty far removed from quantum chemistry. I was not in Central Research, but in an Industrial operating Department. Now, a lot of those people I hired probably could not have got into Central Research. It was too "pure." Central Research essentially didn't hire engineers, for example. So, I believe that I portrayed an image to those young people that was different from that of the "organization man," or of the "pure scientist," and they liked that. Of course, I also hired some excellent people while I was in Central Research.

DAEMMRICH: So, you weren't looking for kind of rule-based behavior?

PARISER: Exactly.

DAEMMRICH: How did you direct what people worked on in your department?

PARISER: I am not a dictator, and I don't believe that I have ever dictated anything to anybody—maybe that's why I didn't do so well in Officer Candidate School. [laughter] I think my people in research appreciated that. We always reached an understanding as to what should be done, and I had a philosophy of letting people boss themselves. I can provide a certain degree of guidance and express some opinions.

I'll tell you a little story Ross Armbrecht told me. As I have mentioned, until recently he was President of the Industrial Research Institute. He retired from that and now he's the Executive Director of the Delaware Foundation for Science and Mathematics Education. The other day we were having a conversation, and he said, "I'll never forget why I came to work for you at DuPont. I had an offer from Eastman Kodak [Company], with a bonus that was considerably larger than what you people were offering me." He told me how large the bonus was, and I said, "Listen, I would never have taken an offer like that because my bonus today is a lot bigger than that. Our bonus system is not based on some percentage of your salary, which is apparently what that was, but it's based on how well you perform. And, if you perform well then you'll get a lot more than that." That's all it took to convince Ross.

DAEMMRICH: You talked a little about how you rewarded performance, but how did you evaluate it?

PARISER: We would have meetings with all my supervisory staff. We sat in a big group for most of the day, two or three times a year, and we would discuss each person. The first level of supervision would say something about him, then the next level, and then the other people to whom this particular person might report would say something. Then we would reach a consensus based on performance and potential, and we'd discuss what should be done to help that person grow in the company. All of that would determine the person's salary.

DAEMMRICH: Would you mind discussing some of your work on the NRC [National Research Council]?

PARISER: I thought I'd bring you one of the reports. The best thing I did was "Polymer Science and Engineering: Challenges, Needs, and Opportunities" (25). This was first report by the NRC in the polymer field. If you look at the people in the group, it's sort of a who's who in polymer chemistry at the time. Charles [G.] Overberger was my co-chair.

I'll tell you about my first experience at NRC. I was a bit overwhelmed, because I was told, "You've got a year in which to do your project." I had more than a full-time job at DuPont at the same time, but we managed to do it, as you can see. We met in Washington quite often, and got wonderful support from the NRC and their staff. We had a lot of good people on the Committee, and each person received one section to do, in their own field, so it wasn't too painful. We put this report together and it was the first time that people had really addressed polymers at that level, with the future in mind. For example, we forecast the emergence and advances of biopolymers quite well.

One of my other experiences was on the *ad hoc* on the DOE [Department of Energy] chemistry research panel, but I did not chair it (26). Alan Schriesheim chaired that one.

DAEMMRICH: You were looking for basic research related to energy sources, correct?

PARISER: Yes. It was a somewhat diffused project and, to tell you the truth, I never fully understood what we were trying to do, but we did write a report.

DAEMMRICH: Was [Edel] Wasserman on that Committee?

PARISER: Yes, he was. Another NRC project where I participated dealt with Halon fire suppression in the [United States] Navy. That one's much more recent, around 1998. That was interesting also because we got to visit a lot of warships. They took us around modern carriers, battleships, and destroyers, and we got lectures and demonstrations by the Navy on their fire suppression systems. I'm not sure we contributed much, because they pretty much knew what they were doing. [laughter] I think I learned a lot more from them than they learned from me. It was very educational. We traveled to San Diego, Norfolk, and other harbors.

DAEMMRICH: Would you mind discussing your involvement with Chemistry Applied to World Needs, or CHEMRAWN? Do you know who started it?

PARISER: Charlie Overberger and Bryant [W.] Rossiter started it. Bryant is a good friend of mine. He was the Research Director for Eastman Kodak at the time. He's retired now and lives in California. Charlie and then Bryant started CHEMRAWN back in about 1970. Their first CHEMRAWN conference was on energy and it was held in Toronto, Canada. It came right at the time of the energy crisis in the early 1970s. They got Irving [S.] Shapiro, then CEO at DuPont, to be one of the keynote speakers at that conference, namely CHEMRAWN I.

There was an opening on the CHEMRAWN Committee soon after they started it and Bryant asked me to fill it. I became very interested and ultimately served on the Committee for ten years. I was involved heavily in organizing two of the later conferences. Other than CHEMRAWN I and CHEMRAN II, which addressed world food supplies, those two conferences are distinguished as being among the most successful to date, especially because of their strong ongoing future actions programs. The topics were "Chemistry of the Atmosphere," for CHEMRAN VII, and "Green Chemistry," for CHEMRAWN XIV (27).

DAEMMRICH: Tell us a little more about what makes CHEMRAWN successful.

PARISER: CHEMRAWN stands for Chemical Research Applied to World Needs, and thus it addresses topics of considerable interest. I was very interested in the future actions programs, a fairly unique feature of CHEMRAWN conferences, and I believe that was where I made my largest contribution. As well as in organizing and fundraising for these two conferences, CHEMRAWN VII and XIV, both had considerable funds that we could use for future actions. We still have a very active Future Actions Committee for each of those conferences. The future actions program is based on what people at the conference thought should be done in that particular field. We continue to work to implement those recommendations, especially through the Green Chemistry Institute, which is being directed by Dr. Paul Anastas, the Future Actions Committee chair for CHEMRAWN XIV.

In previous future actions, I had collaborated with John [W.] Birks from the University of Colorado, who was Future Actions Committee chair for CHEMRAWN VII. The future actions programs from these two conferences have significant overlap.

DAEMMRICH: Let me take a different track with a couple of questions. When did you meet Louise?

PARISER: Louise was a chemist at DuPont. She was working in our Technical Services Laboratory, in Chestnut Laboratory, in Elastomers. I didn't know her. Then, in the spring of 1970 there was a National American Chemical Society meeting in Washington, D.C. and I met her there. The NACS used to have more social functions than they do now. They had a dinner and dance and there was a DuPont table. I was not at that particular one, but she was at it and I saw her, this pretty girl, sitting next to a man I knew. I walked over there and he introduced me to her. We danced, and as they say, "The rest is history." We were married in 1972. So, that's how I met Louise.

Louise was brought up in Wilson, North Carolina, living a good part of her youth in her grandfather's elegant, large home, with domestic help. Much emphasis was placed on education, health, good manners, and social behavior. Perhaps because of this background, even though Louise and I came from opposite sides of our globe, we have much in common regarding our values and objectives. That is probably why we get along so very well!

Louise graduated in chemistry with highest honors from Salem College in Winston Salem, North Carolina, one of the oldest women's colleges in the United States. She was interviewed on campus by Dr. Russel Herbst of DuPont and immediately offered a position at the Elastomers Technical Service Laboratory at Chestnut Run. She became an expert in product toxicity issues. After some fifteen years in Technical Services where she provided support for various products, including Hytrel, she was promoted to Sales Representative covering New

Jersey and New York. I remember her many days and nights on the road calling on customers. When a lucrative early retirement opportunity presented itself coupled with an attractive offer from a real estate company Louise decided to take it, having spent nearly twenty years with DuPont. Her offer was from Patterson Schwartz [Real Estate], then Delaware's largest real estate company.

While at DuPont, Louise became interested in real estate and had taken the time to obtain a Delaware realtor's license, which she later expanded to include Maryland and Pennsylvania. The position she accepted at Patterson Schwartz was as Director of Relocation, where she headed up their Relocation Center. She had the unique distinction of being the first person in Delaware to obtain the designation of "Certified Relocation Professional," conveyed by the Employee Relocation Council, Washington, D.C.

Since I had already retired form DuPont, and Louise had spent some eight years as Relocation Director, she opted to seek an occupation that provided more freedom in regard to her time, and she became a highly respected realtor. Louise also became very prominent in the social scene; she served twice as President of the Delaware Opera Guild, she served on the Board of the Professional Theater Training Program of the University of Delaware, and she is currently President of her Salem Alumnae Class, among various other activities.

Skiing has been a favorite sport for me and for Louise; together we spent many of our vacations doing just that for more than thirty years. Other sports have included tennis, and we still are trying to learn how to play a decent game of golf. Among favorite hobbies, I should mention photography and gardening, and a high priority should be given to ballroom dancing—after some twenty years we are still learning how to do the rumba, waltz, fox trot, and quick step acceptably well.

As to other past times, I should especially mention the "Interlocutors." This is a Philadelphia discussion group comprising of about twenty members. Two friends, a surgeon, Walter F. Ballinger, and a banker, James F. Bodine, founded it in 1958. They were concerned about becoming isolated within their respective professions and started the "Interlocutors" as an antidote to cultural and vocational insularity. A large variety of professions continue to be represented, and have included, for example, [E.] Digby Baltzell—renowned sociologist and coiner of the term "WASP" [White Anglo-Saxon Protestant], and Larry Day, famous contemporary painter. Guest speakers have included Senator Arlen Specter and Mayor and now Governor Ed [Edward G.] Rendell. I joined the "Interlocutors" in about 1961 representing "science." We meet monthly for dinner, currently at Bryn Mawr College; Louise has been a frequent "Interlocutor" as well. This experience has greatly enriched our lives.

DAEMMRICH: That is indeed very interesting about Louise, whom we also got to know and appreciate at CHF. She has been much help to us well.

Now, let's get back to your work at DuPont. What do you think your legacy is within the company?

PARISER: DuPont presented me with me the Lavoisier Medal in 2003, which is DuPont's highest technical award. I was cited for my work in quantum chemistry, my leadership in the development of Hytrel, Vamac, and Kalrez, and for being a mentor to DuPont's leading scientists, like Charles Pedersen and many of DuPont managers. I do think that the PPP theory helped DuPont's reputation, especially in academia. It showed that such work could be done at DuPont, even outside of Central Research.

Also, I wonder if the products which were mentioned in the Lavoisier citation would have been created and when they were created without my leadership, such as the highly improved compression set of Viton types, the discovery and development Hytrel, and Vamac. I felt that Kalrez had been "dying," and so maybe I made a difference in helping to revive it and bring it to commercial fruition. In Central Research, I might have been the first person who came in at a management level from an operating department, and perhaps I brought a more market-oriented thinking into Central Research. Of course I had some very good friends there, like Howard Simmons, and so on, who let me do my own thing in that regard, even though they may have not necessarily agreed with me on everything. Perhaps, my greatest compliment after my retirement has come from people who had worked with me when they said that they were missing me.

I feel that I should say a few words about my activities after leaving DuPont some fifteen years ago. Louise and I formed R. Pariser & Co., Inc., a consulting company in 1990. I consulted for DuPont research, and some others, and Louise continued her real estate activities through the company. I was also active with the then Dean of the College of Engineering, Stuart Cooper, in initiating an Outreach Program aimed at continuing education primarily for industrial engineers. That was around 1990, soon after I retired from DuPont. I continued to consult for that program until recently. That program is still going strong today.

I have also continued serving on various university advisory boards for chemistry and chemical engineering. These have included the University of Pennsylvania, Lehigh University, University of Wisconsin, University of Florida, MIT, and North Carolina State University, among some others. Currently I am quite active for the Chemistry Department at the University of North Carolina, Chapel Hill, as well as their newly formed Institute for Advanced Materials, Nanoscience and Technology.

And, as already mentioned, I am enjoying my activities at the Chemical Heritage Foundation, a truly wonderful enterprise that I have followed pretty much from its beginning days until now, under the exemplary leadership of Arnold Thackray, the CHF President. It has been especially gratifying to participate in the leadership of the Joseph Priestley Society, as well as the Robert Boyle Society and the Board of Overseers.

DAEMMRICH: I think that's a great way to close. Thank you very much for your time.

PARISER: Thank you.

[END OF INTERVIEW]

NOTES

1. Frank H. MacDougall, *Thermodynamics and Chemistry* (New York: J. Wiley Publishers, 1926).

2. Rudolph Pariser and Robert L. Livingston, "The Chlorophyll-Sensitized Photo-Oxidation of Phenylhydrazine by Methyl Red. II Reactivity of the Several Forms of Methyl Red," *J. Am. Chem. Soc.* 70 (1948): 1510; "The Pheophytin-Sensitized Photoreduction of p-Dimethylaminoazobenzene by Ascorbic Acid," *J. Am. Chem. Soc.* 78 (1956): 2944; "Some Photochemical Oxidation Reduction Reactions Sensitized by Chlorophyll a and by Pheophytin a," *J. Am. Chem. Soc.* 78 (1956): 2948.

3. See for more information: P. J. Wingate, "The Colorful Du Pont Company," (Wilmington: Serendipity Press, Wilmington, 1982); D. A. Hounshell and J. K. Smith, "Science and Corporate Strategy," (New York: University of Cambridge Press, 1988): 373.

4. Melvin Calvin and H. Ward Alter, "Substituted Stilbenes. I. Absorption Spectra," *J. Chem. Phys.* 19 (1951): 765-767.

5. H. Eyring, J. Walker, and G. E. Kimball, *Quantum Chemistry* (New York: J. Wiley Publishers, 1944).

6. Robert G. Parr and Bryce L. Crawford, "Molecular Orbital Calculations of Vibrational Force Constants. I. Ethylene," (University of Minnesota, 1947) *J. Chem. Phys.* 16 (1948): 526-32; R.G. Parr, D. P. Craig, and R. G. Ross, "Molecular Orbital Calculations of the Lower Excited Electronic Levels of Benzene Configuration Interaction Included," *J. Chem. Phys.* 18 (1950): 1561.

7. See Note 6.

8. See the Chemical Heritage Foundation oral history research file #0320.

9. Rudolph Pariser and Robert G. Parr, "A Semi-Empirical Theory of the Electronic Spectra and Electronic Structure of Complex Unsaturated Molecules. I," *J. Chem. Phys.* 21 (1953): 466-71; "A Semi-Empirical Theory of the Electronic Spectra and Electronic Structure of Complex Unsaturated Molecules. I," *J. Chem. Phys.* 21 (1953): 767-76; Rudolph Pariser, "An Improvement in the π-Electron Approximation in LCAO MO Theory," *J. Chem. Phys.* 21 (1953): 568-69; Rudolph Pariser and Robert G. Parr, "On the Electronic Structure and Electronic Spectra of Ethylene-Like Molecules," *J. Chem. Phys.* 23 (1955): 711-25; Rudolph Pariser, "Theory of the Electronic Spectra and Structure of the Polyacenes and of Alternant Hydrocarbons," 24 (1956): 250-68; "Electronic Spectrum and Structure of Azulene," *J. Chem. Phys.* 25 (1956): 1112-6.

10. Robert G. Parr and Rudolph Pariser, "On the Electronic Structure and Electronic Spectra of Ethylene-Like Molecules," *J. Chem. Phys.* 23 (1955): 711.

11. Rudolph Pariser, "Theory of the Electronic Spectra and Structure of the Polyacenes and of Alternate Hydrocarbons," *J. Chem. Phys.* 24 (1956): 250.

12. See Note 9.

13. For example: Howard Simmons, "Pariser-Parr theory: Quantum mechanical integrals from the benzene spectrum," *J. Chem. Phys.* 40 (1964): 3554-62; H. Simmons and J. K. Williams, "An empirical model for non-bonded H-H repulsion energies in hydrocarbons," *J. Am. Chem. Soc.* 86 (1864): 3222-6.

14. Rudolph Pariser, "Electronic Spectrum and Structure of Azulene," *J. Chem. Phys.* 25 (1956): 1112.

15. R. Pariser, S. F. Kurath, and E. Passaglia, "The Dynamic Mechanical Properties of Polyhexene-1," *J. App. Phys.* 28 (1957): 499; "The Dynamic Mechanical Properties of 'Hypalon'-20 Synthetic Rubber at Small Strains," *J. App. Polym. Sci.* 1 (1959): 150-157; R. Pariser and T. P. Yin, "Dynamic Mechanical Properties of Neoprene Type W," (Contr. No. 114) *J. Appl. Polym. Sci.* 7 (1963): 667-73; "Dynamic Mechanical Properties of Several Elastomers and the Potentialities in Vibration Control Applications," *J. App. Polym. Sci.* 8 (1964): 2427.

16. R. Pariser and W. J. Remington, "A New Apparatus for Determining the Cell Structure of Cellulose Materials," *Rubber World* 138 (1958): 261.

17. R. Pariser, E. F. Cluff, and E. K. Gladding, "A New Method for Measuring the Degree of Crosslinking in Elastomers," *J. Polym. Sci.* XLV (1960): 341; "Neuere Ergebnisse zur Elastomeren-Vernetzung," *Kunststoffe* 50 (1960): 623.

18. R. Pariser, D.C. England, R. E. Uschold, and H. Starkweather, "Fluoropolymers: Their Development and Performance," Proceedings of the Robert A. Welch Foundation Conference on Chem. Res. XXVI (1983).

19. R. Pariser and C. A. Aufdermarsh, Jr., "Cis-Polychloroprene," *J. Polym. Sci*. Part A. 2 (1964): 4727-4733.

20. R. Pariser, H. K. Frensdorff, "Copolymerization as a Markov Chain," *J. Chem. Phys.* 39 (1963): 2303.

21. Rudolph Pariser, "Improved Process for Manufacture of Thermoplastic Polyurethane Elastomers," U.S. Patent # 3,385,833. Issued 28 May 1968.

22. See Note 9.

23. Rudolph Pariser, “Creativity at the Crossroads Between Science and Technology,” *CHEMTECH*, 28 (1998): 48.

24. Rudolph Pariser, “Industrial Corporate Research: Perspectives on Innovation,” *Innovation at the Crossroads Between Science and Technology*, (Haifa, Israel: Neaman Press, 1989): 185-197.

25. Rudolph Pariser, et al., “Polymer Science and Engineering: Challenges, Needs, and Opportunities,” (Washington D.C.: National Academy Press, 1981).

26. Rudolph Pariser, et al., “The Department of Energy: Some Aspects of Basic Research in the Chemical Sciences, Part 2,” (Washington D.C.: National Academy Press, 1981).

27. CHEMRAWN VII, “The Chemistry of the Atmosphere: Its Impact on Global Change,” Baltimore, Maryland, 2-6 December 1991; CHEMRAWN XIV, “Toward Environmentally Benign Processes and Products,” Boulder, Colorado, 9-13 June 2001.

Appendix

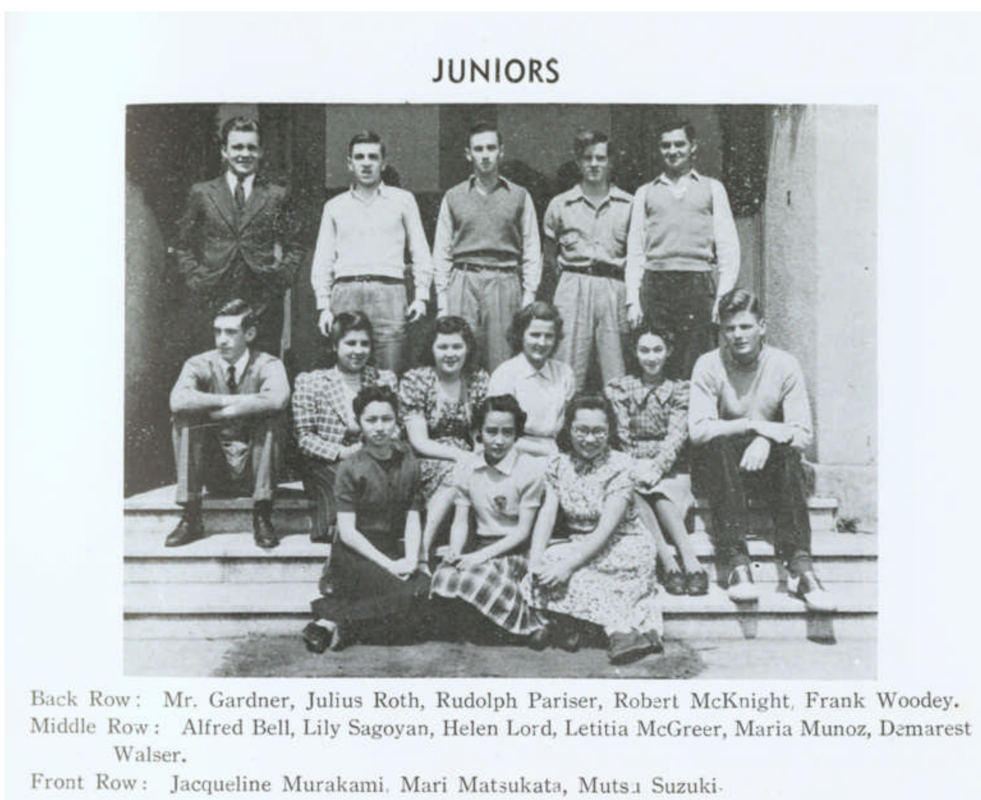
Lia Pariser, circa 1942.

Ludwig Pariser, circa 1940.

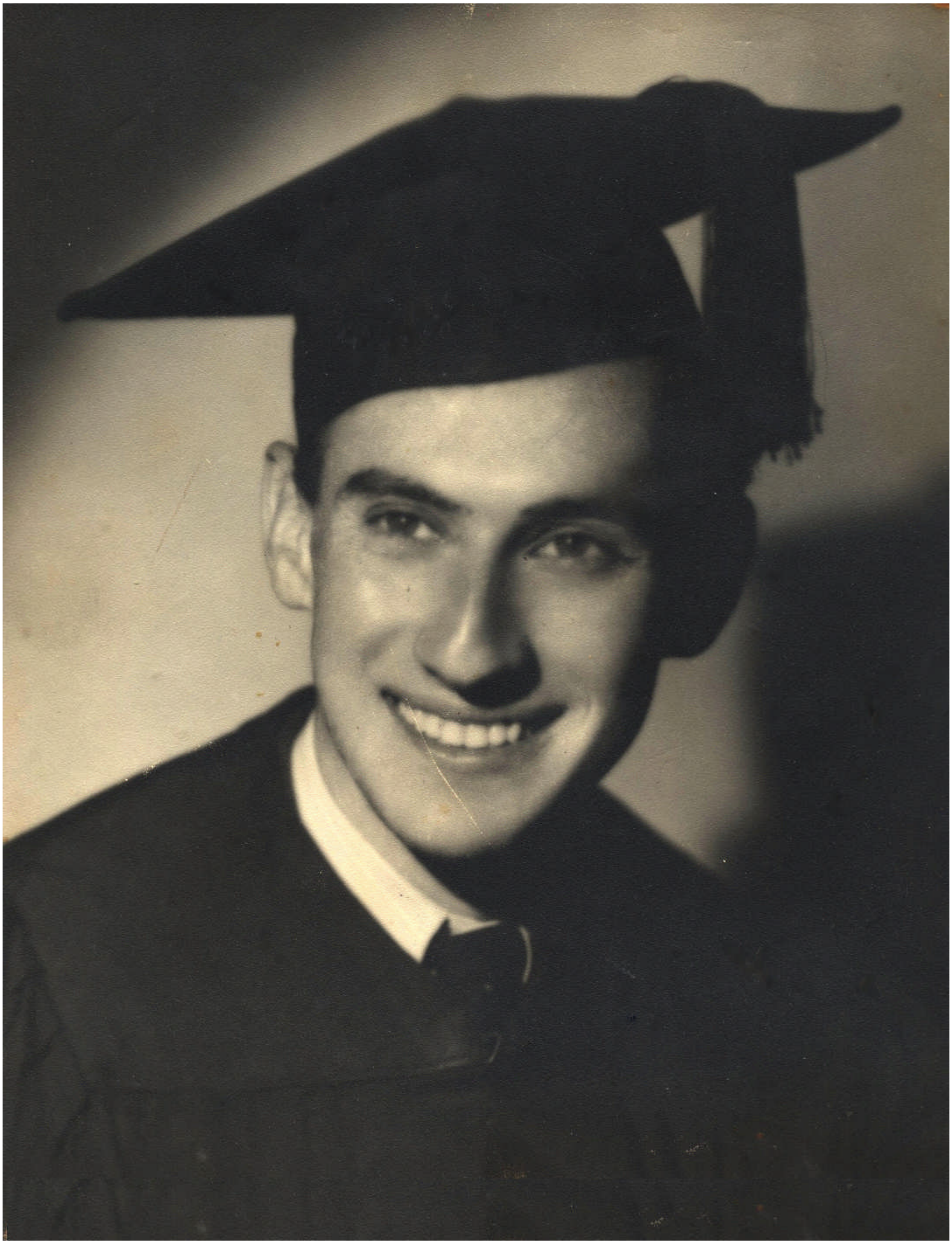
Pariser with friend Olga Guterres in Kamakura, Japan, 1939.

Jim Rasbury, as pictured in the
American School in Japan [ASIJ] yearbook, 1939.

Pariser (center) and his friends at the ASIJ, 1940.

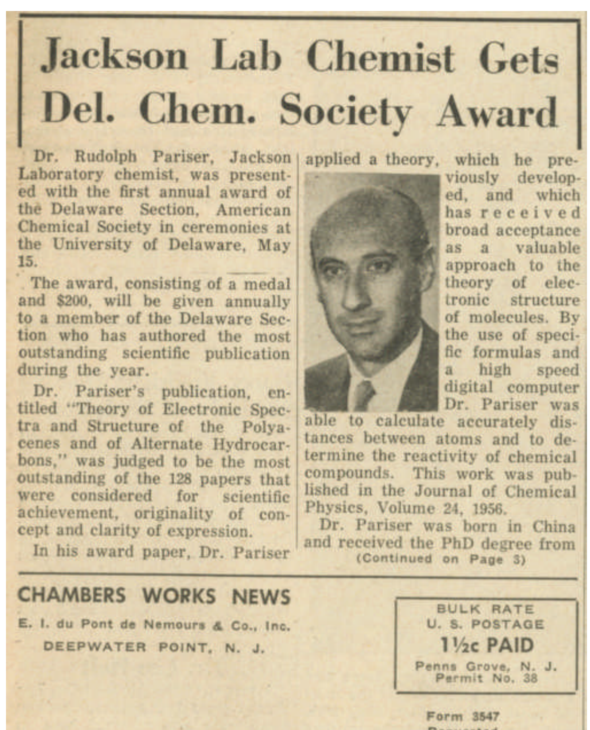


Pariser’s junior class at the ASIJ, 1940.



Pariser after graduating from the
University of California, Berkeley, with his B.Sc. in chemistry, 1944.

Pariser's research group at DuPont's Jackson Laboratory.



Jackson Lab Chemist Gets Del. Chem. Society Award

Dr. Rudolph Pariser, Jackson Laboratory chemist, was presented with the first annual award of the Delaware Section, American Chemical Society in ceremonies at the University of Delaware, May 15.

The award, consisting of a medal and $200, will be given annually to a member of the Delaware Section who has authored the most outstanding scientific publication during the year.

Dr. Pariser's publication, entitled "Theory of Electronic Spectra and Structure of the Polyacenes and of Alternate Hydrocarbons," was judged to be the most outstanding of the 128 papers that were considered for scientific achievement, originality of concept and clarity of expression.

In his award paper, Dr. Pariser applied a theory, which he previously developed, and which has received broad acceptance as a valuable approach to the theory of electronic structure of molecules. By the use of specific formulas and a high speed digital computer Dr. Pariser was able to calculate accurately distances between atoms and to determine the reactivity of chemical compounds. This work was published in the Journal of Chemical Physics, Volume 24, 1956.

Dr. Pariser was born in China and received the PhD degree from

(Continued on Page 3)

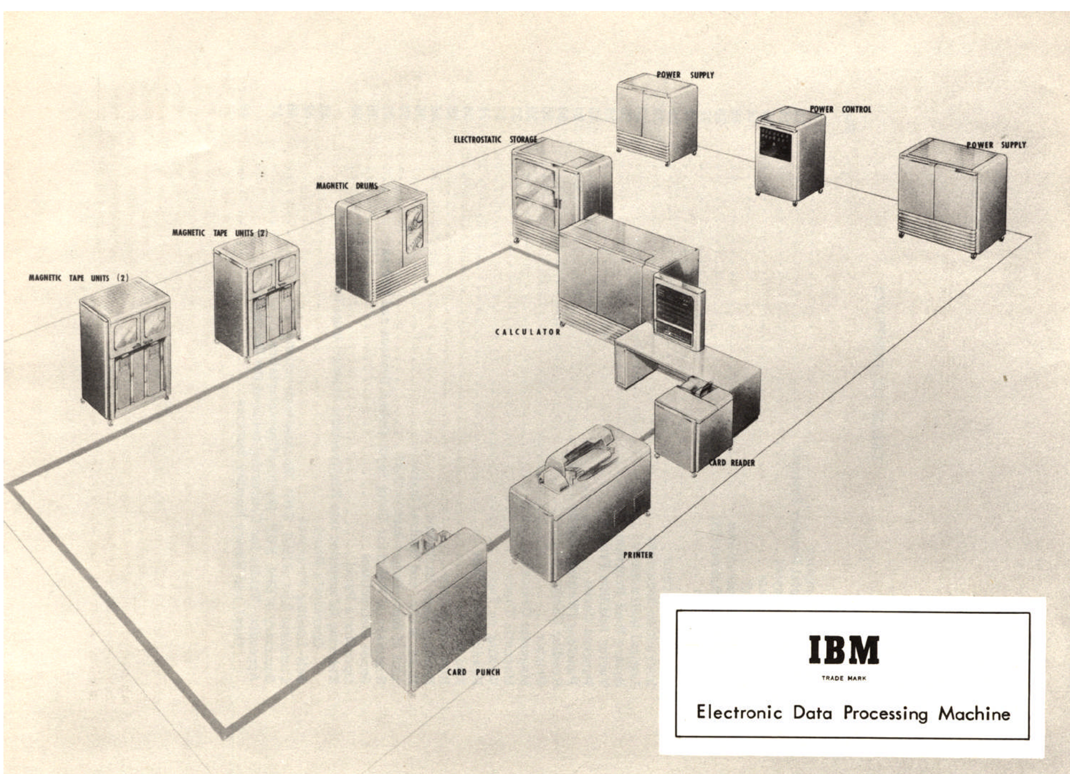
CHAMBERS WORKS NEWS
E. I. du Pont de Nemours & Co., Inc.
DEEPWATER POINT, N. J.

BULK RATE
U. S. POSTAGE
1½c PAID
Penns Grove, N. J.
Permit No. 38

Form 3547
Requested

While at Jackson Laboratory, Pariser received the first annual award of the American Chemical Society, Delaware Section, 1957.

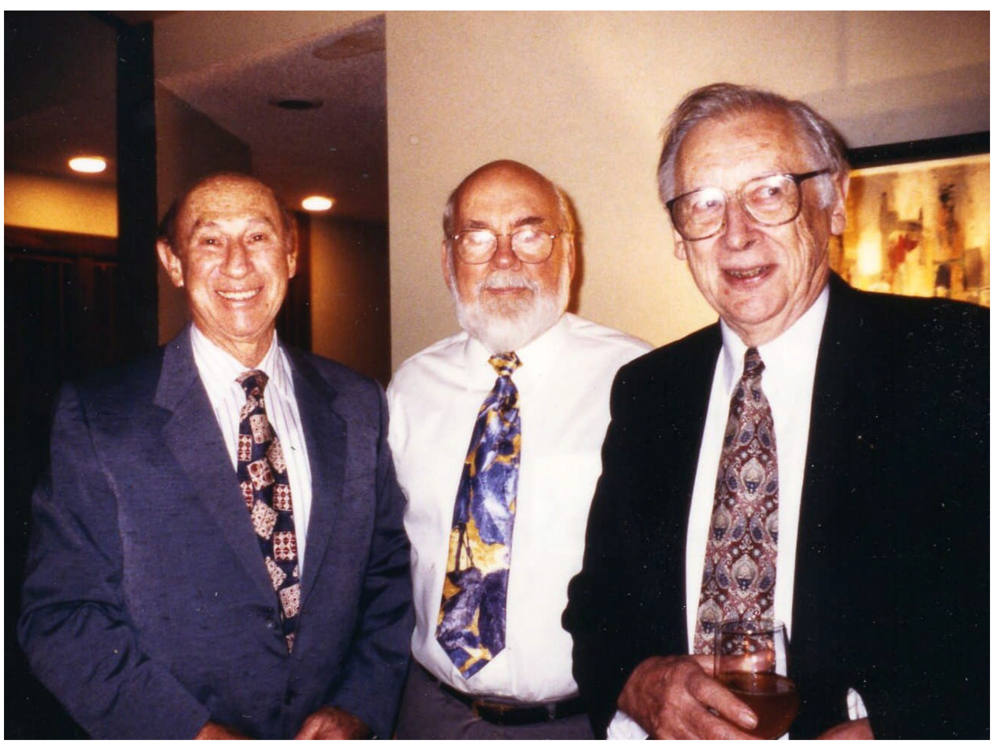
The first instructional class for the IBM 701 Electronic Data Processing Machine.
(Pariser in front row, far right.)



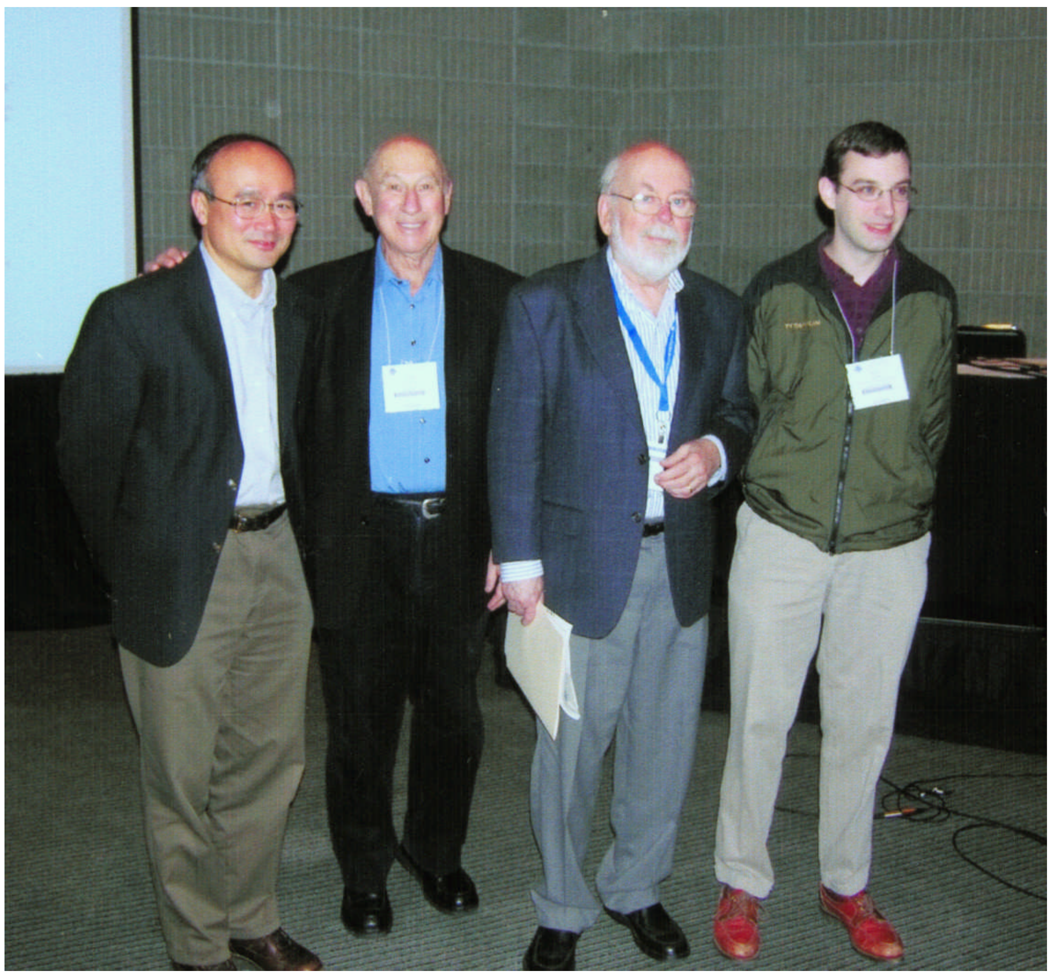
Layout of the IBM 701 Electronic Data Processing Machine.

Pariser, in his office, as the Director of Pioneering
Research for the Elastomer Chemicals Department at DuPont, 1978.

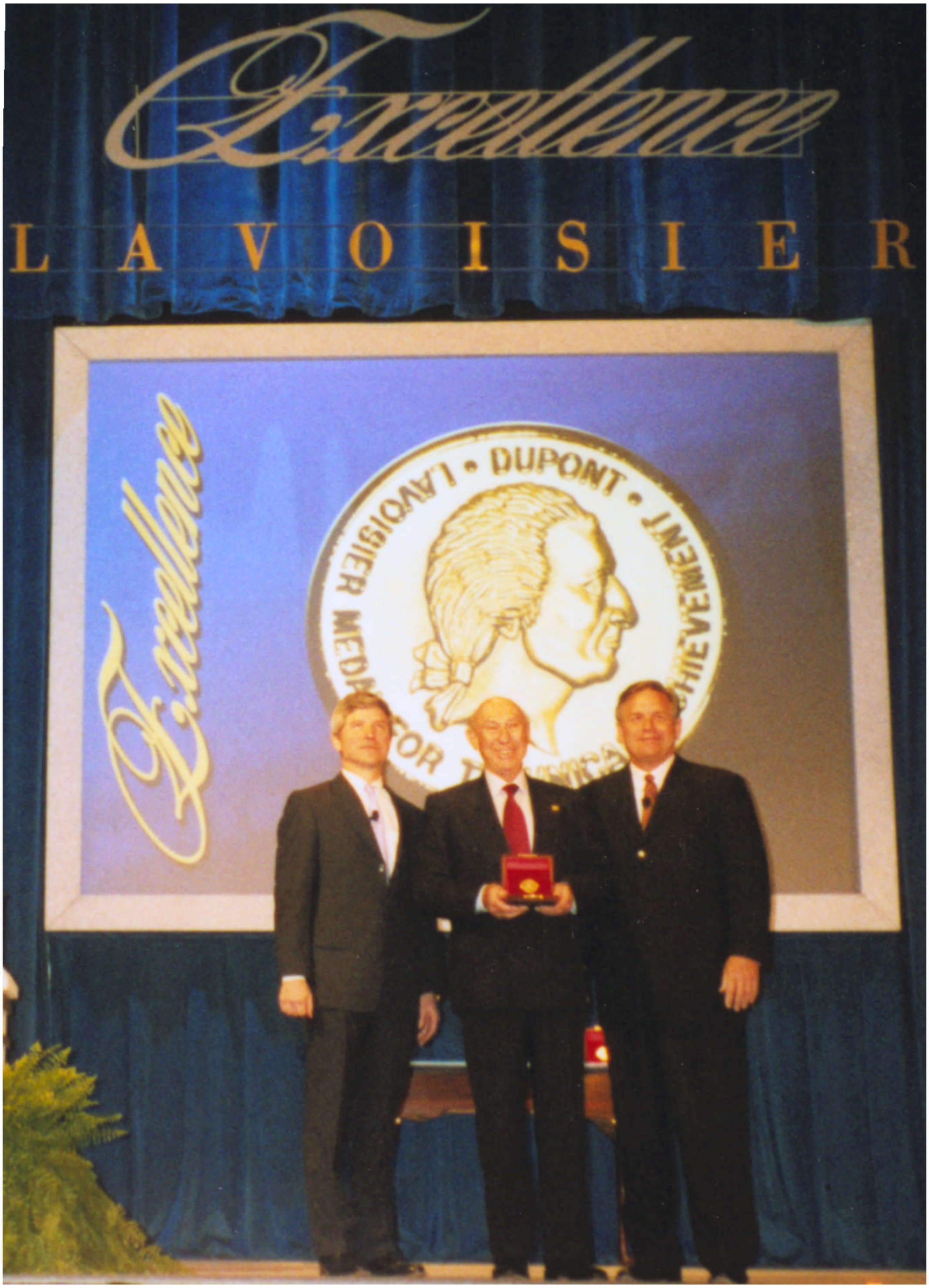
Herman Schroeder, Charles Pederson, and Pariser in
Stockholm, Sweden, for Pederson's Nobel Prize ceremony, 1987.



Pariser, John Pople, and Robert Parr, at Parr's
house in Chapel Hill, North Carolina, 1999.



Weitao Yang, Pariser, Robert Parr, and Paul Ayres
at an ACS meeting in Atlanta, Georgia, 2006.



Thomas Connelly, Jr., Pariser, and Charles Holliday, Jr.
at the Lavoisier Award ceremony for Pariser, 2003.

"The Interlocuters," a Philadelphia-based discussion group of which Pariser has been a member since 1961. (Pariser in second row, center)

Margaret Louise Marsh, circa 1970.

Louise and Rudy Pariser after their wedding
at the Pink Beach Club in Bermuda, 1972.

The Parisers at their home in Hockessin, Delaware, 2003.

The Parisers at “Ruthy’s Run” in Aspen, Colorado, 1997.

Holiday postcards featuring the Parisers skiing.

The Parisers on Louise's birthday, 2005.

INDEX

2,3 dichlorobutadiene, 29
3M Company, 30

A
All-Germany Soccer Team, 2
Alternant hydrocarbons, 24
American Cyanamid Company, 22
American missionary school, 5
  dating procedures, 6
  destruction of, 5
American School in Japan [ASIJ], 6-7
  mentors, 7
  principal [Harold C. Amos], 8
  reunion, 7
  teachers
    Misner, David, 7
    Rasbury, Jim, 8
Anastas, Paul, 43
Anderson, Burt, 37
Anthracene, 22
Anti-Semitism, 4
Arizona, University of, 30
Armbrecht, Jr., F. M. Ross, 40-41
Ascorbic acid, 15
Aufdermarsh, Jr., Carl A., 30
Azulene, 24-25

B
Ballinger, Walter F., 44
Baltzell, E. Digby, 44
Bayer AG, 32
Beckman DU, 18
Berkeley, University of California at [UC Berkeley], 9-11
  accelerated programs at, 10
  chemistry department chair [G.N. Lewis], 11
  curriculum, 11
  International House, 9-10
  professors at
    Calvin, Melvin, 11, 18
    Giauque, William F., 11
    Hildebrand, Joel aH., 11
    Latimer, Wendell M., 11
    Oppenheimer, Frank F., 11

Oppenheimer, J. Robert, 11
Seaborg, Glenn T., 12
Biopolymers, 42
Birks, John W., 43
Bodine, James F., 44
Bryn Mawr College, 45

C
Calco Chemical Company, 23
California, University of, 10, 14
Calvin, Melvin, 11, 18
Camp Crowder, 14
Camp Roberts, 12
Card programmed calculator, 21
Carey spectrophotometer, 18-19
Carnegie Institute of Technology [Carnegie Tech], 19
Carothers, Wallace H., 28, 40
Chelating copper, 38
Chemical Heritage Foundation, 1, 39, 46
Board of Overseers, 46
Chemistry Applied to World Needs [CHEMRAWN], 42
CHEMRAWN I, 43
CHEMRAWN II, 43
CHEMRAWN VII
Future Actions Committee, 43
CHEMRAWN XIV, 43
Future Actions Committee, 43
Chemistry of the Atmosphere. *See* CHEMRAWN II
China
Foreign concessions during World War II, 7
Revolution, 4
Chlorobutadiene, 29
Chlorophyll sensitized reactions, 14
Chowdhry, Uma, 39-40
City of Paris department store, 9
Colorado, University of, 43
Compression set, 30, 45
Compression tests, 28
Connelly, Jr., Thomas M., 32, 40
Cooper, Stuart, 45
Copolymerizing with ethylene, 31
Cram, Donald J., 39
Crawford, Bryce L., 14, 17, 19
Creativity at the Crossroads between Science and Technology, 39
Crown ethers, 38-39

D
Dacron, 18
Day, Larry, 44
DeBrunner, Marge, 30
Debye, Peter J. W., 19
Delaware Foundation for Science and Mathematics Education, 41
Delaware Memorial Bridge, 17
Delaware Opera Guild, 44
Delaware, University of, 44
  Professional Theater Training Program, 44
Department of Energy [DOE], 42
Dimethylaminoazobenzene, 15
Dimethylaniline, 15
Dye molecules, 24

E
E. I. DuPont du Nemours & Company, 16-19, 23-28, 30-34, 36, 38-39, 40-45
  Accounting Department, 25
  Central Research, 17, 25, 33, 36-40, 45
    maintaining the integrity of, 39
  Chambers Works, 17
  Chestnut Run Laboratory
    Technical Services Laboratory, 43-44
  employee bonus system, 30
  Executive Committee, 31
  interaction between departments, 36
  Photo Products Department, 40
  Polymer Products Department
    merger with Elastomer Chemicals Department, 36
  Sales Department, 32
    new product requirements, 32
  stock collapes [1966], 36
Eastman Kodak Company, 41-42
Elastomers Department, 28-29, 31, 36-40, 43-44
  salary system, 34
  sales team, 33
Electron affinity, 21
Electrostatic computer memory, 21
Emperor Puyi, 4
Employee Relocation Council, 44
Estes, Gerald M., 40
Experimental Station, 29, 36

F
Federal Bureau of Investigation [FBI], 9, 11
Fibers Department, 36
  business trouble, 36
Flat televisions screens, 40
Florida, University of, 45
Fortran, 27
Freilich, Steve, 40
Frensdorff, H. Karl, 31
Freon refrigerants, 17
Furs, 2, 4

G
German Army, 1
German consulate in Harbin, 5
German refugees, 3, 4-7
Germay Park facility, 33
G.I. Bill, 14
Giauque, William F., 11
Green Chemistry. *See* CHEMRAWN XIV
Grew, Joseph C., 8
Group theory, 20
Group transfer polymerization [GTP], 37, 39

H
Halliburton Company, 33
Halon fire suppression, 42
Halverson, --, 22
Harbin, China, 1-8
  education system, 3
  Japanese influence, 4
  local Chinese community, 3
Harrington, Charles J., 36
Herbst, Russel, 44
Hewlett-Packard Development Company, 37
Hildebrand, Joel H., 11
Hitler, Adolph, 2, 4-6, 13
  appointed Chancellor of Germany, 4
  speeches, 5
Hotel du Pont, 30
Hsu, Bill, 40
Hypalon chlorosulfonated polyethylene, 17
Hytrel, 31-33, 44-45
  making test quantities in Japan, 33

I
IBM [International Business Machines Corporation], 21-27
  701 Electronic Data Processing Machine, 21-22, 26-27
  Card-Programmed Electronic Calculator, 21
  magnetic tape computer memory, 21
  punch cards, 21, 27
Industrial Research Institute, 40-41
"Interlocutors, The," 44
Ionization potential, 21
Isocyanates, 17

J
Jackson Laboratory, 17-18, 24-25, 27-28, 32, 38-39
Japanese occuaption of Manchuria [1931], 4, 6
Joplin, Missouri, 14
Joseph Priestley Society, 46
Journal of the American Chemical Society [*JACS*], 16

K
Kai Shek, Chang, 4
Kaiser Permente Mills, 11-12, 17
Kalrez, 31, 33, 45
Khan, Ali, 33

L
Latimer, Wendell M., 11
Lavoisier Medal, 45
Lehigh University, 45
Lehn, Jean-Marie, 39
Lipscomb, William N., 15, 19
Livingston, Robert L., 14, 16-17
LORD Corporation, 40
Louis, Joe, 6
Lycra, 32-33

M
MacDougall, Frank Henry, 14-15
Manchuria
  Historic background, 4
Manchuria becomes Manchukuo, 4
Manhattan Project, 12
Manring, Lou, 40
Markov chain, 31
Marvel, Carl S. "Speed", 30
Massachusetts Institute of Technology [MIT], 30, 45

McClure, Don, 24
Mechanics conference in Texas, 25
Meyer, James M., 40
Minnesota, University of, 8, 14
Mitsui, 33
Molecular Orbital Theory, 24
Monterey, California, 12
Mulliken, Robert S., 23

N
Naphthalene, 22
National Academy of Sciences, 37
NACS meeting [spring 1970], 43
National Research Council [NRC], 41
Navy [United States], 42
  warships, 42
Neoprene, 27
Neoprene rubber, 17
Nobel Prize, 11, 15, 17, 38-39
North Carolina State University, 45
North Carolina, University of [Chapel Hill]
  Chemistry Department, 46
  Institute for Advanced Materials, Nanoscience and Technology, 46
NRC Committee, 42
Nuclear magnetic resonance [NMR], 30
Nylon, 18, 40

O
Office of Naval Research fellowship, 14
Officer Candidate School [OCS], 14, 41
Ohio State University, 13
  Spectroscopy and Molecular Structure Symposium, 23
Oil crisis of 1970s, 36
  substitute for neoprene and polychloroprene, 36
Oppenheimer, Frank F., 11
Oppenheimer, J. Robert, 11
Organic Chemicals Department, 28
Orlon, 18
Overberger, Charles G., 42

P
Pariser, Margaret Louise Marsh, 1, 7, 43-45
Pariser Prize, the, 30
Pariser, Rudolph
  bar fight, 6

being promoted to director of Exploratory Research, 31
boxing lessons, 6
choosing a major at UC Berkeley, 11
class president [ASIJ], 8
classified as enemy alien, 9
computer classes, 27
consulting for DuPont, 45
directing people, 41
earning his degree in chemical technology, 10
encouraging Pederson to publish, 38
enlisting in the U.S. Army, 12
enrolling in computing class, 22
evaluating performance, 41
father, 1, 3, 5, 11
  as a Ford dealer, 11
  as in house tutor, 1
  as Lieutenant in German Army, 1
  business move to Shanghai, 7
  businessman, 2
  education, 2
  encouraging interest in chemistry, 7
  purchasing a suitcase full of pearls, 11
  role in local German community, 5
  sneaking out of Japan during WWII, 11
  soccer player, 2
  war decorations, 5
father's brother, 2
first scientific talk, 16
graduation from ASIJ, 10
guiding philosophy, 40
head of neoprene research, 28
hobbies, 44
honorable discharge, 14
initiating outreach program at Delaware Univ., 45
invention for batch process analysis, 16
joining the chemistry department at University of Minnesota, 14
joining the Sales Department, 35
languages, 1
learning advanced mathematics from Parr, 20
making neoprene more crystallization resistant, 29
meeting Robert Parr, 15
military training at Ohio State University, 13
mother, 8, 11
  work with refugees, 4, 6
  working at Neiman Marcus, 9

mother's family, 1, 2
moving to Central Research, 37
perceived legacy, 45
photochemistry research, 15
pneumonia while at U.C. Berkeley, 12
polymer work for elastomers, 28
presenting a Welch lecture on fluoropolymers, 30
purpose of military training, 13
Russian translator in United States, 9
salary program, 34
sea voyage from Yokohama to the United States, 8-9
teaching himself Molecular Orbital Theory, 19
thesis work in chemical kinetics, 15, 20
whitening agent stability work, 18
wife [Margaret Louise Marsh Pariser],1, 7, 43-45
winning the Lavoisier Medal, 45
work in Richmond shipyards, 9
working at Kaiser Permanente Mills, 11
working at Richmond shipyards, 10
working with CHEMRAWN, 42
working with the NRC, 42
Parr, Robert G., 14-16, 19-22, 34
being hired as a consultant for Pariser, 19
PhD thesis, 19
Patterson Schwartz Real Estate, 44
Pattison, Dexter, 30
Pearl Harbor, bombing of, 8, 10
Pedersen, Charles J., 17, 29, 38, 45
Pennsylvania, University of, 45
Pentacene, 22
Pheophytin, 15-16
Photo resists, 40
Polymeric foams, 28
Polymers, 27, 37, 42
Polyurethanes, 17, 31-32
Pople, John, 23
PPG Industries, 40
PPP theory, 27, 37, 39, 45
calculation of electronic repulsion integrals, 20
competitors, 23
computer solutions, 24
early work with benzene, 20-21
initial calculating difficulties, 21
initial discussions between Pariser and Parr, 19
polyacenes calculations, 24

publications, 23
references to, 24
repulsion integrals, 21
reviewer's comments, 23
using a mechanical calculator, 20
zero differential overlap [ZDO], 20, 23
Presidio of Monterey, 12

Q

Quantum chemistry, 25
Quantum Chemistry, 19
Eyring, Henry, 19
Kimble, George E., 19
Walter, John, 19

R

R. Pariser & Co., Inc., 45
Rasbury, Jim T., 8, 11
American spy, 8
disappearance from ASIJ, 8
re-encountering Pariser family in United States, 9
Reichstag fire, 4
Rendell, Edward G., 45
Richmond, California, 9
Robert Boyle Society, 46
Rossiter, Bryant W., 42
Royal Hawaiian Hotel, 9
Russian Revolution, 1
prisoners of war, 1
refugees, 3
Russo-Japanese War, 4

S

Sabine River, 32
Salem College, 44
Schmeling, Max, 6
assistant trainer, 6
Schriesheim, Alan, 42
Schroeder, Herman E., 17, 18, 26, 28, 31-33, 38
Schwartz, Patterson, 44
Scott, Sam, 17
Seaborg, Glenn T., 12
Shapiro family, 1
import/export business, 2
Shapiro, Irving S., 43

Sheldon, John, 21-22, 27
Shortwave radio, 5
Simmons,Jr., Howard E. [Howie], 25, 36, 39, 45
Smith, Clay, 40
Smith Kline and French Laboratories, 17
Snyder, Hal, 40
So Lin, Chang, 4
Specter, Arlen, 45
Stereo-regular polymerization, 30
Stilbene derivatives, 18
  isomerization, 18
Superconductivity project, 39

T
Teflon, 17, 27, 31
Tetracene, 22
Tetraethyl lead [TEL], 17
Textiles, 2
Thackray, Arnold, 46
Thermoplastic polyurethane, 32
Tinker, John M., 17-18
Trainham, James A., 40
Trans-Siberian Railway, 1

U
United States' immigration quota system, 4
US Signal Corp, 14

V
Vacuum tubes, 21
  difficulties, 21
Vamac, 31, 36-37, 45
Vanadium catalyst, 39
Viton, 17, 30-31, 45
Vladivostok, Russia, 8
von Hindenburg Schule, 3
von Hindenburg, Paul, 4

W
Waikiki Beach, Hawaii, 9
Warlords in Manchuria, 3
WASP [White Ango-Saxon Protestant], 44
Wasserman, Edel, 42
Watson, Thomas J., 22
Welch Foundation Lecture Committee, 30

Willett, Al, 18-19
Wilson, North Carolina, 44
Winston Salem, North Carolina, 44
Wisconsin, University of, 45
Witsiepe, William K., 32
World War I, 1
World War II, 4, 7, 28
  concentration camps, 7
  Japanese intern camps, 9
  Japanese occupation of Shanghai, 11
  Normandy Invasion, 13
  Persecutions of Germans in Harbin, 7

Y
Yatsen, Sun, 4

Z
Zero differtial overlap [ZDO], 20-21
Ziegler-Natta catalysis, 30