

CHEMICAL HERITAGE FOUNDATION

JOHN C. WARNER

Transcript of an Interview
Conducted by

John A. Heitmann

at

Gibsonia, Pennsylvania

on

8 February 1984

(With Subsequent Corrections and Additions)

CENTER FOR HISTORY OF CHEMISTRY ORAL HISTORY PROJECT

This manuscript is based on a tape-recorded interview conducted for the ACS-AICHe - University of Pennsylvania Center for History of Chemistry, the tape and the manuscript being the property of the Center. I have read the manuscript and made only minor corrections and emendations. The reader is, therefore, asked to bear in mind that this is a transcript of the spoken word rather than a literary product.

I wish to place the following condition upon the use of this interview, and I understand that the Center will enforce that condition to the fullest extent possible:

(Check One)

OPEN. This manuscript may be read and the tape heard by scholars approved by the Center. The scholar pledges not to quote from, cite, or reproduce by any means this material except with the written permission of the Center.

MY PERMISSION REQUIRED TO QUOTE, CITE, OR REPRODUCE. This manuscript and the tape are open to examination as above. The scholar pledges not to quote from, cite, or reproduce by any means this material except with the written permission of the Center, in which permission I must join. Upon my death this interview becomes Open.

MY PERMISSION REQUIRED FOR ACCESS. I must give written permission before the manuscript or tape can be examined (other than by Center staff in the normal course of processing). Also, my permission is required to quote from, cite, or reproduce by any means. Upon my death this interview becomes Open.

John G. Warner
(Signature)

Sept. 13, 1984
(Date)

Upon John C. Warner's death in 1989, this oral history was designated **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 (CHF) Oral History Program to notify CHF of publication and credit CHF using the format below:

John C. Warner, interview by John A. Heitmann at Gibsonia, Pennsylvania, 8 February 1984 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0044).



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



The Chemical Heritage Foundation (CHF) serves the community of the chemical and molecular sciences, and the wider public, by treasuring the past, educating the present, and inspiring the future. CHF maintains a world-class collection of materials that document the history and heritage of the chemical and molecular sciences, technologies, and industries; encourages research in CHF collections; and carries out a program of outreach and interpretation in order to advance an understanding of the role of the chemical and molecular sciences, technologies, and industries in shaping society.

JOHN C. WARNER

1897 Born in Goshen, Indiana on 28 May

Education

1919 A.B., chemistry, Indiana University
1920 M.A., chemistry, Indiana University
1923 Ph.D., chemistry, Indiana University

Professional Experience

1918-1919 Barrett Company
Chemist

1920-1921 Cosden Oil Company
Research Chemist

1922-1923 Indiana University
Chemistry Instructor

1924-1926 Wayne Chemicals Corporation
Research Chemist

1926-1928 Carnegie Institute of Technology
Chemistry Instructor

1928-1933 Assistant Professor of Chemistry

1933-1936 Associate Professor of Theoretical Chemistry

1936-1938 Associate Professor of Metallurgy

1938-1949 Professor of Chemistry and Department Head

1945-1949 Dean of Graduate Studies

1949-1950 Vice President and President Elect

1950-1965 President

1965-1989 President-Emeritus

Honors

- 1958 Pittsburgh Junior Chamber of Commerce Man of the Year Award
- 1963 Pittsburgh Graphic Arts Council Award
- 1964 Horatio Alger Award
- 1965 Western Pennsylvania Board of Industrial Realtors Award
- 1966 Pennsylvania Award for Excellence in Education
- 1968 Distinguished Alumnus Award, Indiana University

ABSTRACT

John C. Warner begins the interview with a discussion of his family and childhood years growing up on a farm. He developed an interest in science in high school due to the encouragement of his science teacher, G. W. Warner. He enrolled in Indiana University in 1915. There, he received his A.B. in chemistry in 1919, his M.A. in 1920, and his Ph.D. in 1923. While in college, Warner worked for the Barrett Company working on synthetic phenol processing. As a graduate student, he was a research chemist for the Cosden Oil Company. After working for Cosden for just under a year, he returned to Indiana University as a chemistry instructor while completing his graduate studies. In 1926, he joined the faculty of the Carnegie Institute of Technology (later Carnegie Mellon University) as a chemistry instructor. Warner spent the rest of his career at Carnegie. He rose through the university ranks, eventually becoming president of Carnegie Mellon in 1950. Warner restructured and developed the University's chemistry department. During his time at Carnegie, he worked closely with Charles Thomas on the chemistry, metallurgy, and plutonium purification aspects of the Manhattan Project. He also served as a liaison between Oak Ridge Laboratories and Monsanto Company for this project. Warner became a board member of Jones and Laughlin, Pittsburgh Plate Glass, and served as director of Spang and Company. Warner concludes the interview with a discussion of his family and reflections on his role in the advanced educational development in Southeast Asia and the Middle East.

INTERVIEWER

John A. Heitmann holds a B.S. degree in chemistry from Davidson College and an M.A. degree in history from Clemson University. From 1971 to 1977 he worked as a chemist in the metallurgical industry. He then studied at The Johns Hopkins University under Owen Hannaway and received his doctorate in the history of science in 1983.

TABLE OF CONTENTS

- 1 Family History and Childhood
Parents and grandparents. Childhood on tenant farm. Early interest in science.
Inspiring science teacher.
- 8 College and Graduate School
Fraternity and football activities. Faculty members. Chemistry department.
Academic chemistry in the 1920s. Colleagues at Indiana University.
- 16 Early Work Experience
Wartime research at the Barrett Company. J. Bennett Hill. Harold Urey and other
colleagues. Cosden Oil Company. Racial tension in Oklahoma. Louise Hamer.
- 23 Teaching career
Carnegie Tech chemistry department. Harry Seltz. History of Carnegie Tech.
Developing graduate courses and choosing texts. Funding for graduate students.
President Doherty. Kinetics research. Goals and guidelines for chemical education.
Remembering graduate students: Paul Fugassi, Bill Svirbely, Dave McKinney, Sam
Eagle and others.
- 36 Chemical Community of Pre-World War II
Farrington Daniels, Morris Kharasch, Jack Kirkwood, George Scatchard, Charles
Price and Tom McCutcheon. Work of Martin Kilpatrick. Speakers at Carnegie
Tech. Early years of chemistry department. Anti-Semitism in chemical profession.
- 45 Manhattan Project
Glenn Seaborg and the Chicago Group. The Hanford piles. Coordinating research.
The bomb. Liaison between Monsanto and Oak Ridge.
- 54 Post War Environment at Carnegie Tech.
Slow beginnings. Becoming president of Carnegie Tech. Fundraising. Influence of
Ben Fairless. Mellons and Scaifes. Educational goals.
- 67 Corporate and Committee Work
Jones & Laughlin, PPG, and the Dravo Corporation. Spang and Co. Louise Warner.
ACS activities. Election to the Academy. Nuclear powered airplane. I. I. Rabi.
- 74 Final Thoughts
Future of fundamental research. Children. Science and engineering in developing
nations. The Ford Foundation and UNESCO.
- 84 Notes
- 85 Index

INTERVIEW: John C. Warner
INTERVIEWER: John A. Heitmann
LOCATION: Gibsonia, Pennsylvania
DATE: 8 February 1984

HEITMANN: I know that you were born on 28 May 1897 in Goshen, Indiana, but I don't know very much about your parents and your family background. Can you tell me something about your parents?

WARNER: I know the family on the Warner side only back as far as my grandfather, William Warner. In 1847 he traveled from Dresden, Saxony, which later became a part of unified Germany, to northern Indiana. He was a potter and a farmer. During the same period, the woman who became my grandmother, whose name was Elizabeth Enders, came from Hessen-Darmstadt to northern Indiana to join her brother, who had already migrated there. They didn't know each other before they met in northern Indiana. They had a family of four boys and one daughter. My father, Elias, was the second son. The daughter was the youngest in the family. All of these Warner children of my grandfather and grandmother had families of their own, mostly boys. I think there was only one daughter, and she had one daughter. Of that generation of grandchildren, I am now the only remaining living one. I think Grandmother Warner came from quite a good family in Hessen-Darmstadt. I think her father was head of the legislative body there. There are a lot of interesting stories about her. She was supposed to have had an affair with the ship's captain on the way over. She appeared very well-to-do because she had twenty pair of shoes.

HEITMANN: That was well-to-do for those days.

WARNER: Grandfather Warner died before I ever knew him. I knew and loved Grandmother Warner. She was a very good-humored person.

HEITMANN: And she lived in Goshen?

WARNER: No, she lived in Nappanee, Indiana, which is another northern Indiana town. All the time I knew her, she lived with her daughter. She was delightful. My father married into the Plank family; some of them spell it Planck. That's the other side of my family. Their history went back one generation more than the Warner side. That side of the family that we

know best was grandmother Plank's family, which was named Mehl. They were primarily a mixture of English and Pennsylvania Dutch who came from Pennsylvania into Ohio and then into northern Indiana. The Mehl family was a very large family. They had fifteen children that grew to adulthood and had families of their own.

HEITMANN: Were they Catholic?

WARNER: No. They were actually Mennonites. That was the church that they belonged to, at least when they were in Indiana. One of their boys, John Christian Mehl, was a Mennonite preacher. He was the one that I was named after. He was one of my mother's favorite uncles; so she named me John Christian Warner.

HEITMANN: Your mother was a country schoolteacher, wasn't she?

WARNER: Yes, she was one of the granddaughters of the old patriarch, great grandfather Christian Mehl. I don't know anything about his wife, but they had quite a mixture of boys and girls. One of the girls was named Lydia, and she was my mother's mother. She was the Mehl girl who married a fellow by the name of John Plank. The Planks were supposedly French Huguenots who settled first around Easton, Pennsylvania and then moved west. John Plank was a combination carpenter and farmer. He and Lydia had four daughters and one son.

HEITMANN: The reverse of the Warner Clan.

WARNER: Evening up the sexes. Anyway, my mother was the second child, second daughter and she would normally have been very eager to get an education. She graduated from grade school, I guess, and then went to a normal school. I think she only had to go to normal school for six months in order to become a country schoolteacher. From what I remember she must have been an excellent teacher, and very bright.

HEITMANN: This was in the 1890s?

WARNER: I was born after she began teaching school. She taught school and then got married. I was the second of her children. She had an older son and I was born in 1897. I have forgotten just when she was married, but it must have been in the 1880s that she was teaching school. I think she taught school for only two years before she was married. She married the second son of my grandfather, William Warner. The name has been prominent in the Warner family. Grandfather William Warner had a son named William Warner, I have a son named

William and a grandson named William Warner.

HEITMANN: That's through four generations.

WARNER: Father didn't have anything but a grade school education. You can describe him best as a farmer and a lumberman. He was a lumberman every winter and a farmer almost every summer.

HEITMANN: How big a farm did you have?

WARNER: We didn't have one. He was a tenant farmer. You see, Dad died when I was eight years old. He and my mother had been married just fourteen years when he died of pneumonia. We didn't know how to cure it then. One whole summer he didn't farm, either. There was a hardwood lumber company, Sanders-Egbert Company, that had a plant in Goshen, Indiana and one in South Bend, Indiana. They made all kinds of building lumber. They would go up into Wisconsin and northern Michigan and buy a stand of timber, rough saw it, and then ship it down to these plants where they would make things, even veneer. Dad would work for them in the wintertime. One year he went into one of these stands that they had bought in upper central Wisconsin, and put in a rough sawmill to take out this stand of timber. He didn't do any farming at all that year. His first bout of pneumonia was a result of that. One of his workmen fell into the river in the wintertime. Dad jumped in to save the guy and he wound up with pneumonia. He had pneumonia the following two winters and finally died.

HEITMANN: So, when you were young you had it very hard.

WARNER: We were very poor.

HEITMANN: How did you get money just to exist?

WARNER: My eldest brother, who died just six months ago, and I went to work right after Dad's death. We worked before and after school, and in the summertime. Dad died in April, and that summer we were loaned out as slave labor to an uncle, weeding onions and celery and stuff like that.

HEITMANN: Just as farm labor.

WARNER: Yes. From then on I was always working, before and after school. When Dad died he left ten acres of pretty poor land right across the highway from Grandfather Plank's farm. Grandfather Plank and Grandmother Plank had a farm of over one hundred and ten acres, and right across the highway my dad had bought ten acres. I don't know what he had planned to do with it. It wasn't good for much of anything except raising strawberries. It was pretty sandy.

When Dad died, my mother was pregnant with her only daughter, who was born a month after Dad died. We had a big family. I had three brothers and this sister.

HEITMANN: And you were the second.

WARNER: I was the second. After me, there were two more boys, and then finally, the daughter, who didn't live. I think she was six or seven years old when she died of spinal meningitis. We boys all grew up to adulthood and married and had families. They are all gone now. I'm the only one left.

HEITMANN: What did your brothers do?

WARNER: When Grandfather died, he was in his seventies, maybe eighty. He died within a year of the time my father died. My grandmother thought she could run the farm by hiring help. She was a pretty difficult person to get along with. I'd say she was a very bossy and arrogant sort of old girl. She never had anyone she hired to help run the farm that would stay with her very long. When Dad died, my older brother would have been ten. By the time he was around thirteen or fourteen, she gave up and got him to start running the farm for her.

HEITMANN: So he ran the Plank farm and your plot as well?

WARNER: In the meantime, my mother had built a cottage on this ten acres of land across the highway. Finally, my grandmother decided that my brother, who was a great big fellow over six feet tall, and just as broad as I am, would run the farm. Everyone thought my brother was older than he was. His name was a strange one, DeMain. I don't know how my mother ever thought that one up. Anyway, DeMain was a hard-working, strong guy and he had learned how to get along with grandmother pretty well. I think she did him dirt often enough. She'd always get him to do things for her saying, "You'll be rewarded someday." I think that when she died she left him one hundred dollars in her will.

HEITMANN: That was his reward!

WARNER: Well anyway, five years before she died, she decided that DeMain could run the farm himself. She had built a house in town, in Goshen, and moved into that. Then the farm became our home. It must have been about the time that I started high school, although it may have been the year before I started high school. My education before high school was all in one-room country schools.

HEITMANN: Did you have any interest in science during these early years? Or was it just a matter of survival and learning what you could along the way?

WARNER: It was a struggle for survival, in a way. I think my interest in science started in high school. A farm boy learns to do all kinds of things, some of which have scientific aspects.

HEITMANN: Did you have any contact with County Agriculture Agents?

WARNER: I didn't, but my brother did. I was born on a farm about five miles east of Goshen. My father was then a tenant farmer for one of my mother's uncles, a lawyer in town who was one of the Mehls. He was a lawyer, but he owned a farm about five miles east and got my father to farm it for him. That's where I was born. When father died, he had a farm he rented a couple of miles closer to town than that. But then, when father died and we built this cottage across the highway and started farming Grandfather's farm, the old farmhouse became our home. I think I went to a country school one year more after we moved onto Grandfather's farm. There was just one farm between that farm and the city limits line of Goshen. The ten acres across the highway were right next to it because the residential part of Goshen had come out farther on that side of the highway. These ten acres was the first open land outside of the city limits. It was not very far to the high school and I rode a bike to the high school all the time.

HEITMANN: It's in high school that you really got interested in science. Who was the person you mentioned earlier that was a very important high school teacher?

WARNER: His name was G. W. Warner, but he was not a relative of mine. I think I put that picture in here thinking that you'd probably want to see it. Here are G. W. Warner and J. C. Warner, in front of Goshen High School.

HEITMANN: Yes, he was very young himself, as a teacher.

WARNER: I thought he was pretty mature.

HEITMANN: He doesn't look like he's older than twenty-five or so in this picture.

WARNER: I think he was a little older than that.

HEITMANN: Was he?

WARNER: He was an Indiana University graduate. That's old Goshen High School behind us.

HEITMANN: I see.

WARNER: He was so encouraging. In the wintertime, when I had time on Saturday and there wasn't any farm work to do, I'd come in to the high school to do extra experiments in physics and chemistry. He'd come over from his home to be with me while I was doing it. He'd go to that bother. I had really more interest in physics than chemistry at that time. I think I really made up my mind to major in chemistry when I went to Indiana because, as far as I could tell, you had a better chance of getting a good job in chemistry than you had in physics.

HEITMANN: Certainly at that time.

WARNER: At that time the only outlet for a physics major was academic.

HEITMANN: Do you remember any of the experiments that you did in high school?

WARNER: Oh, I did almost everything that they do in a first year physics course in college. I bound the reports of my experiments all together in a sort of mimeographed volume.

HEITMANN: In a binder?

WARNER: In a binder, and I designed and drew a cover for that. He suggested that in the cover I try to illustrate all the different sections, or divisions, of physics: light, heat, and mechanics, and so forth. I took that volume along with me to Indiana at his suggestion. I didn't

do any freshman physics. They let me go ahead and do advanced physics.

HEITMANN: What was this high school teacher's first name?

WARNER: It was George Washington, but everybody just called him G. W.

HEITMANN: Did he encourage other students other than yourself?

WARNER: I think I was pretty much of an exception. I can't think now of any other kids from that high school that went on in science. I'll tell you another interesting thing about him. A few years after I was there, I think I was still at Indiana, he left Goshen High School to become head of science in the Chicago junior college system. He started a magazine for high school science and math teachers called *School Science and Mathematics*.

HEITMANN: He was a very dynamic person.

WARNER: I have a copy of his magazine. He kept following my career.

HEITMANN: I was going to ask you that. Did you go back and see him from time to time?

WARNER: Yes. He stopped once here in Pittsburgh to see me. He had been to some meeting in the East. I used to see him back in Indiana once in a while. Twice he invited me to write little articles for his magazine.

HEITMANN: I wonder if he ever dreamt that someone to whom he taught high school physics experiments back in Goshen High would ever go on and become president of a major university.

WARNER: He kept in correspondence with me all these years and he was delighted, I'm sure. I have a copy of his magazine and I'll show it to you.

HEITMANN: I suppose it was due to G. W. Warner's influence that you went to Indiana University.

WARNER: I don't know how much stuff you're interested in. What I really wanted to do was to go to Swarthmore. But I couldn't figure out any way under the sun that I could afford it. I think, as far as my career is concerned, I would have had a much earlier start in my scientific career if I had gone to Swarthmore, or Harvard, or someplace like that for graduate school. We haven't gotten to my views about Indiana University yet.

HEITMANN: We're getting close to that. So, you enrolled at Indiana University in 1915. What was your first impression of the school when you got there as a freshman, coming from a farm community and then going to Bloomington?

WARNER: I had a good English teacher in high school. Harlan Yenne went back to Indiana University as a graduate student the same year that I went there as a freshman. His intention was to become a professor of English. His father was a superintendent of schools in Washington, Indiana, which is a southern Indiana town. I didn't know it at the time, but he had fallen in love with a girl in my high school class, and kept on courting her. I, of course, joined his fraternity.

HEITMANN: What fraternity was that?

WARNER: Delta Upsilon, and it did a good job of taking a green country lad and making him a pretty sophisticated guy, socially. It happened pretty rapidly, I would say. I found my freshman year at Indiana University nonsensically easy. It was mostly just a repetition of what I had in high school.

HEITMANN: Did you have other activities that you began to pursue during those years?

WARNER: I played football.

HEITMANN: On Indiana University's team?

WARNER: Yes, but I was a third-string halfback. I think I was motivated to do something to evade physical education, so I went out for football both in the fall practice and spring practice. In between, I went out for wrestling. So I never had to take a physical education course, which I despised.

HEITMANN: Did you think Indiana was a backward place in science?

WARNER: Before I tell you more about it, I think there was another man who had a definite influence on my career. He was Mr. Harry Sanders of the Sanders-Egbert Company.

HEITMANN: That was the timber company.

WARNER: The timber company that my father worked for in the wintertime. If this had happened a little earlier, I think I would have gone to Swarthmore. When I was a senior I had already made up my mind and made application to Indiana, because I knew that's where I had to go. But the principal called me in and said that Mr. Sanders would like me to come to his office. He was supposed to be the richest man in Goshen. I wondered what in the heck this was all about. Well, I went to him and Mr. Sanders essentially said, "Look, J. C. (they called me J. C.), I've followed you through your work in high school and you've done exceptionally well. I want to be sure that you go on to college." He asked me where I planned to go, and of course, I told him Indiana University. He said, "Well, I want you to go and if you get hard up, you write me like you would write to your Dad if he were alive. He worked for us. He was a good man. He did a lot of good work for us and I want to help you." Well, during the time I was an undergraduate at Indiana, I needed only three hundred and twenty-five dollars from Mr. Sanders.

HEITMANN: That was a lot of money in those days.

WARNER: That was a lot of money in those days. But by the time I was a junior, I was making my way as a lab assistant in chemistry. So I didn't need to ask him for any more help.

HEITMANN: But, he was around.

WARNER: He was around. He and his wife moved to South Bend, but they kept in touch with me as long as they lived, as a matter of fact. Every time Louise and I were back in Indiana, he would have left word with my brother DeMain that they wanted us to come to South Bend to have lunch with them. He was a very unusual guy in a way. He had a good family, and being as well-to-do as he was, they all amounted to something. His elder son was a Phi Beta Kappa at Harvard and a Rhodes scholar.

HEITMANN: It was very unusual, coming from a wealthy family like that.

WARNER: So, we would go to their place for lunch quite a number of times. The payoff came when I was inaugurated President of Carnegie. I saw that Mr. and Mrs. Sanders were invited to the inauguration, and later to a reception at the president's house. Gee! You'd think something had shone on them from heaven. Mr. Sanders said he'd never made such a good investment.

HEITMANN: So, you really had two very important people in your early career—a schoolteacher and your father's former employer. Now, getting back to Indiana, did you major in chemistry or physics?

WARNER: All science at Indiana was very descriptive and classical. There were one or two people in physics that were good teachers, [John B.] Dutcher and Mason E. Hufford. Hufford was a Penn [University of Pennsylvania] Ph.D. Every instructor that I had, and every instructor in chemistry and physics and all but one in math, were undergraduates at Indiana University. It was the most inbred place you ever ran into. O. [Oliver] W. Brown was not an undergraduate at Indiana. He did his undergraduate work at little Hanover College.

HEITMANN: Yes, that's Harvey Wiley's old school, too.

WARNER: He did his graduate work at the University of Wisconsin. I never knew whether O. W. Brown got a doctorate at Wisconsin. They called him "Dr. Brown" but it's my impression he got a master's at Wisconsin and that his old school, Hanover College, gave him an honorary doctorate. All the rest of them, outside of Brown, were undergraduates at Indiana. As so many did in those days, the head of the department, [Robert E.] Lyons, did his doctorate in Germany at Heidelberg. Clarence [E.] May obtained his at Columbia, and Frank C. Mathers at Cornell.

HEITMANN: Some of the faculty received their advanced degrees from prestigious universities. Yet, they weren't doing a lot of first rate research.

WARNER: Well, there was hardly any, except O. W. Brown and Mathers. It was not very exciting research. I suppose that Brown had students who did better graduate work than anybody else.

HEITMANN: As an undergraduate you worked as a lab assistant in the chemistry department.

WARNER: Yes, I began with sections of qualitative analysis.

HEITMANN: I guess that's for freshmen and sophomores.

WARNER: General chemistry was a first semester course, and qualitative analysis a second semester course. We met five days a week. I started being an assistant in qualitative analysis during my second year. During my third year I was in charge.

HEITMANN: You actually taught the whole course?

WARNER: Yes.

HEITMANN: That's very unusual. I mean, to have an undergraduate do that. Presumably you taught other students whom you knew on campus from other activities.

WARNER: Well, I think I kept my good senses about it. I remember a few fellows who I knew personally that weren't doing well, and I just flunked them. I think I had high standards about handling students. I had quite a lot of trouble with a pre-med that I knew. All the pre-meds had to take general qualitative analysis. His father was a prominent doctor and head of the Lutheran Hospital in Fort Wayne. This boy was just playing around. I gave him plenty of warning, but he didn't straighten out, so he had to repeat the course.

HEITMANN: That sometimes is a good lesson and has to be done as soon as possible. Would you say some more about the chemistry department?

WARNER: I would say that it was a very obsolete department. The equipment was essentially the equipment you'd need for teaching the undergraduate courses. In research you generally made your own equipment. For example, I got to be a pretty damn good glass blower.

HEITMANN: You had to be at that point. Did they have glass blowers in the bigger departments?

WARNER: I wouldn't have heard of them in those days. There wasn't a glass blower at Carnegie Tech when I first arrived. When I first came to Carnegie Tech, I bet that I was the best glass blower around.

HEITMANN: So you were a very valuable man in the department at Indiana.

WARNER: At Indiana I got imposed on. But, I became pretty clever. If you wanted an autoclave, to do something under pressure, you'd make it out of pipefitting. The physics department had a machine shop that the chemistry graduate students could use. I learned to use metal lathes and that kind of equipment.

HEITMANN: From your own experience, you'd almost think that if a student has a great deal of talent, you can't keep that student from doing well in his field. Sometimes chemistry departments will be loaded with very expensive equipment. But there is something that won't stop a good student from succeeding.

WARNER: Well, you've got to have a lot of drive and ambition.

HEITMANN: Right!

WARNER: A lot of students that went through Indiana graduate school that never amounted to much. One good thing about O. W. Brown was that he worked in the laboratory himself. He was there almost all the time. If you needed a little help, he would give some suggestions.

HEITMANN: Just back-tracking a minute, you graduated from Indiana University in 1919. You then decided to go on to graduate school at Indiana. Is there any particular reason why you didn't go then to one of the bigger schools?

WARNER: It was because Indiana made it so damn easy for me to stay. As a graduate they made me an instructor, and paid me eighty dollars a month. I should have had enough guts to turn that down and go someplace else. It was so easy to stay, and besides that, I had girlfriend there.

HEITMANN: I thought something like that might be playing a role, too. Sometimes making decisions is a complex matter.

WARNER: The real trouble with Indiana University at that time was, as I said, that it was obsolete. Brown was a professor of physical chemistry and he didn't really believe in the Arrhenius Theory.

HEITMANN: He was a student of someone in Wisconsin by the name of—

WARNER: [Louis] Kahlenberg. He was anti-Arrhenius.

HEITMANN: That's where Brown got his ideas from.

WARNER: I never heard a word about chemical thermodynamics. I never heard a word about modern solution theory. I never heard anything about modern theories of kinetics or quantum chemistry. You have to remember, of course, that the year I got my doctorate, 1923, was the year that [Gilbert N.] Lewis and [Merle] Randall was published (1). That was the book that really taught chemists to understand chemical thermodynamics and to apply it. In the good graduate schools, professors were teaching chemical thermodynamics. You had Herbert [Spencer] Harned at Penn. He later went to Yale. Joel [H.] Hildebrand was at Penn in those early days, and he had a very modern knowledge of solution theory.

HEITMANN: I guess W. A. Noyes, Jr. was at MIT [Massachusetts Institute of Technology] and then at Caltech [California Institute of Technology].

WARNER: A. A. Noyes.

HEITMANN: And Wilder D. Bancroft.

WARNER: Bancroft was a strange physical chemist. He was a sort of colloid or surface chemist. But at MIT, at Harvard, at Yale, they were teaching the modern state of physical science.

HEITMANN: Johns Hopkins had some good teachers.

WARNER: Hopkins, and Berkeley, of course, Caltech, and Chicago. There the students came away with some understanding of the modern state of physical science. This was not so at Indiana. Well, 1923 was the same year in which [Peter Joseph William] Debye and [Erich] Hückel published their new theory of electrolytes (2).

HEITMANN: Exactly.

WARNER: By the way, 1923 was also the year that the first unit-process chemical engineering book by [Warren K.] Lewis, [William H.] Walker, and [William H.] McAdams was published (3).

HEITMANN: A very important book.

WARNER: In a way, the obsolescence of Indiana University can be excused by saying that these things were not common property. By that time, however, the professors in the good schools were already aware of this in the literature of chemistry. When I came to Carnegie Tech, I had to spend my first six or seven years just studying advanced science and mathematics in order to learn the modern state of physical science, and to decide on fields in which it seemed possible to do important research.

HEITMANN: I noticed that you did post-doctoral work.

WARNER: That was part of my training. I went to Michigan one summer just to study classical theoretical physics. I studied classical analytical dynamics with [David Mathias] Dennison and also band spectra. I also learned the technique of doing infrared spectral work. With a very bright graduate student and the help of a good instrument maker, we built the first infrared spectrometer in Pittsburgh.

HEITMANN: Then it really took some time for you to make up that lost ground.

WARNER: Six or seven years.

HEITMANN: What was O. W. Brown like as a professor? Did he supervise your work very well? Did he work closely with you?

WARNER: He would always come and have some suggestions, if I had a problem. He encouraged me to go ahead and do it myself.

HEITMANN: Were there any other graduate students whom you can remember who later did pretty well in chemistry?

WARNER: There were a few. There weren't a great number of graduate students in those days. Clyde [O.] Henke was probably one of the prominent ones. He was one of Brown's students. He spent his whole career at DuPont. He did very pragmatic catalytic work.

HEITMANN: He worked at the experiment station at Wilmington?

WARNER: I don't know whether he was moved out to some of DuPont's divisions or not, but as far as I know he spent his whole career there. He's dead now. He was a bear for hard work and long hours. He was bright enough. He was Phi Beta Kappa, I guess. So was I. I didn't work very hard for it. Another man who had a very good career was Herman [T.] Briscoe. Herman was an undergraduate at Indiana. I guess he got his master's degree at Harvard and then he got married. He needed some more income and taught chemistry at Colby, for a couple of years. He then came back to Indiana and got a doctorate with [Frank C.] Mathers. Briscoe was a very bright guy and learned an awful lot of chemistry on his own. He also knew what was the matter with Indiana. He became head of the chemistry department and finally Vice President for Administration at Indiana. He and Herman B. Wells really changed the direction of Indiana University.

HEITMANN: Was there a large agricultural school at Indiana at the time?

WARNER: No, that's at Purdue. Purdue is the A & M school in Indiana. There's no engineering and no agriculture at Bloomington.

HEITMANN: Right.

WARNER: There was a graduate student by the name of Brindage. I can't remember his first name, but he later became interested in pharmacological chemistry. His career was down in Texas. I don't know whether it was Texas A & M or the University of Texas, but it had a department of pharmaceutical chemistry. I think he made himself quite a reputation. Verling [Milton] Votaw was another one who became very successful. He went to Procter and Gamble, where I think he became a director of research.

HEITMANN: So, there actually were a number of graduate students who were at Indiana during that period who went out to do well.

WARNER: Another one was Robert Frye. He had the research lab right next to mine, in physical chemistry. Bob Frye was a reserve officer in chemical warfare. During World War II

he was a colonel in charge of chemical warfare in the South Pacific. He became director of research at the arsenal up in Jersey. He married a Bloomington girl that we knew. We used to stop and see them on the way to vacation up in New England. Bob is also dead now. There was another chap by the name of Carrick. I don't remember his first name. None of these people did a graduate thesis of very great importance. Carrick did just a phase diagram for a ternary system. He had a pretty good career at Southern Illinois University. I think that at the time he went there it was really a teacher's college. He was there for his whole career, and I think he became head of chemistry. Another one was Lee Smith. He's not the organic chemist at Minnesota. He was put in industry for a while and then came back and got his doctorate in organic chemistry. He then went to work for the Eastern Laboratory in the Department of Agriculture. He was a queer duck. I don't think he lasted very long in industry. That was the reason he came back. But a queer guy can last quite a long while in a government laboratory if he keeps his nose clean.

HEITMANN: Throughout your graduate career in Indiana, you worked at various times. For instance, you worked at the Barrett Company in Philadelphia for a while.

WARNER: That was interesting and a good experience. I went to work for Barrett at the end of my junior year in college. J. Bennett Hill was a Penn Ph.D. who was director of research at the Barrett Company. I think he came through the Midwest hiring anybody who was warm and breathing.

HEITMANN: During World War I, I guess.

WARNER: World War I. He hired me when I had just finished my junior year. I went to work down there and had interesting work to do. There was a great demand for picric acid explosives. That's trinitrophenol, of course. The only phenol source in America was as a byproduct from coke ovens. That was just a drop in the bucket, so there was great pressure for a synthetic process to make phenol. The Dennis-Bull process had been developed on a laboratory scale at Cornell. [Louis M.] Dennis was a Cornell professor and [Robert W.] Bull was a director of research, I think. He became a superintendent at a synthetic phenol plant. When I first went there, I was assigned to the group that was doing the pilot plant work on the synthetic phenol process. Originally, we used solid caustic soda, knocked out the sulfide group, and it went right to sodium phenolate. Later we learned we could do it with just the concentrated sodium hydroxide solution in autoclaves. I was a turn chemist, taking one shift while we were in the pilot plant stage, and getting the regular plant in operation.

HEITMANN: It must have been a great experience for you.

WARNER: It was a great experience. (That process was replaced after World War I by direct chlorination of benzene followed by hydrolysis of the product.) After that plant was in operation I worked again as shift chemist, separating meta-xylene from para- and ortho-xylene. Nitrated meta-xylene was the best stuff for anti-submarine bombs. Its boiling point was right in the middle of the other isomers. In those days, fractionating columns were not what they are today. It took an extremely careful regulation of temperature on every plate in the column. I was on that job until they found out exactly how to operate the distillation columns. Then I started working on the organic bases, the pyridines and the quinolines and so on. I was sent out to Pittsburgh. As a matter of fact, I was loaned to Major Dodge. He was a Major in the Ordnance Department, and they were buying ammonium sulfate. The first big by-product coke oven plant was out here in Clairton, Pennsylvania. The government was buying the ammonium sulfate to make ammonium nitrate. They were getting too much pyridine carried over into the ammonium sulfate, so I was sent out here with Major Dodge to be his flunky in making a pyridine balance on the coke-oven plant.

HEITMANN: What a great learning experience!

WARNER: After the war was over I came back to Indiana. Indiana had gone on a term system during the war, so that they started a second term in January. I worked for some good people at Barrett. J. Bennett Hill was director of research.

HEITMANN: He may have been an Edgar Fahs Smith student.

WARNER: I don't know, but J. Bennett Hill was director of research. Dusty Rhodes was the head of the section of the real research. Hill had all the science and technology.

HEITMANN: Hill was a technical superintendent.

WARNER: Something like that. Anyway, Rhodes was really the head of most of the research that I'm talking about. My direct boss, who was head of a division or section under Rhodes, was Pete [Peter Andrew] Van der Meulen. He was my direct boss. When I went in to tell Bennett Hill that I was going back to school, he said, "Well, I understand why you would want to do that, but I want you to know you don't have to go back there. We'd like to have you stay around here. You're the only guy who goes up the steps two at a time." After the war, Bennett Hill became director of research for Atlantic Refining, and then Atlantic Richfield. He has kept in touch with me all these years. At ACS [American Chemical Society] meetings he'd always look me up and find out how everything was going. When I was promoted in some way, made head of the chemistry department or something like that, I would get a letter from Bennett Hill. After the war, Dusty Rhodes became head of chemical engineering at Cornell. There was

another chap who was on leave there who was chemistry professor at Penn. I can't remember his name.

HEITMANN: Hiram [Stanhope] Lukens?

WARNER: Hiram Lukens. Yes, that's right.

HEITMANN: Yes, he became chairman of the chemistry department in the 1920s. He had a very long and successful career at Penn, lasting on into the 1940s.

WARNER: He was at Barrett during the war. I've forgotten what he was working on. Another interesting guy that was there was Harold [Clayton] Urey.

HEITMANN: He went to Hopkins in 1924.

WARNER: That's before he did graduate work at Berkeley. He was a graduate from Montana State.

HEITMANN: He was at Barrett as a kind of a rookie chemical engineer.

WARNER: I thought he was doing pretty uninteresting work compared to what I had to do. I think he was working on getting resins polymerized and out of solvent naphtha. I guess that was the first step before we started separating the xylenes.

HEITMANN: So those were really exciting times. It must have made Indiana even easier when you returned because you learned so much on the job.

WARNER: It was a valuable experience for me. I went back and finished a year's work in two terms.

HEITMANN: You also worked for a time for Cosden Oil Company.

WARNER: Yes, although that wasn't nearly as interesting or good an experience. Dr. [Charles

Kenworthy] Francis was director of research at Cosden. At that time he visited the Midwestern universities looking for people. He offered me a job right after I got my master's degree. I think they misled me on how interesting it would be. I didn't really enjoy it very much. I did do some interesting stuff. Do you remember the Ellis patents?

HEITMANN: Ellis, yes.

WARNER: Ellis made all kinds of things from unsaturated hydrocarbons.

HEITMANN: Making glycols.

WARNER: Well, they had me making isopropyl ether out of propylene. Then they had me study the blending of gasolines, and the blending of petroleum fractions and lubricating oils. If you were doing that in a careful scientific way, studying the partial molal volumes of all these constituents, why then it would have been an interesting sort of thing. But this was just a very pragmatic sort of thing. You mix so many CCs of one with so many CCs of another.

HEITMANN: Did you really use your book-knowledge much at Cosden? I guess you'd say, "No" since there was a lot of pretty practical work.

WARNER: A chemical engineer and a mechanical engineer from Cornell were hired on that same tour. The chemical engineer was Sherwood and the mechanical engineer was Malcolm Tuttle. We became good friends. The three of us all lived at the YMCA. We were all unmarried, of course. We had a terrific experience. We were there when they had that big racial riot.

HEITMANN: What do you remember about that?

WARNER: I remember that the three of us often went out for a walk in the evening and then went to dinner together. Well, we walked by the jail and we saw this great crowd around the jail. As we were watching this crowd, a car came loaded with black people from the wrong side of the tracks. Tulsa was on the other side of the Santa Fe tracks and that was "nigger town." A load of them came in a car and they fired guns right into this crowd. Well, the crowd evaporated at once. But in just about that much more time they were all back with guns.

HEITMANN: You were in the middle of this.

WARNER: We scooted for the YMCA as fast as we could. We got up on the roof of the YMCA and watched the battle. It was back and forth all the way down the Santa Fe track. Finally, some of the rabid white folks broke into the armory and put machine guns on the back of a truck. They went over around “nigger town.” There was a bluff on one side of it. They got the truck up on that bluff and started just machine gunning the town and setting it on fire. Before morning the black folks were just leaving for the country. An interesting thing happened the next night when the white folks went out to the country to pick up these black people and bring them back. They put them in the fairground pavilions and took care of them. These were the same guys who had been shooting at them the night before.

HEITMANN: Why do you think they would want to do that?

WARNER: Well, this started because the guy who was in jail, and who this crowd was trying to get out of jail to lynch, was a black man who had stopped an elevator between floors and tried to rape a white woman.

HEITMANN: Well, that’s one of the great causes for such things.

WARNER: Well, it was an experience, anyway.

HEITMANN: You added to your life experience in a lot of ways in Oklahoma.

WARNER: At the time, Cosden Oil Company had one of the largest oil refineries in the United States. It had the largest battery of cracking stills of any refinery, and they used the old Burton thermo-cracking process. But, there again, that’s a very pragmatic thing. It just involved heating under pressure. They had drag chains in each of these stills to keep the tar that was formed from collecting on the bottom and getting hot spots. It was very primitive compared to the modern day catalytic cracking.

HEITMANN: I’ve seen a picture of the old Burton stills.

WARNER: Well, Sherwood became superintendent of the cracking stills. He was the chemical engineer that I mentioned. Between Dr. Francis and myself there was a so-called chief chemist. It turns out that this “chief chemist” studied chemistry by taking an international correspondence school course.

HEITMANN: Well, you were really at the beginning of a whole new era of chemistry, where the college graduate was replacing this nebulous kind of figure. After about a year, you returned to Indiana and completed your graduate studies.

WARNER: A little less than a year. I went back as an instructor in chemistry. I was then very independent financially. In addition to teaching qualitative analysis, I taught a course in organic chemistry for the home economics students. I also taught a physical chemistry laboratory.

HEITMANN: After you graduated in 1924 you worked for a couple of years in Fort Wayne?

WARNER: I would just as soon forget about that.

HEITMANN: Okay, we'll just pass that by.

WARNER: I was doing consulting for a little company owned by Paul McNutt's father-in-law in Bloomington. They made polishing waxes of all kinds, and various other things. So, I knew that business. At that time I wanted to get married. Lolly Carter and a fellow named Updike, both from the business school, said, "You know this business. You can make these polishing waxes and develop new products. Let's also get some business as manufacturer's agents, and we will be the salesmen." These guys were either completely lazy or not capable of selling anything. I found myself being a salesman more than a research man or a manufacturer. They thought that we could make money fast.

HEITMANN: One of these get-rich-quick schemes.

WARNER: Get married in a hurry.

HEITMANN: In 1926 you came to Carnegie Mellon. How did you get that job?

WARNER: I was married already. My wife graduated from Indiana in 1923.

HEITMANN: Is that where you met her, at Indiana?

WARNER: Yes. As a matter of fact, she was a student in my qualitative analysis course, just before I went to the Cosden Company. That was when I first knew her. I never courted her then. After I came back we started courting. She had been teaching high school for two years.

HEITMANN: She was Louise Hamer.

WARNER: Louise Hamer, yes. My older son's middle name is Hamer. She was very bright. She was a Phi Beta Kappa, and I got to tell her when she had been elected to Phi Beta Kappa. The old professor knew we were courting and he called me up and said, "Wouldn't you like to tell her?" Louise said, "I've saved all the money I made. I lived at home and taught high school. If you want to get out of this business..." I had dropped three thousand dollars on this thing. She said, "Let's get married anyway. We'll get along." We decided that I wanted to get back into academic work. After we were married, I made the final decision. I wrote to fifteen or twenty schools. I got offers of employment that were essentially identical from three places: the University of West Virginia, Georgia Tech, and Carnegie Tech. I visited West Virginia and Carnegie Tech. I knew more about Georgia Tech, but I decided that Carnegie Tech had the best future.

HEITMANN: I guess you interviewed at Carnegie Tech. Whom do you remember there?

WARNER: Well, chemistry and chemical engineering were together in one department at that time. The head of the combined department was Joseph Hidy James, a Ph.D. from Penn. He took his doctorate in chemistry. He was an industrial chemist, but he called himself a chemical engineer. The early chemical engineers were all industrial chemists. They had taken their degrees in chemistry. One very attractive, bright member of the faculty at that time was Harry Seltz, a Penn Ph.D. who was one of Herbert Harned's students. He was really a red-hot teacher of chemical thermodynamics and a real good chemist. He and I were the two young Turks in the chemical engineering department. Harry went there a year before I did. We became very close friends. We made that chemistry department. At the same time, this young Turk invasion started in physics, mathematics, and in some of the engineering fields, particularly electrical engineering. We were all of a mind about what Carnegie Tech needed to do.

HEITMANN: What did you think it needed?

WARNER: It needed to become a graduate research university.

HEITMANN: Up to then it was pretty much an undergraduate technical school.

WARNER: That's right. They had good students. It wasn't an easy place. I think some of the difficulty was due to the fact that you had to do so much of the same thing—twenty problems in mechanics that didn't involve much difference in principle.

HEITMANN: It was variations on a theme.

WARNER: Yes, you can make things hard by using a lot of time.

HEITMANN: There was plenty of money at Carnegie Tech in the 1920s, wasn't there?

WARNER: Well, for that time it was pretty well endowed. I don't remember how much endowment it had but they didn't take advantage of it. One problem that Carnegie Tech had in those days was that everyone in the Pittsburgh community thought Carnegie Tech was rich as hell—that Mr. Carnegie had left it so damn much endowment. Well, that appeared to make it hard to raise money for the place. The first president of Carnegie Tech was [Arthur A.] Hamerschlag. Carnegie began as Carnegie Technical Schools, a division of Carnegie Institute. Hamerschlag had been head of a technical high school in New York. He was chairman of a committee that Carnegie appointed to advise him about what kind of place he should start. In Mr. Carnegie's mind, the goal at first was to establish a place to train people for the steel industry.

HEITMANN: To really supply his own industry.

WARNER: The Carnegie Technical Schools program was just a three-year certificate program. From 1900 until 1912, there was a great emphasis on shops. They were training machinists and foundry men, and taught pattern-making, engineering drawing, printing, and other practical things. It didn't become a regular four-year college until 1912. In 1912 it was incorporated separately from Carnegie Institute. Carnegie Institute of Technology offered a four-year program that led to the baccalaureate degree. It contained a school of engineering and science, a fine arts college, and the Margaret Morrison College, which was supposed to be a professional college for women. The latter had home economics, secretarial studies, and even another science department.

HEITMANN: Carnegie was certainly not a graduate school. There wasn't an emphasis on Ph.D. training.

WARNER: Oh, no.

HEITMANN: Until really you got there?

WARNER: There were some master's programs. But a fellow by the name of Baker followed Hamerschlag. Thomas Baker was an interesting guy. He became president in 1923, and was president the first ten years I was there. He had been professor of Germanic languages at Hopkins and a music critic for *The Baltimore Sun*. He had been weaned away from that to be headmaster of the Tome School for Boys in Eastern Pennsylvania. I don't think it's in business anymore. Anyway, Hamerschlag had two sons, and one of them was a wildcat. They had a lot of trouble with him. Well, he got him into Tome School and Tommy Baker made a man out of him.

HEITMANN: Now I see the connection.

WARNER: So, Hamerschlag brought Baker to Carnegie Tech as secretary of the Institute of Technology. Carnegie died in 1919. Up until that time Hamerschlag had a free hand. If he wanted to build another building, he would go ahead and build it. Then he'd go down to New York, or to Skibo Castle in Scotland, wherever Carnegie was, and Carnegie would write him a check.

HEITMANN: He was really a very powerful man.

WARNER: Yes.

HEITMANN: He didn't wait for anybody to tell him what to do.

WARNER: No. Well, after Carnegie died, the Carnegie Corporation got hold of all the Carnegie money. The Carnegie Corporation said, "Now boy, you don't do that any more. You come and make a proposal to us like any other university, you see." Well, Hamerschlag didn't like that way of operating and let them know it, so they kicked him upstairs. At that time they had gotten the Cottrell patents, and they made Hamerschlag president of the Research Corporation.

HEITMANN: The precipitators?

WARNER: That's right. Research Corporation of America was an organized non-profit corporation. I guess that the Carnegie Corporation owned it. They made Hamerschlag the head of the Research Corporation, and he went to New York. They had a trustees committee looking for a new president. They never got anyplace and finally they made Baker, who had been made acting president, president by default. He didn't know anything about science and technology.

HEITMANN: Germanic languages, yes.

WARNER: Well, he was a scholar and he would go to Europe every summer and give lectures in Germany, where he was very popular. As a matter of fact, when they started being tough on the Jews, he brought some of them here. In 1933 he brought Otto Stern and Immanuel Estermann to Carnegie Tech. Estermann was Stern's assistant. He also brought [Ernst] Berl, an industrial chemist, over from Germany in 1933.

HEITMANN: I've actually seen some of Berl's papers.

WARNER: He has a son who got his doctorate in chemistry in our department. As far as I know, he's still at the Hopkins physics lab. I think for a time he was on leave and he was arranging the annual meetings of the AAAS [American Association for the Advancement of Science]. Walter, that was it, Walter Berl.

HEITMANN: That's his son. Walter Berl.

WARNER: Yes.

HEITMANN: When you got to Carnegie Tech, Baker was the president.

WARNER: He was president. I think he became president in 1923, or maybe in 1921 or 1922.

HEITMANN: He would clearly see the value of graduate education, since he had been at Hopkins.

WARNER: He had outside advisors. The chairman of his advisory board was a president at

that time. He told Baker, "If you want this place to amount to anything, you've got to get research going." I wish I could remember his name, but my memory is getting to be terrible. Anyway, as a result of this advice, and since the faculty was not of a quality to initiate advanced graduate work rapidly, they decided to start special laboratories that were separate from the faculty. They established a coal research laboratory and a metallurgical research laboratory, thinking that they were picking things that were important to the Pittsburgh area. They established a joint program in the physical chemistry of steel-making between Carnegie and the Bureau of Mines, which was right next door.

HEITMANN: How did this fit in with the Mellon Institute? Were they doing this kind of work?

WARNER: Yes, they were doing this kind of work, but not in these fields. The Mellon Institute was almost completely involved in chemistry and chemical engineering. It thrived on captive fellowships. Every industry that the Mellons had interest in, either owned or had a good bit of stock in, had to establish a fellowship at The Mellon Institute. That's what eventually led to the difficulties at Mellon Institute. Ed [Edward Ray] Weidlein didn't know what to do when he had to go out and get support for research. This is not for publication, of course. Ed thought he was a great guy.

HEITMANN: We can just "X" it out when the time comes.

WARNER: But Ed didn't know what to do when companies like Gulf and Koppers and U.S. Steel started their own research laboratories.

HEITMANN: All the great Pittsburgh companies.

WARNER: Well, they're research campuses today. These companies kept some work going at Mellon Institute for a while. But there was a great decrease in the amount, and that's what killed Mellon Institute. Ed had about three or four blue ribbon committees with people like Roger Adams to tell him what to do. They never came up with anything more than to suggest that the Mellons ought to throw in more endowment and let Mellon Institute just do basic research.

HEITMANN: Any graduate research conducted at Carnegie Tech would not in any way compete with what was taking place at Mellon Institute during that time.

WARNER: Mellon Institute is part of Carnegie Tech now, you know.

HEITMANN: Yes, I know. It is now, but back in those days it was separate.

WARNER: Well, it finally stopped listening to Ed Weidlein. Dick [Richard King] Mellon and Paul Mellon put me on the board of trustees of Mellon Institute.

HEITMANN: And they listened to you instead?

WARNER: They asked me to make a very confidential plan for the merging of the two. It happened two years after I had been president.

HEITMANN: Getting back to those early years at Carnegie Tech, it was really you and whom?

WARNER: Harry Seltz in chemistry.

HEITMANN: You were both really pushing for this graduate research project.

WARNER: Yes, that's right, and for several in physics. Then, you see, Stern comes along. Originally, Stern was set up in a separate molecular physics laboratory, reporting to the president, and not to the physics department. Those things were never straightened out until [Robert Ernest] Doherty came as president. But in physics we had [Jonas Bernard] Nathanson and Dutch Leiberknight, who were a couple of young Turks. Graduate students in chemistry, whether they were interested in organic chemistry or physical chemistry, had to take Harry's thermodynamics course, my theories and properties of solutions course, and the advanced organic course. Even physical chemistry majors had to take these three courses. We didn't like to have them highly specialized.

HEITMANN: So the first year or so was pretty much a general education period in which you used the classic texts. What were some of the classic texts that you used? Do you remember?

WARNER: You mean in the graduate work?

HEITMANN: Physical chemistry and the like?

WARNER: Well, of course, in theories and properties of solution there were none. I had to make the course. Harry had them use Lewis and Randall, of course, in thermodynamics. Harry also taught a graduate course in kinetics. I know he used [Cyril Norman] Hinshelwood (4), at one time. I'd have three or four kids go together, get a copy of books in the theories and properties of solutions: things like [Peter] Debye's *Polar Molecules* (5) and [Joel Henry] Hildebrand's *Solutions* (6). I also had [H.] Falkenhagen's *Electrolytes*.

HEITMANN: Falkenhagen?

WARNER: Falkenhagen on modern solution theory (7) and Hückel on conductivity were other books. I had extra copies of a number of these books and if students had time to come to my outer office, they could read them.

HEITMANN: Where did you find funds for these graduate students? Did they teach?

WARNER: They were primarily teaching assistants in the early days. There were never a very great number of graduate students before World War II. It was just gradually building up when the war interfered. During my first ten years at Carnegie, Baker was President. Baker and I became quite good friends. He found out that Harry and I were intent on doing research and publishing. When we published a paper we would always send a copy over to the president's office. We would always get a lovely note back from Baker, and he showed by what he did that he agreed with us that we had to build the faculty and get ordinary graduate work going if we were going to be a center of learning. The separate laboratories that had no connection with the teaching departments were just never going to do the job. They became less powerful because people started learning that what the young Turks were advocating was generally being supported by Baker. So we became power centers instead. When Doherty came, it really changed things.

HEITMANN: What was Doherty's background?

WARNER: Doherty was an electrical engineer from Illinois who went to work for GE as [Charles P.] Steinmetz's assistant. At GE he developed an advanced training program for newly hired engineers, and it was really something. It was a real tough program that had basic and advanced mathematics, classical physics, and an analytical approach to all kinds of engineering problems. It was not just filling in a formula, or something like that. Instead, you really looked at what principles were involved and decided what the best route for solution was. After you got a solution or various solutions, you then decided which one was the best solution. Doherty

would emphasize, for example, the importance of defining a problem. He would say, "If you can't define the problem, you sure as hell can't solve it."

HEITMANN: It's very true, too.

WARNER: It is true. So, after you've gotten the solution, or several solutions, you ought to consider everything and make up your mind, which is the best solution. Not just the science, but also the economics and the effect on society. Today, you would probably say environmental consequences. Which is the best solution, considering everything? Then, after you've done that, you sit back and say, "Now, what did I learn from this experience?" He was a great guy, a great educational leader.

HEITMANN: He was actually your predecessor?

WARNER: That's right, he came in 1936.

HEITMANN: He retired in 1949?

WARNER: Yes, 1950.

HEITMANN: He had a powerful influence on the way education developed.

WARNER: He had a very great influence on developing effective, excellent undergraduate teaching and graduate work that was really significant, and was not just going through the motions.

HEITMANN: In the 1930s you began to publish some pretty good studies essentially on salt effects, and solubilities.

WARNER: That interest grew out of my course on theories and properties of solution.

HEITMANN: It was essentially one of your basic research programs that you carried out with graduate students during the period. Do you recall any of your graduate students? Certainly you had a big impact on the lives of others.

WARNER: Before World War II, and before we had any doctoral candidates, I had a number of people who did master's theses with me. One of them was [James] Paul Fugassi. He did a master's with me in kinetics and I sent him on to Farrington Daniels at Wisconsin. His doctorate was with Daniels in gas kinetics. After obtaining his doctorate he was brought back on the Carnegie faculty. He had a good career there. He was dedicated to gaseous kinetics research. He was a wonderful and very clever experimenter. He had quite a number of doctoral students. Another one was a high school chemistry teacher by the name of Lee. He was one of the best high school chemistry teachers in this district. He did several research jobs with me. They weren't that important. Although one of them was interesting. This occurred when I started teaching this course in solutions. I wondered whether I could pick three components that would make an almost ideal solution. The binary eutectics and the ternary eutectics would be essentially what you would calculate if they were all in ideal solution. We did the experiments, and found one that was pretty close. Let's see, who else? There were a couple more with master's theses. Fred [B.] Stitt.

HEITMANN: He published a paper with you (8).

WARNER: Yes. I sent him on to Caltech for his doctorate.

HEITMANN: Whom did he work with at Caltech?

WARNER: At Caltech? [Linus Carl] Pauling. He went to Harvard as a post-doctorate for a couple of years. Then he went to Indiana. Indiana didn't have enough sense to treat him like the guy he was so he left and went to the Western Regional Laboratory of the U.S. Department of Agriculture, where he mostly did molecular spectra.

HEITMANN: I know that you published a paper on the conversion of urea to ammonium cyanate (8).

WARNER: It's the old Wöhler reaction.

HEITMANN: You used the salt effect to determine whether it was a bimolecular reaction.

WARNER: And we studied the dielectric constant effect.

HEITMANN: Yes.

WARNER: I guess it's no use taking time to think of any others. Then doctoral students started coming along. My first doctoral student was Bill [William J.] Svirbely. You've noticed some early kinetics papers he was on (9). He joined the faculty of Maryland and became head of the physical chemistry division. He's retired now. Another doctoral student was Dave [David Scroggs] McKinney, who was a very bright guy. He had been out in industry but came back. He helped build that first infrared spectrometer. He continued to do molecular spectra. He got an extra grating instrument to go out beyond the rock salt range. Our first instrument had a rock salt prism. The light went through it twice, and it had resolution as good as you could get with a grating for the rock salt range. After I became president, I got Dave an automatic recording infrared spectrometer.

HEITMANN: It's grand when your dissertation supervisor becomes president.

WARNER: Well, Dave is retired now. In fact he is not in good health. He's in a nursing home not too far away. He could have been a very great internationally known physical chemist, because he could master almost any field he wanted to. But, he was a bachelor and lazy. He didn't have a wife he had to take care of.

HEITMANN: He didn't have the responsibility—or someone pushing him.

WARNER: He was kept on the faculty. He and Fugassi were the only two Carnegie Tech undergraduates who were kept on the faculty. Jim [James Elbert] Ablard was a University of Wisconsin undergraduate. We started exchange programs because we didn't want our good students to stay on at Carnegie Tech for a doctorate and we wanted to get good students from other good schools.

HEITMANN: One was with Wisconsin.

WARNER: Yes, I got Ablard in exchange for Fugassi.

HEITMANN: You traded personnel as though you were baseball executives or something like that.

WARNER: Ablard did his doctorate with me. His thesis was on dissociation constants over ranges of temperature of organic amine bases. He did another job, too. He was a very good experimenter and he spent a lot of time trying to determine the dielectric constant of weakly conducting solutions. We never found out how to do that. Jim worked very hard. He knew electron circuits, conductivity bridges, all that kind of stuff. We worked for quite a long while with Tony [Raymond T.] Gabler, one of the young Turks down in electrical engineering who was an expert in vacuum tube applications. He was very helpful to Jim and me. We had the idea that the trouble was with a leaky condenser that was losing voltage to ground all the time. He thought that between the condenser and ground we could devise a tube that would act as a negative resistance and it would make up for the loss. Well, it never worked out. We certainly spent a lot of time on it. All these things were going on up in my laboratory.

HEITMANN: There was quite a crossing of disciplines up in your laboratory.

WARNER: All that research was going on when Doherty came to visit to see whether he wanted to take the presidency. He was brought to my laboratory and they couldn't get him out to go anyplace else.

HEITMANN: That's great!

WARNER: Well, Ablard, Svirbely, McKinney, Sam Eagle, and [Leon Oscar] Winstrom were graduate students with me at that time.

HEITMANN: Eagle?

WARNER: Eagle, yes. I think he is one-fourth or one-eighth Indian. He was a graduate of Montana State. That's at Bozeman, isn't it?

HEITMANN: Yes.

WARNER: From the start, his family ran West Yellowstone every summer. Father and mother and all the kids moved down from Bozeman. They ran the post-office, lodge, filling station, and everything else up there in the way of enterprise. Sam did solution kinetics with me. After his doctorate he went to work right for Standard of California. He had a good career, he became a technical assistant to the president of Standard of California. Leon Winstrom was a Hope College undergraduate. That's a very good Dutch Reformed liberal arts college in Holland, Michigan. They had a very famous chemistry teacher up there. I think his name started with

“Z”. Anyway, we’ve had several very good students from there. Leon worked with me in solution kinetics and then immediately went to work for Allied Chemical. It later became Allied.

HEITMANN: Well, General became Allied after a while.

WARNER: Yes. General Chemical, in fact, became a constituent of Allied Chemical, isn’t that the way it went?

HEITMANN: Yes. General Chemical is a very old chemical company.

WARNER: Yes, that was a real heavy chemical company. Anyway, Leon worked his whole career with them in their pigments division. In fact, he became an expert on catalytic processes. After he retired from Allied, he had a consulting job with another chemical company for about five years. I think he’s still doing a little consulting work for them although he’s primarily retired. He had a good career.

HEITMANN: These are all graduate students you had during the 1930s then?

WARNER: They were all before World War II.

HEITMANN: Did you get any industrial funding for these people, or did you try to stay away from getting industrial funding?

WARNER: We had one or two. In chemistry we had a DuPont graduate fellowship and we had one from Gulf Oil, and I think we had three graduate fellowships. Those went to second or usually third year graduate students. They started out as teaching assistants and as research assistants.

HEITMANN: They were funded by these corporate fellowships later on. Were the students obliged to do a certain kind of a project related to the company?

WARNER: You mean an industrial project?

HEITMANN: Yes.

WARNER: No. We never took them. We wouldn't take any with any strings attached.

HEITMANN: So it was just whatever they wanted to do or whatever you wanted to do with them.

WARNER: That's right.

HEITMANN: Very good. I would think that would be the case with you because if you're interested in building up pure science, you don't want to get involved with these kinds of things.

WARNER: No, there is nothing purer than Carnegie Mellon University, on all these matters. If you're nice about it, you can usually explain things well enough to the industrial people, particularly the large corporations that have research labs of their own. The ones you have trouble with are the little dinky ones that think they can get their own research done cheaply.

HEITMANN: Essentially, they think they can buy you.

WARNER: Yes.

HEITMANN: Before we go on to World War II, there's another thing I'd like to ask you about. There's a paper you had written around 1941 (10). It concerns the education of chemists and chemical engineers and the distinction between the two disciplines. It was in that paper that you essentially called for a revision of the curricula to make sure that chemists and chemical engineers were distinguished. Within that text, there was also a plea for a healing of the wounds between the two professions. I don't know if you remember anything about that.

WARNER: I don't remember that, no. I have a copy of it here somewhere.

HEITMANN: I have a copy of it with me, but you say interesting things, so go ahead.

WARNER: Well, I think this was the Doherty influence. Was it in the 1940s?

HEITMANN: December 1941. It essentially deals with the curriculum for the professional training of chemists.

WARNER: Well, it's really a specialized interpretation of Doherty's scheme for education.

HEITMANN: Then that's where you really got your interest in it?

WARNER: You see, Doherty came in 1936.

HEITMANN: Now I remember. You were talking about how before the 1940s the chemical engineers had pretty concrete goals for their professional education and chemists never did. It was really the first time that someone stood up for the chemical profession and said that we need to have some guidelines now. And you say that it was Doherty's influence?

WARNER: Well, it's a Doherty doctrine, but applied to chemistry or physics.

HEITMANN: So that's where that came from.

WARNER: Of course, I was completely simpatico with Doherty on his ideas about education, and how to promote them. Generally the young Turks supported Doherty's ideas.

HEITMANN: When you began to argue this, that paper put you in the national scene in terms of chemical education.

WARNER: Well, there are some more papers on that. One is, "Is Chemical Education Too Specialized?" (11).

HEITMANN: Yes, I saw that one.

WARNER: There's another one that you ought to read to understand me and my ideas about what the university ought to be. I'll give you a copy of it if you don't have it. It's "Some Candid Comments" (12).

HEITMANN: Yes, I think I have that.

WARNER: On the last page of that document I make a plea for what the university ought to be for and what it ought to be like.

HEITMANN: We may get back to that later on when we talk about that chronological plan as president. Getting back to the 1930s again, you had an interest in research and education. You had more and more influence as president of Carnegie-Mellon and a sense of where things were going.

WARNER: I was not president yet.

HEITMANN: No, no you were not, but you were very close to Doherty in the sense that you're very sympathetic to his ideas. During the 1930s, what other chemists in other universities were you close to, either in correspondence or in going to meetings? When you mentioned Farrington Daniels, I assumed that would be one person, but who were some of the others?

WARNER: In those days I was active in the ACS nationally and I attended almost all ACS meetings. So, if I had some common interests with someone I would almost always have a conversation with them. Farrington Daniels was famous for having discovered what they thought was a unimolecular reaction, the decomposition of N. Well, before he went to work with Daniels, Paul Fugassi and I thought we could find another one, and that it would be a racemization reaction. We picked on D-limonene, which is octahedral in form. We happened to have a very good polarimeter in the department. It was one of the more expensive pieces of equipment and it used an ultra-violet mercury-arc light. So we used that to study the loss of dextrorotary power going toward the racemic mixture.

HEITMANN: As it is changing.

WARNER: Well, we had to have a pretty high temperature to start anything for the reaction to occur. I'll tell you what a poor place Carnegie Tech was to do important research at that time. The power was turned off at eleven o'clock every night, until seven in the morning.

HEITMANN: You could get a good night's rest.

WARNER: Well, the reaction was so slow that we had to run it more than twenty-four hours. Well, we fixed up a thermostat, which was heated, of course, with boiling biphenyl at a constant pressure. How did we maintain the constant pressure? We had an aspirator on a water spigot that would pull the pressure down. Then it would cut off the suction line if it got down to the right pressure. We would heat the thing with gas underneath the thermostat. It was made out of pipefittings. We had to stay there, but we didn't have electric lights. We had a gas mantle we had found somewhere, so we would have gas light. We did have gas, of course, all night long. So Fugassi and I had to take turns on the night shift.

HEITMANN: Your wife must have not thought too much about that.

WARNER: Oh, she was a good wife.

HEITMANN: Very understanding to put up with that kind of research.

WARNER: She was especially good.

HEITMANN: We've talked about Farrington Daniels. Who were the other chemists?

WARNER: I knew [John] Warren Williams at Wisconsin quite well. I knew [Joseph H.] Matthews, who was head of the department in those days. I also knew Joe [Joseph O.] Hirschfelder.

HEITMANN: We're going to interview him.

WARNER: Joe's a damn good chemist. At Minnesota I knew [Izaak Maurits] Kolthoff and Lee [Irvin] Smith. I knew Lloyd Ryerson. My son bought his house. I knew [William] Lipscomb. He was at Minnesota before he went to Harvard. At Chicago, I knew Warren [Charles] Johnson, Thorfin [Rusten] Hogness and [Hermann Irving] Schlesinger.

HEITMANN: Did you know Morris [Selig] Kharasch?

WARNER: Yes, but not well. I guess I didn't know him until wartime.

HEITMANN: Did you know that Harkins claimed that he discovered something?

WARNER: Harkins was arguing that he had priority on new ideas that other people published.

HEITMANN: Apparently Kharasch was a sort of a difficult fellow, as well, from what I hear.

WARNER: He was pretty spigotty.

HEITMANN: Yes. Apparently [Carl S.] Marvel and Kharasch were not on great terms.

WARNER: At Ann Arbor [University of Michigan] I didn't know the chemists as well as I knew the physicists. But there was an electrochemist at Ann Arbor, [Alfred L.] Ferguson, that I knew quite well. He was active in the Electrochemical Society when I was. [Frank R.] Elder went there, but that was later. Do you know him?

HEITMANN: Elder? No, I don't.

WARNER: Well, he had been at Columbia, I think and went from Columbia to Ann Arbor.

HEITMANN: I've never heard of him before.

WARNER: I may not have the name quite right.

HEITMANN: Was there anyone of these people or anyone we haven't mentioned, who is particularly important to you.

WARNER: Yes. I haven't mentioned a big one. Another one who became a great friend of mine was Jack Kirkwood.

HEITMANN: Kirkwood?

WARNER: Kirkwood was a great theoretical chemist. He was very interested in solutions. In some of my work, I was interested in the properties of solutions of amino acids and I used lysine in order to increase dielectric constants. I got acquainted with Edwin Cohen, who was head of the physical chemistry laboratory at Harvard Medical School. He was very interested in these solution properties, as was George Scatchard over at MIT. Kirkwood was at MIT with Scatchard before he went to Cornell. Then he went from Cornell to Caltech and from Caltech back to Yale. He died much too young. During his later years at Yale he was getting at some very interesting problems about the theory of nerve action. You get a source of potential at the boundary between different dielectric constants. This was part of his theory of potential for nerve action. George Scatchard was probably the most influential of all these people. I could tell you a story about why. Now, let's see, who else? I knew [Theodore] Shedlovsky at the Rockefeller Institute at the time. He was a friend of mine, interested in these solution problems.

HEITMANN: What about George [Bogdan] Kistiakowsky?

WARNER: I knew Kistiakowsky very well during the war years. I could tell interesting stories about his fights with his first wife and how he picked out his second wife at Los Alamos.

HEITMANN: That's a story.

WARNER: I knew [Lars] Onsager and [Raymond Matthew] Fuoss. He was another old friend of mine and went to Brown from Yale. Fuoss was at Brown with Charlie [Charles August] Kraus. He's retired now. At Harvard I knew Paul Bartlett, a physical organic chemist.

HEITMANN: Did you have correspondence with some of these people?

WARNER: Yes, a great deal, and I saw them at Academy [National Academy of Sciences] meetings as well as ACS meetings.

HEITMANN: Why was George Scatchard such an influential person?

WARNER: Well, I'll tell you. At Columbia I knew Vic [Victor Kuhn] LaMer very well. Urey was at Columbia in those early days before World War II. He was at Columbia even during the war on the uranium project. I also knew Louis [Planck] Hammett. He was another good friend of mine at Columbia. At Princeton I was a good friend of [Henry] Eyring while he was there, before he went back to Utah.

HEITMANN: Did you know Hugh Taylor?

WARNER: I knew Hugh Taylor as well as Eyring. I knew the Smyth brothers that were there, one in physics and one in chemistry. The one in chemistry was a great dipole moment investigator. I knew the fellow who ran analytical chemistry at Princeton for so many years.

HEITMANN: Nathaniel H. Furman?

WARNER: Furman, yes. I knew the Penn crowd quite well, by the way. You know the general chemistry by [Thomas Potter] McCutcheon, [Harry] Seltz, and Warner (13)?

HEITMANN: Yes.

WARNER: I knew Tom McCutcheon very well. I also knew Charlie [Charles] Price, the organic chemist, very well.

HEITMANN: He is very active in the Center for History of Chemistry. Do you know Claude [Knauss] Deischer by any chance?

WARNER: No, I don't. I knew [Herbert Spencer] Harned when he was at Penn, and he later went to Yale. Of course Harry Seltz had a Ph.D. from there. The head of my department, Joseph James, received a doctorate from Penn, but I expect he's from back in Edgar Fahs Smith's days.

HEITMANN: He may have been a Smith student.

WARNER: I expect he was. Also, [Martin] Kilpatrick was at Penn and then went to Illinois Tech. You know Martin and his wife Mary? We used to say that Mary made 'em and Martin shot 'em.

Now, I had started to talk about Scatchard. Well, when I did this first job on the effect of dielectric constants and ionic strength on activation energies, LaMer in this country and [E. A.] Moelwyn-Hughes at Oxford had been assuming that you could treat activation energies in solution reactions in the same way that you did in gas reactions. That is, just get it from the variation of the rate constant with temperature. If you look at activation energy that way, then

in solution reactions you have a variation of activation energy with temperature. Out of this Vic LaMer got a very fancy idea that there's an entropy of activation just as there is in total energy. He developed quite a fancy idea of calculating activation entropies. Well, we found that all he was getting was another effect on rate constants other than temperature. That is the change in dielectric constants of the medium as you change the temperature. The dielectric constant decreases, you see. So, we claimed that if you wanted something to compare with gas activation energies, you should clear out everything except the effect of temperature on solution reactions. I had one of these papers on the New York ACS meeting program and had already submitted it to the journal for publication. I never knew George Scatchard before, but out of the clear sky George writes me a letter: "Dear Dr. Warner: I know it isn't the usual thing for reviewers of papers to do, to reveal themselves to the author, but you have a paper on the New York meeting schedule that I have received for review for the journal. I want to tell you, Dr. Warner, that you're not taking a bunch of violets to the New York meeting, you're taking a bombshell." And he also said, "But, I want you to know that I'll be in the front row to defend you." That started the Scatchard-Warner friendship. He came to speak about my work when I got the Pittsburgh ACS award.

HEITMANN: Yes, he was a very influential figure in physical chemistry, writing and corresponding alike. Very interesting. You could say that during the 1930s there was a pretty tight group of physical chemists that were working on various problems, writing to each other, seeing each other at meetings, and trading students. I've heard organic chemists talking about their hostility to physical chemists within departments.

WARNER: Oh, I think there's a lot of feuding about, but there was never any at Carnegie Tech.

HEITMANN: Never at your institution?

WARNER: We never let things get to the state where they were almost like different departments within a department. No. There were a lot of places where the organic chemistry doctorates never took a course in physical chemistry. They don't know anything about thermodynamics. They really didn't know anything about gas kinetics.

HEITMANN: There was resentment by organic chemists who felt that physical chemists didn't have to teach as much in the department but got equal treatment.

WARNER: I don't know. I'd be greatly surprised if that were true. At Carnegie there was a sensible sort of look at what a guy was doing. If you had a lot of graduate students, you weren't asked to teach as many undergraduate courses. You weren't asked to teach as many hours a week. We more or less counted every doctorate and every master's student who worked with

someone as part of his teaching load. This applied all across the department. Graduate courses were valued at twice or one and a half times the value of undergraduate courses. But, a member of the faculty, of the old guard, that never had graduate students and never taught graduate courses taught twelve hours a week.

HEITMANN: During the 1930s, as you were getting your research program going at Carnegie, did you get a lot of visiting speakers coming through Pittsburgh? And did these people ever make appearances at Carnegie Tech?

WARNER: Oh, quite a few.

HEITMANN: Can you think of any memorable speakers who either influenced you or influenced any of the other faculty or students?

WARNER: I don't remember anyone who was startling. We had Charlie Kraus. We had [Peter] Debye when he was at Cornell. We had Warren Williams from Wisconsin. I guess we had Daniels. We never had Scatchard as a visiting lecturer. We never had Harned, although Harry Seltz kept in touch with Harned all his life. Harry, by the way, died much too young. Harry died when he was only fifty-five years old.

HEITMANN: Oh, yes, that's very young. I think we have a pretty good view of chemistry in the 1930s. Let's shift gears now to World War II.

WARNER: I'll tell you what happened up to World War II at Carnegie Tech. It takes a hell of a lot of energy, interest, and enthusiasm to get a place going. Seltz and I ran a graduate seminar that met on Saturday morning. Now, where in the hell will you get faculty to come out Saturday morning in most big universities?

HEITMANN: Not today.

WARNER: You don't. Where in hell do you get a professor that will do what I did? I bundled up my family, went to Ann Arbor, and studied theoretical physics for the summer at my own expense. Where in hell do you find that any more? They want a big grant.

HEITMANN: Well, I think in some ways, people don't take as much initiative as they used to. There are too many things that can diffuse a person's effort.

WARNER: We had the sort of enthusiasm among the graduate students and the young Turk faculty that you don't find in many places. The whole seminar would go for lunch down to Talberts in Oakland or then later, when it closed, to Foos, a German restaurant up on Mount Washington. We'd sit there, have lunch and drink beer.

HEITMANN: After the seminar?

WARNER: After the seminar was over, yes.

HEITMANN: It makes for a great department. What did the old-timers think of the young Turks?

WARNER: Well, some of them resented it. Some of them recognized the facts of life. One of them was Tom [Thomas Rush] Alexander, who was a Penn Ph.D., W. and J. [Washington and Jefferson College] undergraduate. Tom Alexander was graceful about it. He didn't know any more modern chemistry than he did when he first finished his doctorate. He could throw the best cocktail party of anybody in the department, and he was a great guy with the ladies. I don't think he ever published a paper. I don't even know what his doctorate thesis was. He was professor of inorganic chemistry. He had learned something about the transition metal complexes, the Werner coordination compounds. He knew a fair amount of descriptive inorganic chemistry, but he knew hardly any mathematics. He couldn't integrate dX over X .

HEITMANN: At Carnegie, after you got through the really descriptive chemistry, it was replaced by chemistry based on physics and mathematics and was much more quantitative and exact.

WARNER: That's right.

HEITMANN: And that really all took place before World War II under your leadership, and with Harry Seltz.

WARNER: You see, at that time, in addition to Seltz and myself, we also had McKinney and Fugassi, who were well-trained young Turks. We had bad luck with the two young organic chemists we hired in those days. On second thought, I guess one of them was hired before I was head of the chemistry department. I became head of chemistry in 1938, and hired a Bartlett

Ph.D. He wasn't my choice. My choice was Saul Winstein, who went to UCLA. He would have come to Carnegie Tech, but Webb [Webster] Jones, dean of engineering science, was really anti-Jewish. He didn't want me to hire Saul. Actually, Saul and I had a good bit of correspondence after he went to UCLA because I started doing kinetics on some reactions that he was very interested in. These were epoxy reactions, like ethylene chlorohydrin to ethylene oxide, and the whole series of those things. Kinetics was my area after the war. My students started working on a lot of interesting things.

I might say that Joseph Hidy James was anti-Jewish. The whole university was pretty anti-Jewish when I first went there. That was because they were preparing people primarily for industry, and industries were biased. DuPont or GE wouldn't hire a Jew as a chemist or a scientist.

HEITMANN: Well, you know Max Tishler only got one job offer in 1936.

WARNER: There you are.

HEITMANN: Melvin [Spencer] Newman had no jobs for about a year and a half. He was hanging on, just waiting for somebody to take him. They were pretty good chemists, too.

WARNER: Tishler was out of the ordinary.

HEITMANN: But it was only because George Merck, Jr. saw something in Tishler that he liked. Otherwise Tishler would never have been hired.

WARNER: I think it was because they found placing Jewish students difficult. But it really changed rapidly. Of course, as soon as I became head of the chemistry department, we made no distinction and started hiring independently. I would have hired Saul Winstein except, as I say, my dean kept me from doing so.

HEITMANN: I wonder what could have happened at Carnegie in terms of physical and organic chemistry.

WARNER: Oh, yes. You see, Jones was supposed to do that, but Jones had a lousy disposition. He couldn't get along with anybody. So we didn't encourage him to stay on. He went to industry someplace. I don't know whatever happened to him. He was the kind of person that would park any place that was restricted.

HEITMANN: Just to start a fight?

WARNER: He was nasty as the devil to any security guard who would call him on it. He'd be the same way with students and with faculty.

HEITMANN: There are people who are just like that.

WARNER: Just ornery.

HEITMANN: Now, I would like to get to World War II. You worked in the Manhattan Project?

WARNER: That's right.

HEITMANN: What did you do for the project?

WARNER: Before we get to that, I would like to say that after Doherty came and we started going places, I was made chairman of what they called the committee on graduate degrees. We got a new deal in physics when Fred Seitz came to Carnegie. He'd been on the physics faculty at Penn, you remember. He was a Princeton Ph.D. and one of our great guys in solid state physics. They brought him here from Penn as head of the physics department. With that Stern's separate laboratory for molecular physics became part of the physics department. The metal research lab became a part of the Department of Metallurgy. Robert [Franklin] Mehl was another young Turk who was brought to Carnegie. His last name is the same as one of my ancestors, but we are not related. He was a Franklin and Marshall graduate who had a degree in physical chemistry from Princeton. He was a National Research Fellow at Harvard. Then he went to work for the Naval Research Lab where he was responsible for developing a method for finding defects in castings by gamma radiation. That attracted so much attention that he went from there to be associate director of research at Armco. Doherty then brought him here as head of the metals research laboratory. The next year Mehl was made head of Metallurgy, and they became one. Well, Bob Mehl built a very good department. He had the same ideas that Seltz and I had. We were all good friends. The year that Doherty came here, Mehl invited me to come down to his faculty to start a program of research on the physical chemistry of metallurgical reactions. Two years later, they separated chemistry and chemical engineering into two departments. On my suggestion, we brought Warren [Lee] McCabe from Michigan as head of our department of Chemical Engineering. He came a couple of years before Doherty

did with the understanding that we would make him head of chemical engineering. It was all set. When the separation came in 1938, there I was down in Metallurgy. Doherty asked Jones, the dean of science and engineering, who was going to be head of chemistry. Doherty knew all about my research. We, of course, had kept in touch with his ideas about education, but Jones didn't seem to know. He said, "Why the hell did you let Jake Warner go down into Metallurgy if you needed a head of chemistry?" Well, Webb didn't quite understand what it was all about, but I was brought back in a hurry to be head of chemistry.

HEITMANN: Then McCabe became head of chemical engineering?

WARNER: Yes.

HEITMANN: What was McCabe like?

WARNER: He was a very pleasant fellow. I think he was a very good chemical engineer.

HEITMANN: Did he built a strong department?

WARNER: Yes, quite a good one. He had a wife who was quite demanding; I think she kept after him to take a better paying industrial job. He did finally leave Carnegie Tech and became Vice President for Research of Flintcote Company. He made more money, but he was unhappy. You know what he did? He went back to Brooklyn Polytech as dean of the faculty.

HEITMANN: I didn't know that. He's one of the really great chemical engineers, and wrote a textbook (14).

WARNER: He had brought in a number of good younger men. One chemical engineer from Wisconsin later became dean of engineering at Maryland. He brought a young one from Rochester. He brought Carl [Corydon] Monrad, a Michigan Ph.D. in chemical engineering who had gone to Standard of Indiana. Monrad became head of the department when McCabe left, and was there until his retirement. McCabe gave chemical engineering a big shot in the arm. Old Doc James retired right at that time, in 1938. That's what they'd planned, to separate the two departments when Doc James retired. McCabe is now retired and lives in Chapel Hill, North Carolina.

HEITMANN: I didn't know what had happened to him. Of course, everyone recognizes his

name as a very important figure. What did you do during World War II? What were the positions you held and what kind of work did you do?

WARNER: Well, initially we got involved in synthetic rubber research.

HEITMANN: At Carnegie?

WARNER: At Carnegie. Harry Seltz and [Guido H.] Stempel, organic chemists, and Fugassi took that over when I went on the Manhattan Project. I guess it was 1942 or 1943. I came back to Carnegie Tech in 1945. Anyway, I told you Fred Seitz was there as head of physics and I was head of chemistry. When the heat was put on me to go I made an appointment with Doherty to ask for leave. While I was waiting for my appointment, who comes out of Doherty's office but Seitz, who had just been there to ask for the same thing. So we left for the project and our offices were in Chicago. Mine was a peculiar organizational arrangement. The fellow who put heat on me was Charles [Allen] Thomas of Monsanto. Do you know that name?

HEITMANN: Yes.

WARNER: He died just recently.

HEITMANN: His son is in California.

WARNER: That's right.

HEITMANN: He wanted one of my friends to write a biography of Charlie Thomas. Anyway, Charlie Thomas was your boss in Chicago?

WARNER: Yes, on paper. My title was really Assistant Director under Thomas. Charlie was asked to be Director of the Manhattan Project program on the chemistry, metallurgy and purification of plutonium. On paper, I was his assistant. Charlie was more and more busy running Monsanto Chemical. He had moved from Dayton, where he was head of the Central Research Department, the old Thomas Hochwalt Laboratory. I had known Charlie quite well because we were associated in ACS business, and somehow we just felt comfortable together. Charlie was pretty good at understanding and getting along with university scientists. I guess I was pretty good at getting along with industrial people. Anyway, on this business of coordinating, the job was coordinating all the research on chemistry, metallurgy and purification

of plutonium. I guess on paper Charlie was the Director and I was the Assistant Director, but I did all the work. That was the way it turned out because Charlie had demands on him. At Monsanto, he became president and then chairman of the board. Charlie liked the arrangement. He liked me doing the work. He trusted me and thought I had the right answers. Now, what was the area, anyway? Well, work on this chemistry, metallurgy and purification of plutonium also involved some of the uranium chemistry, and particularly metallurgy. I was mostly asked to coordinate the locations where work was going on. You see, everybody wanted to do something spectacular. Then, they'd get interesting sidelines that had nothing to do with the project at all. It was just interesting chemistry.

HEITMANN: These were mostly academic people?

WARNER: Yes. Milton Burton, who later went to Notre Dame as head of the radiation laboratory there, headed the radiation chemistry division in Chicago. Well, I'm getting ahead of myself. The major work in chemistry was done by the great group that [Glenn T.] Seaborg headed in Chicago. Work was also going on at Ames, Iowa under Frank [Harold] Spedding. Some work was also going on at Oak Ridge, which was called Clinton Laboratories then. They had the pile built there, and it was supposed to give the usual pilot plant information for the design and building of both the pile and the chemical separation process at the Hanford job. They were all anxiously waiting for a time to kick some of the slugs out of that X-10 pile at Oak Ridge so that they could get hold of some plutonium. The other places were Los Alamos and Berkeley. There was a good bit of work going on at Berkeley. Most of Seaborg's people went to Chicago with him. I guess Seaborg would have preferred to have this whole thing in Berkeley, but the Manhattan Project didn't want that because a lot of the other work was already going on at Berkeley.

HEITMANN: Did you see [J. Robert] Oppenheimer?

WARNER: Oppenheimer was never there. He was at Berkeley, then at Los Alamos. Some work was at other places. For example, some of the work had to be done in ceramic crucibles because it was very hard to get materials that won't contaminate uranium or plutonium. The salt is reduced in a ceramic container, because one doesn't wish to introduce impurities, which capture neutrons and prevent a chain reaction from being maintained. So, once a month we had information meetings of the chemistry and metallurgy people. Most of them were held in Chicago. Of course, Seaborg's group was one of the largest groups in the bunch. Seaborg's group was really doing the microchemical work on the Hanford separation process. How are you going to separate and purify the plutonium that came out of these slugs? The plutonium had to be separated from the uranium, separate it from the fission products. That whole chemistry of plutonium was primarily worked out by Seaborg's group in Chicago. Of course, they were eager to check their microchemical work and to get the Hanford piles built. They really couldn't wait for the pilot pile there at Oak Ridge to kick out some slugs to give them

plutonium. They said, “We can’t wait that long to design this plant and start building it.” So the whole chemical separation plant at Hanford was designed and built on the basis of the chemical information that Seaborg’s crowd got on a microgram scale. That’s the damndest step-up known in history. No industry would take that chance by a long shot. Imagine the millions of dollars invested in a chemical separations plant with every operation by remote control.

HEITMANN: Had they failed, it would have taken a lot of time to retool.

WARNER: Oh, my God, yes. It was one of the biggest risk-taking events in history. And the damn thing worked. That was one of the miracles of the thing. The damn thing worked. It’s a great credit to Seaborg and his crowd. They did the work.

HEITMANN: Who worked with Seaborg on that project? Do you remember any of the other people who were working there in Chicago?

WARNER: Oh, yes, I knew most all of them in the chemistry division. Of course, Farrington Daniels was there for a time. I am trying to think of the section heads under Seaborg. Truman [Paul] Kohman was one of them. I brought him to Carnegie Tech and he’s still there. I guess he retires this year. There was a chap who was head of the section on microscale. He actually made some plutonium metal. He went to Caltech. His name starts with “N”. I’m trying to think of the section chiefs. One of them was with Seaborg at Berkeley. He came to Chicago and after the war went back to Berkeley. He has had a good reputation and I think he has become a member of the Academy. It’s a Jewish name. I haven’t done a very good job on Seaborg’s lieutenants. I knew them all at the time. People, particularly the chiefs, from these different locations came together at these information meetings.

HEITMANN: Not only the Chicago group, but also from Hanford?

WARNER: Yes, and from Ames, Iowa, from Berkeley, from Oak Ridge. We even had a refractory project at MIT under [Frederick Harwood] Norton, who was a professor of ceramics. I forget what the other sections or divisions outside of Seaborg’s group were called. One of them was the effect of radiation on materials. That was headed by Milton Burton. There was another section studying radiochemistry. That was headed by—I don’t know whether he’s dead by this time or not. I haven’t heard anything about him for a while. He went first to Oak Ridge and then joined the faculty at MIT. What was his name?

HEITMANN: Manson Benedict?

WARNER: No. Manson wasn't involved in our plutonium project. He was on the uranium project, wasn't he? On the other side.

HEITMANN: He may have been, I don't know that much about it.

WARNER: Some people on the Uranium 235 project were also at Oak Ridge. They developed a diffusion process for separation. I think Manson was in that crowd, which was headquartered at Columbia. We weren't supposed to exchange information any more than necessary between the plutonium project and the uranium project, the U-235 project.

HEITMANN: But you didn't really do any laboratory work, then?

WARNER: No. My job was to keep them from duplicating work, and to see that the work went along. For example, Milton Burton found out that under high radiation benzene would polymerize. He thought that that was an interesting chemical problem. He wanted to devote a lot of his section's effort to study high radiation causing polymerization. Well, I had to go to Milton and say, "Milton, dammit, you save that problem until after the war, when you're a professor. What you're supposed to do now is to tell us whether those Hanford piles will crumble or will stand up."

HEITMANN: You really had to ride herd on all these people.

WARNER: Yes. Well, Seaborg knew what the score was. He saw that his crowd was all going in the right direction. But when it came to things like refractories, we had work going on at Berkeley, at Ames, Iowa, and at MIT. And you had to keep hammering that they should take different classes of materials and not try to work on crystalline MgO or something like that.

HEITMANN: What you were trying to do was make sure the thing would work at the end.

WARNER: That's right. DuPont, of course, had the contract on building Hanford and the separation process. I had to handle problems like what's his name, who became president of DuPont—he takes those wonderful movies of hummingbirds. Crawford Greenwalt. A member of the Academy. Well, he and I had to figure out in what form plutonium, as a salt, should be sent to Los Alamos from Hanford. And the design of the vessels and all that kind of stuff. Those things I had to help decide. I had to write the agenda for these monthly meetings, also,

and write the report of the meeting.

HEITMANN: Really, that plant experience you got way back when came to a great deal of—

WARNER: Well, these things went unbelievably smoothly, really. Well, I expect Frank Spedding in Ames, Iowa caused me more headache than any other group. Frank would have liked to duplicate everything that was going on in Seaborg's group, you see. And they had done a marvelous job on the production of pure uranium. It was better than that of any of the industrial people that were trying to do it, like Westinghouse. The professors at Iowa State, under Spedding, they made all of the pure uranium to get that first chain reaction going under the Stands in Chicago and they kept it up until Nagasaki and Hiroshima. I think that essentially all the uranium that was used to make 235 was produced by those professors at Iowa State in temporary buildings. But then Spedding wanted to get into plutonium chemistry and metallurgy. Metallurgy of plutonium went almost entirely to Los Alamos. I think you got to the place where you're dealing with quantities. You had to have remote control on the thing and the two men at Los Alamos who were primarily responsible for the work there were Joe [Joseph] Kennedy, who was a graduate student with Seaborg and involved in the Seaborg-MacMillan discovery of plutonium, and Cyril [Stanley] Smith. I don't know whether the name means anything to you or not.

HEITMANN: Chicago.

WARNER: Smith became head of the Chicago Institute of Metals after the war. He later went to MIT.

HEITMANN: Yes, Cyril Stanley Smith.

WARNER: Cyril Stanley Smith, a Britisher. One of my jobs was to educate him on the project when he was brought on. He didn't know a thing about it. That was one of my jobs. He stopped in Chicago on his way to Los Alamos to take up his appointment there heading a metallurgy division at Los Alamos. It took two or three days for me to educate him on the project.

HEITMANN: You did a lot of travelling then, during those days. Crisscrossing the country, back and forth to the East coast.

WARNER: Well, the monthly meetings did take me to Los Alamos and Oak Ridge and to

Ames every once in a while, maybe once or twice a year to each place.

HEITMANN: Were you all kept pretty well informed on what the Germans were doing? Did you have a sense of urgency?

WARNER: Yes, I think we were oversold on it. The original evidence was that there was no question about it, they were interested, at first. The evidence was largely their great interest in heavy water from the electrolytic cells in Norway. That's one reason they went into Norway. They were using heavy water as a moderator in their piles, or they wanted to. But it never got very far. [Adolf] Hitler got very much enamoured with the idea of the rockets on Britain.

HEITMANN: The V2s.

WARNER: Yes, their scientists were pretty much diverted to the rocket business. I think our intelligence probably knew that there had been a shift in that, but they kept repeating the same story so that no one would throw the monkey wrench in the plutonium, well, the Manhattan Project. A lot of millions were going into it.

HEITMANN: What do you think about your years in the Manhattan Project? Did you get a lot of experience that you used later on when you became administrator and president of Carnegie-Mellon?

WARNER: I got a lot of administrative experience. Some of them were tough problems to handle. I learned to not put off a problem. Soon as you know it, dig into it. Get it over with. If you try to sweep it under the rug, it'll just get worse.

HEITMANN: And probably also, it means handling a lot of different kinds of people.

WARNER: Oh, yes, everyone from [Edward] Teller, to Eugene [Paul] Wigner, and many others.

HEITMANN: Debye?

WARNER: No, Debye wasn't there. I'm thinking of a theoretical physicist at Cornell. His name was Hans Bethe. He's a fine guy but awfully hard to understand. He had a terrific accent.

[Leo] Szilard from Chicago was another. Eugene Wigner is a dear man. He's a kind, thoughtful, extremely able, theoretical physicist. He's one of the kind of people you would like to be like because he can take a problem and by just reasoning mentally, you might say mental calculations, can come up with an answer of the right of magnitude. He's a remarkable fellow. In fact, such a gentle soul that he wouldn't hurt a mouse, I don't think. He's a dear friend once you know him. Married a professor at Wellesley. Mary Wigner. She's a very dear person, too. Made a wonderful wife for Eugene.

HEITMANN: Did you know Vannevar Bush during this period?

WARNER: I met him a few times. He wasn't much involved.

HEITMANN: With what you were doing?

WARNER: He was chairman of the chief advisors to the president. I think he would be the guy that could take General Groves to the woodshed. If anybody took him to the woodshed, it would be he.

HEITMANN: Do you remember your impressions after the bomb went off? Did you have a different sense of the potential of science? For some people it was quite a change. The way they viewed science really changed.

WARNER: Most of the good scientists on the project that I knew, people like Wigner and Seitz—but I don't think that's true of Teller—but I think even Seaborg and MacMillan, a few people like Cyril Smith and Joe Kennedy, people with important positions in this as well as on the uranium project, the ones I knew, an awful lot of them were strongly advocating that we not drop the bombs on Japan. A great majority of those people were urging that we invite the Japanese to a demonstration. If they don't come to the demonstration, or if they don't surrender when they see the demonstration, then go ahead and use it. Now that was a very general view of a great many of those scientists. I remember that right after the bombs had been used, and I guess after the Japanese had surrendered, I thought, "Gee, now I can go home." By that time Monsanto had taken over from the University of Chicago the Oak Ridge contract. Charlie Thomas pleaded with me to stay, come to St. Louis and act as liaison between the Monsanto brass and the Oak Ridge brass, at least for a few months to get the thing organized. So I did that. From about June of 1945.

HEITMANN: I think July and August, of 1945.

WARNER: It must have been July.

HEITMANN: VJ day was September of 1945.

WARNER: Well, that came after the Japanese surrender.

HEITMANN: Right, VJ day was the day they signed the document on the Missouri. I think they dropped it in July.

WARNER: Well, anyway, after that, Charlie Thomas was up at Chicago, one of his infrequent visits. I remember Charlie had a suite out in Park Plaza, or something like that, on the edge of that big park in St. Louis and he invited me to have dinner with him in St. Louis. That's when he put the bee on me about this contract. So of course then the news came out and everybody knew it before we went to dinner that night. So we celebrated. All the people who knew anything about it got together that evening and got a little high, I guess. So we had dinner and then Charlie put the heat on me for this job. That provided me with a two-motor Cessna airplane to shuttle back and forth between Oak Ridge and St. Louis. I lived in Missouri Athletic Club, had an air-conditioned office right next to Charlie's out at Monsanto headquarters. I spent my time between St. Louis and Oak Ridge.

HEITMANN: That sounds like a tough job in many ways. Commuting back and forth like that.

WARNER: Well, my wife and family weren't out in St. Louis at all. By October things had settled down and they were satisfied that we had straightened out administrative matters between Monsanto brass and the Oak Ridge brass, so that I was able to go back to Carnegie Tech in October. I hadn't more than started at Monsanto before Doherty was on my back about getting back. They gave me a nice boost in salary over what I had had before I left on leave. They wanted me to be of dean of graduate studies. So, I became dean of graduate studies as well as head of the chemistry department.

HEITMANN: You did both for about four years or so.

WARNER: Yes. Between then, 1945, and the time I became president.

HEITMANN: Did you continue to take graduate students then?

WARNER: Yes. That's when we did all this epoxy work. True acid-base level of solvents. That's not an easy concept. Louis Hammett has come about as close to being helpful in that as anybody, but we got some striking results in epoxy formation in relation to the true acid base level of the solvent.

HEITMANN: Getting back to your post-war years at Carnegie—you got back, you got a raise, and you got a new job, too.

WARNER: I might say, to interrupt a bit, on this matter you asked about what scientists felt about dropping a bomb. When I was living at the Missouri Athletic Club my brother DeMain wrote me, once it was public knowledge, "Now I know what you've been doing and tell me how you feel about this." I wrote him a letter expressing my views at that time. It was interesting that he sent me that letter maybe six months before he died.

HEITMANN: It's very interesting, yes. Do you remember any of it?

WARNER: Oh, I said that all of us were worrying whether this was going to be used to destroy civilization.

HEITMANN: So there was a real fear then.

WARNER: Yes.

HEITMANN: That was a concern, not a fear, but a concern.

WARNER: All right.

HEITMANN: Now, getting back to Carnegie just after the war, you were dean and department head, doing research and in administration, I guess there were a lot of veterans who went back to Carnegie after the war, too. Did you feel any different about Carnegie? Did Carnegie change a great deal in the years that you were gone? Was it a different university in some ways?

WARNER: It was a period of slumber almost. There was not much university activity except a

few research contracts. There was very little in the way of graduate work or undergraduate or graduate instruction. I think, altogether, sixteen members of the Carnegie faculty were on leave on the Manhattan Project. I don't know how many others were—some were all over Washington, I think. The head of mechanical engineering, for example, was developing this radar screen for ships at the Office of Naval Research. By the way, that graduate student of mine, Jim Ablard, worked for Kisty on OSRD8, which was the high explosives division out at Bruceton, right after he got his doctorate. He then went with Aerojet when OSRD wound up. And then from Aerojet, he became director of High Explosive Division of Naval Ordnance Lab. That's where he was the rest of his career.

HEITMANN: But Carnegie Tech was pretty much in slumber then.

WARNER: Well, there wasn't much going on.

HEITMANN: All of a sudden it gets started again.

WARNER: Doherty was anxious for us to regain the momentum we had created in graduate work and research during the war. I guess in every university, graduate work had almost disappeared. And there were very few undergraduate students. But there was still synthetic rubber research going on, and Seltz had a small contract from the Manhattan Project, determining solubility of noble gases in uranium and zirconium and other important metals. There were quite a lot of contracts going on in areas like mechanical engineering and particularly electrical engineering was humming with research contracts. All the way from solid state devices to magnetic amplifiers. They had a very ingenious head of electrical engineering, Rod Williams. Poor devil, died of cancer, much too young.

HEITMANN: Then you started getting graduate students again.

WARNER: That's right.

HEITMANN: And working on these epoxy problems.

WARNER: My own students started working in that area, but it wasn't exclusively that. There was some diester solution kinetics, which a graduate student I had from Puerto Rico, [Angel] Alberto Colon, did. My gosh, he went back to be director of research for the Puerto Rican Industrial Development Corporation. He was dismayed at the low state of industrial hygiene and safety in industry in Puerto Rico. He found that people didn't pay much attention to him

because he wasn't an M.D., so he went to the University of Madrid and got an M.D. We were in Madrid the Christmas that he just finished his medical work. Christmas Eve we had dinner with him and he gave me that poster, with my name on it. Then he came back and you know what that guy wound up doing? They grabbed him for the medical school at University of Puerto Rico and he wound up being dean of the medical school.

HEITMANN: Boy, that's an interesting graduate student you had there.

WARNER: That's true. He's retired now, living in Florida.

HEITMANN: I imagine that's how you got interested in serving on the commission on science and technology.

WARNER: That was a National Academy commission. That wasn't the reason I was asked to serve on it. But the fact was that I had this guy as my own graduate student and his brother was a graduate student in organic chemistry at Carnegie Tech at the same time. We had these two doctorates from Carnegie Tech down there, and boy were they helpful to the commission. They gave me the real low-down on everybody and everything. Their father was the Secretary of Agriculture and Commerce. One of the well-to-do old Spanish Puerto Rican families.

HEITMANN: Who were some of your other graduate students in that post-war period?

WARNER: Well, [Charles] Law McCabe was one. Law McCabe. He was probably the best one.

HEITMANN: Warren McCabe's son?

WARNER: No. He's not related in any way, I don't think. He's a Dickinson graduate. Phi Beta Kappa from Dickinson. He was probably in all respects probably the best graduate student I ever had. If you look at the publication list, he did a lot of problems. He started the really serious epoxy work. He studied the whole series of fluoro-, chloro-, bromo-, and iodohalohydrine. He started the relationship of the acid level of the solvent to the reaction kinetics. After he finished with me he went on the faculty at Harvard. He was really substituting for Kisty. Half the time Kisty was out of town and Law would have to take over his course in thermodynamics. Then Bob Mehl, the fellow that came as head of metallurgy, and the Metals Research Lab, knew Law well because Law was running around with Mehl's daughter. So he talked Law into leaving Harvard and coming back and joining the metallurgy faculty and

really picking up what I had started down there, the program on the physical chemistry of metallurgical reactions.

When he was there, Mehl was about to retire so he left on leave to be a sort of watch-dog for what's going on in Europe for U.S. Steel. And Law became head of the metallurgy department and director of the Metals Research Lab. When I was president, I made him dean of graduate studies. Then he went on leave for Washington as Deputy Assistant Secretary of Commerce for Science. When he was at Harvard he met a girl who was in some girl's school up there, Ingrid Goebels. Her father had started and operated a very profitable diamond studded tool company in Detroit. It was quite a well-to-do family. Ingrid had a brother who was no good as a businessman. Ingrid's father got sick and they tried to sell the company without much success. They didn't think the price they were offered was right. So Law, instead of coming back to Carnegie Tech, where he was to be Vice President for Research and Dean of the Graduate School, had to go to Detroit to rescue the family fortune. He did. He disposed of the company at a very good price to the Norton Company. They made Law vice president in charge of that division. Law wasn't too happy with the whole arrangement. Law wanted to be more independent of the family business. Although by this time, when the father died, Ingrid and Law were pretty well-to-do people. Law disposed of the business at a good price. But Law wasn't the happiest man in the world. He really preferred—at that time, if he had had a chance to get a good academic job, he would have taken it.

About that time another Carnegie Tech fellow, a metallurgy student who had been a student of mine, George [Adam] Roberts, knew Law McCabe and George, who had a doctorate from Carnegie in metallurgy, had become president of Teledyne. George had been president of Vanadium Alloy Steel up at Latrobe, and they had sold out to Teledyne and in the deal George had become president of Teledyne. Later they bought Firth Sterling Steel, which is a tool steel company here in Pittsburgh. Roberts then got ahold of Law and offered him the job of being president of Firth Sterling, so Law went to Firth Sterling, and did a good job. Then another Carnegie Tech man, Bob Sharpie, who got his doctorate under Seitz in physics and then was associate director of Oak Ridge after the war, was brought up to New York by Carbide and then later was made president of Cabot.

HEITMANN: The Cabot Corporation?

WARNER: Yes. Sharpie knew Law McCabe, too. So Sharpie went after Law after he'd been president of Firth Sterling for three or four years, and got him to come to Cabot as vice president in charge of the high alloy division out in Kokomo, Indiana.

HEITMANN: McCabe was going all over the place.

WARNER: That's Law. I remember a party in Pittsburgh for Law and Ingrid before they went

to Kocomo. I don't think Ingrid was very keen about going to a relatively small Indiana city. I remember proposing a toast to the King and Queen of Kocomo.

HEITMANN: I guess you knew more about small Indiana cities than anyone there.

WARNER: Well Law's job was probably the best job in Kokomo. I don't know what's happened to Law and Ingrid since they went to Kokomo.

HEITMANN: Well, in 1949 then, you became president-elect.

WARNER: Vice president and president-elect. Isn't that a crazy thing?

HEITMANN: How did this come about? Was it Doherty that did it?

WARNER: What they did was decide in 1949 that I was to be the next president. You said something about who was my sponsor. The trustees had a search committee. I guess I knew all of them. The top administrative people under Doherty were asked by the trustee search committee to write an essay about the kind of person they should find for president. I was on that writing committee. They had a meeting to decide what would be sent to the trustees committee and it turns out that I was the only that had written an essay. So naturally my specs were sent to the trustees. They weren't describing me so that they would be obvious in any way. Doherty had brought from Yale a man by the name of Elliott [Dunlap] Smith, who was made provost in order to promote Doherty's undergraduate educational program. Elliott Smith was on this writing committee. Elliott, I know, had been promoting Leland Hazard as a candidate. Leland was in-house legal council for Pittsburgh Plate Glass Company, and part-time lecturer in the Graduate School of Industrial Administration. Hazard was an interesting sort of man. He didn't know much about universities. One of the search committee members was Jim Beal. He was a senior partner in the largest corporate law firm in Pittsburgh, Reed, Smith, Shaw & McClay. I heard a rumor that when Smith had pleaded for Hazard before the search committee and then left the room, Jim [James Harvey] Beal had said, "I think that would be too big a hazard." One lawyer's opinion of another, I guess.

HEITMANN: I could see where it would be very difficult for a lawyer to take hold of what's really a technical university in many ways.

WARNER: I do know that the Committee was interested at this time in Baker, who was professor in Harvard Business School. I think the committee finally decided that Baker wasn't

their man. Baker did become dean of Harvard Business School a year or two later. So, he probably would have been a pretty good man. Then, out of the blue I was asked to come down to the office of the Chairman of the Board of Trustees.

HEITMANN: Were you in your office when they called you?

WARNER: Yes. Jim [James Moorhead] Bovard and I were good friends. He was president of Carnegie Institute and chairman of the board. He was a boyhood friend of Dick Mellon and was well-connected socially, around town. He was helpful in many ways to a certain extent. I sat down and he said, "Jake, the Board of Trustees wants you to be the next president of Carnegie Tech. Will you do it?"

HEITMANN: Did you say yes right away?

WARNER: I said, "Well, I guess I have to consult my wife."

HEITMANN: Smart move.

WARNER: It was strange, too. I went home. My wife's name is Louise. She was on a stepladder cleaning wallpaper. I said, "I have news for you, but you have to get down off that stepladder, or you won't be able to stand it. So she got down with a dirty napkin around her hair. I said, "Jim Bovard just told me that the trustees want me to be the next president of Carnegie Tech. I said I thought I would like to accept but I'd have to consult my wife." She started to weep. She said, "Jake, you wouldn't do that to me, would you?" Sharply, she got over it and agreed that I should accept. She had a wonderful life. She was a wonderful president's wife. She helped unify that university.

HEITMANN: She was then very much the president's wife.

WARNER: Oh, yes. She had a fine personality.

HEITMANN: But she also knew the demands that would be placed on her.

WARNER: Every member of that faculty was in the president's house sometime during the year, and she mixed them up, you know. When I first went to Carnegie Tech the people in

engineering and science didn't know and didn't care about the people in the fine arts college. But she would mix them up. It would never be just one college or one school. She mixed them up with town people, too.

HEITMANN: There was one thing I noticed looking at your resume—it seems once you do get to be president of Carnegie Tech, you really have tremendous activities in the Pittsburgh area. It seems like your whole career changed in some ways. This may not be true, but just looking at the resume, all of a sudden, you, as the highest representative of the university, really go into the community and become very much a part of it.

WARNER: Very much a community figure. Partly the property of the community in a way.

HEITMANN: Was your predecessor the same way? You were talking about all the problems you had on that job and the kind of strength you had to have on the job.

WARNER: I felt a great obligation when I undertook this job to have clear goals and very definite ideas about what I thought the university ought to become, and then look at the resources required to do it. I have in my files a study that the assistant president and I and the chairman of the executive committee of the trustees, who was very friendly about it, had made, and it showed that if we were going to become a graduate research university, that we really needed a good bit of money. We needed more space. We needed more equipment. We needed higher salaries. Many additional resources were required in order to promote excellence all the way across the line of undergraduate teaching, graduate teaching, and research. We had to have standards in all these areas that were respectable and would be admired by the public, particularly by our colleagues in other good universities. Well, it turned out that I estimated we had to raise quite a lot of money. Remember, this was way back before inflation started—money was still worth something. I suggested that we ought to have a meeting of the full board of trustees to let me tell my story about where we wanted to go and what it would take in order to do it, in the way of resources. Well, I couldn't get a move out of the trustees. They would sort of say, "Yes, yes," and then do nothing. I remember having a meeting of the executive committee up at the Pittsburgh Golf Club, I think it was after my second or third year. They asked me if I would like to say something about the state of the university. I said I certainly would. I guess I'd had two or three cocktails, so my inhibitions were all gone. So I told them, "Look, I've been trying to get an opportunity to tell my story to the whole board of trustees about what I thought our goals at Carnegie Tech ought to be, what resources were needed to achieve those goals, and I haven't gotten any response. Now, I have only this much more to say: unless you arrange to give me an opportunity to tell my story to a special meeting of the whole board of trustees, you'd better start looking for another president." I sat down. That got the executive committee excited, of course. And by golly, they did arrange a special meeting of the board of trustees and they had a very good turnout. There was a big attendance. One reason was Ben [Benjamin] Fairless, who was head of U.S. Steel and was a trustee and on the

executive committee.

HEITMANN: Fairless was his name?

WARNER: Ben Fairless. He was chief executive officer of U.S. Steel for quite a long while, before [Roger] Blough. He took the lead at that time and said, "I think we must arrange this meeting. Tell you what I'll do—I'll invite you to have the meeting in the U.S. Steel Board Room and then I'll have you all as my guests for dinner in the Executive Dining Room, afterwards." Well, almost everyone came. Even Jack [Henry John] Heinz, who misses more trustee meetings than anybody else. Anyway, I was prepared—I was really prepared for this. I had some good slides and I had tables and everything. I had a good projector. I had an assistant to run the projector. What I said we needed to do was to start immediately a fund raising campaign to raise at least fifty million dollars. When I finished there was complete silence. Ben Fairless leaned back and said, "I move that we do what Jake advises us to do." That passed. They authorized the executive committee to implement this plan for a fund campaign.

HEITMANN: When was this, about 1951?

WARNER: It was around 1954 or 1955. I guess it was about that. I could tell you by looking at some things back here. But anyway, this wasn't the end of the story. They instructed the executive committee to make plans for this campaign. And then nothing happened. So Walter Blenko, who was chairman of the executive committee, was about as annoyed about the whole thing as I was. At that time our Carnegie Tech trustees were the same individuals that served as trustees of Carnegie Institute. They had to be residents of Allegheny County.

HEITMANN: So you couldn't have trustees from, say, any other part of the country.

WARNER: You couldn't have alumni as trustees, unless they lived in Allegheny County, but the executive committee only had to have a majority of its members as trustees. So we had several prominent alumni on the executive committee. One of them was Charlie [Charles] Wilson. He was a Tech alumnus and he was by that time Secretary of Defense. That'll tell you when this action got going. So, Blenko and I said, "Let's go down and talk to Charlie Wilson." So we did. We made a date and went down over at the Defense Department and then told my story to Charlie. He thought about it for a while, then said, "I'll tell you what to do. You go back to Pittsburgh and tell those bastards that I'm coming to Pittsburgh (and he named a time) and I'm going to throw a big dinner at the Duquesne Club and I'm going to invite all of them and I'll tell them what to do." Well, I came back with that news and what happened? Dick Mellon, Bill Price, who was head of Westinghouse, and Jim Bovard, Chairman of the Board of Trustees, became the hosts for the dinner and Charlie Wilson became the honored guest. Then

things started to move. Ben Fairless took the chairmanship of the fund drive. What a guy he was! He had a private railway car, you know, and we would go down to Philadelphia and he would invite all of his friends to lunch or dinner at the Union League Club. We'd go to New York, same way, Cincinnati, Chicago, St. Louis, Cleveland. He did the same thing with the big shots in Pittsburgh. He would invite them to lunch at the U.S. Steel Executive dining room. Then his trick was always the same, he would say, "Well, Gentlemen, I suspect you know what we're here for. Now, Jake, you tell them what we want them to do."

HEITMANN: So he was really your helper in this fund raiser.

WARNER: He was a real helper, yes, indeed.

HEITMANN: You still had to give the pitch, but he was right behind you.

WARNER: That's right. What a wonderful man he was. He was worth millions to Carnegie Tech. We completed this campaign, in fact they raised a good bit more. We won the complete affection of the Mellon families, which had never been done before. They thought Carnegie had taken care of us. But I had become quite a good friend of Dick Mellon. I don't know how much money we took in. Sarah Scaife was Dick Mellon's sister. She married Alan [Magee] Scaife and she and Dick Mellon were the two richest people in Pittsburgh, of course, after R. B. [Richard B. Mellon] and Andrew [Andrew William Mellon] died. Sarah Scaife had a foundation and some charitable trusts and was very generous. She built an engineering building for us and Herb [Herbert Alexander] Simon got a professorship that was endowed by Dick Mellon. Dick Mellon gave the school about a million bucks a year. Out of the A. W. Mellon trust, I got five million dollar endowed professorships, a million dollars a piece for each department in the fine arts college. There were four departments. Then another endowed visiting professorship, so they could bring distinguished artists here that they couldn't hire as permanent faculty members. For example, they had Casals here one semester, on this endowed professorship. They had a great city planner from England who planned the rebuilding of those towns that were bombed during the war, David Lewis. He stayed in America permanently.

HEITMANN: Not Buckmeister?

WARNER: No, well, he was quite a famous man. All of these people were very stimulating to the faculty and the students both.

HEITMANN: They are very important for the modern university.

WARNER: That's right. I don't know how much money we raised, but it was just a new period in the history of Carnegie Tech—money came in over the transom, really. You know, it's just annual giving. It changed the whole attitude of the community, people all over the country and the alumni, too.

HEITMANN: You made it into a national university.

WARNER: That's right.

HEITMANN: In a way that it wasn't before the war.

WARNER: This was a great experience for me. I'll tell you. You can imagine how delighted I was and how much this did. This university just started going to town. H. [Horton] Guyford Stever was my successor. I left him reserve funds amounting to seven or eight million dollars. Money that was not committed. This did not include the endowed professorships or anything like that because that's committed money. This was money that had no commitment on it at all about how it's supposed to be spent. In our drive, we arranged six million of that as a fund to be used to support the budget in engineering and science, as needed. But anyway, there was something like seven or eight million dollars in these reserve funds when Stever took over. He was president six years, I think. Six or seven at the most. The last couple of years he was there, he had spent all his reserve funds and he was running deficits of a couple of million dollars per year. Then the trustees blew the whistle. He went to Washington as director of the National Science Foundation.

Then they made [Richard Michael] Cyert dean. He's been a good dean. He's a University of Minnesota boy who did his doctorate at Columbia in economics. I think in management science, too. He's a very bright man and he turned out to be a damn good administrator. He got the school back in the black the first year he was president, without having anybody commit suicide or having to reduce salaries. He just took natural attrition for a year or two and then he started a campaign. His campaign was a hundred million dollars, and it was finished successfully. You see how much easier it becomes all the time. But I suppose his hundred million isn't worth much more than my fifty was.

HEITMANN: It might be worth closer to thirty million depending on buying power.

WARNER: Yes.

HEITMANN: What is your vision of the modern American university? What do you think it should do? What do you think it shouldn't do? You definitely raised the money and once you got the money you did something with it.

WARNER: My idea of what a good university ought to be, a general sort of all-conclusive statement, is it should be a center of learning. I think it should aim to do good teaching and good scholarly research. I don't think it wants to be the slave of any segment of our society. I think it wants to be independent. I think it would be a disaster if a good university started doing secret work for the government. If the university has scholars that are important to the national security, there is always a way, without degrading the university. There is always a way of letting their knowledge be used for the national security. They can be consultants to Los Alamos, Oak Ridge, etc. They can take leave for a year if they are particularly important for some project. But you don't have to use their talents for doing secret work on the campus. I think that they have to be the source of basic new knowledge. I'm not talking about technology. I like it when university professors will do something of use to society once in a while. I don't like scientists like the one who said when he was being interviewed, "I'm glad to tell you that the research I do couldn't possibly be of any practical importance." That, to my mind, is not a proper honest attitude for a good scholar. He ought to be glad if some of his ideas are useful to society.

HEITMANN: Do you feel that the undergraduate student, or the graduate student for that matter, at Carnegie Tech during your period—would you like to see that what that person takes away is an excellence in the understanding of his field and leave it to the employer or society to create an opportunity for him to use it, in a technological society really?

WARNER: Some very well trained basic scholars, once they have finished their education, really they never finish their education, obviously, they have to keep studying. If you are going to be a scholar, you're always a scholar. But the market will take care of some of this. Suppose a fellow has done a lot of studying and a lot of research of a very basic character for his doctorate, maybe even a year of post-doctorate work too, then takes a job with Merck or with Allied or with Monsanto. If he sees the high salaries coming to people who can and will do some applied research, why, he won't be so damned particular about what he does.

HEITMANN: I think you're right. The market often will determine which, but often you can't learn pure science outside the university. It's very hard to do that.

WARNER: It's very hard. A lot of people learn a lot of it on their own. God knows I did, but it's not easy.

HEITMANN: It's not ideal, anyway.

WARNER: No sir, not the time. I shouldn't have had to spend six or seven years just learning the state of modern physical science. It means that my graduate years were pretty largely wasted, and so were my undergraduate days.

HEITMANN: Well, it was during this period of your presidency that you only had effective arguments for this, "The goals of a national university" was what you called it. You were also very busy at the presidency. You were beginning to serve on a whole lot of different corporate boards, and the like. I was wondering if there were some that were of particular interest to you.

WARNER: Most of them were rewarding. The first substantial board I was on was at Jones and Laughlin [Steel]. I went on that, I think, the first year I became president. I was on it until well after I had retired. They had a retirement age of seventy-two, which means I was on it from 1967 to 1972, after I had retired. I was kept on as a consultant after that for several years. I was what you'd call the science member of the J & L board. I was supposed to follow their research and development work. They did build quite a big research laboratory while I was on the board. I was talked to a lot by their director of research, Herb [Herbert Winfield] Graham. Directing his staff on what kind of things they were going to do. You see, this came after I had some experience in the metallurgic field. I joined the metallurgy faculty for two years, 1936-1938. I figured, well, if I'm going to start a program on physical chemistry of metallurgical reactions and teach a graduate course in that subject, I'd better find out how steel is actually made. I went out to Homestead, No. 4 open hearth, where I knew the superintendent, and one of my former students, Max Leohtner, was the assistant superintendent, and I actually lived with him for a month or two. The next summer I went down and spent a month at Armco at Middletown, Ohio, actually helping in experiments that were being done out on the open hearth floor. I don't know whether the name John Chipman means anything to you.

HEITMANN: No.

WARNER: John wound up his career as professor and head of metallurgy at MIT. He was a physical chemist from Berkeley. He went into metallurgy at the University of Michigan after he got his doctorate. He became probably the best-known man in America on the physical chemistry of steel-making reactions. I knew John. I don't know how we first got acquainted. He was at Michigan and then he went from there to being associate director of research for Armco. Let's see, Bob Mehl had been associate director then came to Tech and there's John Chipman that I knew. When I went he was doing a lot of these metal slag equilibrium studies, by taking samples out of the open hearth, while steel was being made. I would help break the slag off of these ladles that we took samples out with and worked like a regular member of the research staff or the lab staff.

HEITMANN: This was while you were president?

WARNER: No, this was back in the period of 1936 to 1938.

HEITMANN: Okay, way back then.

WARNER: When I was on the metallurgy faculty, for two years.

HEITMANN: So you were really a board member at Jones and Laughlin.

WARNER: I knew something about it. Of course I knew physical chemistry. They let me help them on a lot of corrosion problems. I enjoyed that very much. Next Pittsburgh Plate Glass, now PPG Industries, invited me to come on their board. That was a good experience, too. I liked that. The other, next large one was Dravo Corporation.

HEITMANN: Oh, yes. I know they are a local Pittsburgh Company.

WARNER: Well, they are pretty much a national or international company now. The Depression hit them pretty hard. They are heavy contractors, steel mills and various metallurgical plant builders, builders of dams and docks and bridge piers, diggers of tunnels, etc. I was on their board for I don't know how many years. My background didn't help Dravo Corporation nearly as much as it helped J & L and PPG Industries. You see, PPG is pretty diversified. It's not just a glass company—it's a little over a third glass, a third chemicals, and a third coatings and resins. I guess if you put the glass fiber in with the glass, it would be a good bit over a third, more like forty percent or forty-five percent glass and glass fiber. I knew I could be helpful on a lot of problems at PPG. Then there were a bunch of smaller companies I was in.

HEITMANN: Magnetics.

WARNER: Magnetics was interesting. It was an outfit up here at Butler. It was started by Art Black. It was bought up after I had been on it for, I don't know, four or five years, by Spang and Company. I became a director of Spang and Company.

HEITMANN: Spang?

WARNER: Spang, yes. It's a privately owned company by the Spang family and some of the employees. The head of Spang and Company, now, was a student of mine, Frank Rath. He was a student in the days when the big Depression was on. Back in 1929 to 1933. Frank worked in my lab on a WPA sort of deal, you know, washing dishes and cleaning up.

HEITMANN: Glassware and the like, yes.

WARNER: When he graduated, he must have graduated from metallurgy because he went to work for U.S. Steel. Then he married a Spang daughter.

HEITMANN: Well, that's good.

WARNER: Married the boss's daughter. And now the Spang family, as far as men are concerned, is thrown out. So Frank is now chief executive officer of Spang and Company. It was a funny situation because I think Frank Rath was a little unethical when he got hold of magnetics. I never liked Frank too well, but I helped Frank some after I went on the Spang board. But that was because the only remaining Spang was so old that when he'd come to a board meeting he'd go to sleep. So I proposed to Frank that if he could, why not get a retirement age for directors. They have a retirement age at PPG, at J & L, Dravo, so why not get rid of this old man? Or, if you don't want it to apply to him, put a grandfather clause in, but get the thing cleaned up. He did. Frank made some bad deals for Spang and Company. He bought up some drug and another discount firm and put them together as Spang stores. I think that they just had to write off all of it. Spang was successful at manufacturing a variety of oil drilling equipment. I guess they got their start from recovering metal from slag in the steel companies, then preparing the slag, sizing it, grinding it, breaking it. But enough of that. Nuclear Science and Engineering was bought out by a very hot internal political fight. I was on the board when this thing came up. A couple of people had put up a good bit of the money and owned a good bit of the stock. They started trying to get complete control of the thing. They did some very dirty tricks to the management. The guy who had started it and was running it was a man who split off from Westinghouse. They tried to get rid of management. I was so unhappy with the way the whole thing was going, I just resigned from the board. I was chairman of the board at the time. It was picked up then by this international pharmaceutical company, a fellow by the name of [Milan] Panic was the head of it.

HEITMANN: It's a good name. I wouldn't want to work for him, I think. I think I'd worry about that for a while.

WARNER: He offered to buy all outstanding stock at a bargain price, of course. So I decided, “Well, I don’t have much invested in this.” I had five hundred shares or something like that. I don’t know what I paid for them—I think two dollars and fifty cents a share. I decided, “I don’t know whether this man is a genius or a damn fool, but I’m not going to sell my shares back to him at a bargain price, I’m just going to divide them equally between my two boys and tell them to just hang on to this forever and see whether they amount to anything.” So they still have them.

HEITMANN: Panic.

WARNER: Let me tell you something else. You noticed that I was on the advisory board, the research advisory board of City Service.

HEITMANN: Right.

WARNER: They paid a nice salary. I think we had maybe four meetings a year. I think they paid us five thousand dollars a meeting. Maybe it was five thousand dollars a year, but it was juicy anyway. All expenses. I was on the Westinghouse research advisory board for a number of years. All of these directorships and advisory boards pay a good stipend. That’s the reason the Warner family got pretty rich. My dear wife was an honors graduate, Phi Beta Kappa, in economics. She was the bookkeeper and controller of the Warner family. She had very cute ideas. After I began to earn a decent salary and had a number of consulting jobs, she said, “Now look, Jake, you know very well that we can live on your salary, let’s start another bank account, which will be our investment account. All of your consulting fees, all of your dividends on stock you already have, all of your fees like Westinghouse advisory board, this goes in the investment account. All of our dividends from investments will go in the investment account.” I said, “Yes, but this income, I’ll have to pay the income tax on it.” She said, “Okay, we’ll pay the income tax out of your regular salary.”

HEITMANN: I find that whenever I speak with eminent chemists, their wives played a great role. They never get credit for it and behind them all the time, their wives are doing things for them. You just mentioned that your wife was being the great financial wizard, can you tell me some more about her, just some of the ways she helped you?

WARNER: She was a wonderful woman. Her last four years were in declining health, and I devoted more and more of myself to taking care of her. When we moved out here, she was a wheel-chair case. But she still had this exuberance and when you introduced her to anyone, her

face would still light up, you know, vivacious. People always thought she was one of the friendliest people and still she'd never butt into anybody's affairs. She wouldn't have anything to do with gossip. We were still in love after fifty-five years. I'll tell you, it was great.

HEITMANN: She was still helpful to you when you were president of the university, as well.

WARNER: Oh, she was a wonderful president's wife. What a recognition people on the campus have given her. I told you that Mrs. Scaife gave me that engineering building. After this campaign was all over—

HEITMANN: Oh, yes. And Warner Hall is the administration building.

WARNER: That's the administration building.

HEITMANN: Wow! It looks like it's, what, about nine or ten stories.

WARNER: No, it's only six.

HEITMANN: Six stories?

WARNER: Well, it's two stories below ground.

HEITMANN: I'll ask one other question and then we'll go back to your family. In 1956 you became president of the ACS. You were president-elect in 1955 the way they work it. I was wondering what your recollections of that year were? Who were the people you enjoyed working with at ACS and who were the people you didn't?

WARNER: Of course, I was very active. I had been on the board of ACS for quite a while. I don't know how many years before. I had been on the council for many years as a councilor from the Pittsburgh section. I had been chairman of the Pittsburgh section and got the Pittsburgh section award one year. Do you know anything about the Hancock Report [on Organization of the American Chemical Society]?

HEITMANN: No.

WARNER: In ACS?

HEITMANN: No.

WARNER: Well, it's a historic document. We developed in the council of ACS, I don't know how many years before I was president, what you'd call a young Turk organization. Old Charlie [Charles Lathrop] Parsons was a dictator. He ruled the roost with an iron hand. He would make the nominations for president and everything else. Council wasn't anything. There wasn't anything like a democracy in the ACS. Well, we finally made enough noise in council, this young Turk crowd, that we got, I don't know whether this was before or just after Charlie Parsons died, anyway, we got a management firm in. I think it was a Chicago firm, Hancock, to study management of the ACS. And their report was the famous Hancock Report. Then we started a campaign to get the Hancock Report implemented. Again, I was chairman of the council committee to implement the Hancock Report. I was sort of—I don't think I ever was ugly or disagreeable, but I was insistent, and we got a revolution in the ACS. It really was a revolution. We rewrote the constitution and by-laws and I was on that committee. The council became a body that decided some things.

HEITMANN: Who were some of the other young Turks with you on that?

WARNER: Well, there was a chemist at Upjohn. He was very well known as a research man. I don't remember these names too well. There was another fellow that was very active, who was from Chicago. He was a little too nasty sometimes. You know, you get things done if you don't call people a son-of-a-bitch the first time you meet them. He was inclined to start out by telling a guy he was no damn good, you know.

HEITMANN: You have an uphill road from there.

WARNER: There were people from the Philadelphia section, from the New York section, from Chicago section, the Pittsburgh section. I don't remember West Coast people being very active in this. Right along the same time we had this whole business about what to do about Universal Oil Products. These were very interesting times in the ACS. Then Alden [Hayes] Emery came in on this. Alden had inclinations, and I think probably a desire to be what Charlie Parsons had been. But, this thing had gone too far and Alden decided that it was better to join them than to fight them. If you can't lick 'em, join 'em. That was his goal. Another one of the old guard, Walter [Joseph] Murphy, was editor for many years of *Chemical and Engineering News*. He had to have his come down.

WARNER: By the time I got to be president, the revolution was over. Things went quite smoothly. We had some good people on the board. Ernie [Ernest] Volwiler was chairman of the board for a while when I was on it. Roger Adams was chairman one time when I was on the board. We had some good people on the board. One thing I enjoyed, and I think I did a lot of good for society on—when I was chairman of the publications committee of the board, I started a summer seminar meeting of the publications committee plus the editors of all the journals. We would go to a place like Bedford Springs or various places. There is a little place called White Sulphur Inn.

HEITMANN: That's in West Virginia.

WARNER: You're thinking of Greenbriar or Berkeley Springs.

HEITMANN: Berkeley Springs.

WARNER: This was White Sulphur Inn. It's at Manns Choice, Pennsylvania. They had a sulphur spring there. It was a nice place to go. Louise and I used to go there. The president of W. and J. and his wife would go out there for a weekend just to relax. They had built a stone building, just one whole floor of it. I don't know how big you would say it would be. It was for meetings. Then, down below, they had several smaller rooms. That was a good place for us. The dining room was good. They had a game room in the basement, where we would meet for the cocktail hour. Those were the days when Art [Arthur Clay] Cope was an organic chemist at MIT, and on the board of its Publications Committee. He died and left his fortune to ACS. Ralph [Alexander] Connor was on the committee. He was a Penn man, wasn't he? He was at Rohm & Haas, director of research at Rohm & Haas.

W. A. Noyes was editing the journal in those times. We had the *Journal of Physical Chemistry*, which we took over, and the *Journal of Organic Chemistry*, when I was running the committee. We started a couple of journals. It was nothing like what they have now. A lot of people were amazed that we were increasing the number of journals, anyway. But we settled a lot of things. We settled a lot of stuff about chemical abstracts at these meetings, too. Dale Baker was always there, and the fellow before Baker, W. A. Noyes, Sr. started *Chemical Abstracts*, as a supplement in the journal, years and years ago, before I was active in chemistry. That became a separate thing later. Of course, *Chemical Abstracts* was a separate venture, and a very important one, by the time I was active in chemistry. Establishing an active Publications Committee was one thing I think I did for the ACS that was important.

HEITMANN: Straightening out the journals?

WARNER: Yes, that's right. Beginning a publications program that made sense—conservative, but not too conservative. If a real need developed for a new journal, we would try it out.

HEITMANN: I was wondering, of all the many, many honors of all these years, beginning in the early 1950s, and late 1940s, and going on into the 1970s, there is a whole list of medals and awards that you won—the gold medal of the AIC and over a dozen honorary degrees, and the like, Horatio Alger Award—was there any one of these awards in particular that you look at now and think of as being the greatest honor?

WARNER: The greatest honor, beyond question, was election to the National Academy. I think of myself as being sort of a questionable case. I don't know whether having so many friends in the fraternity was the reason for my election or not. I know that people like Scatchard, Jack Kirkwood and so on, Fred Seitz. [Isidor Isaac] Rabi was another physicist that I knew very well. It's surprising. You know, the man that called me when I was elected to the Academy, the first that called me was Rabi. Imagine that! A physicist calling me!

HEITMANN: Wow!

WARNER: Rabi was a man of good humor, too. A lot of fun. We were on the advisory board of the AEC together for quite a number of years. I served two six-year terms on the General Advisory Committee. Rabi was on part of that time. We were asked again for recommendations about this program for a nuclear powered airplane.

HEITMANN: Yes, I remember that!

WARNER: The advisory committee thought it was a nutty idea from the start. We agreed with Charlie Wilson, his comment at the Department of Defense, when the thing was brought up about radiation and all that. Charlie Wilson's remark was, "What in the hell are you gonna drive 'em with, eunuchs?" We'd been down to Oak Ridge and came up to Cincinnati, Evandale, where General Electric had a big contract on a nuclear airplane engine. Actually it just isn't realistic to think that you can carry that much shielding and have any performance out of an airplane. It just didn't make sense. It just about died and then some damn laboratory would get their congressman involved again.

HEITMANN: And you became really familiar with this kind of politics in science.

WARNER: Our statement was that these damn projects are harder to kill off than a cat. We came into Evandale and of course, the GE boys threw a big party for us as soon as we arrived, the night before we were to visit out at the plant. We had dinner at this hotel in Cincinnati where the diners and offices are down below and the hotel is up above. A pretty classy place. Well, we had a big cocktail party and then dinner for us and then we got in a bridge game afterwards. Rabi and I were playing two General Electric men. Then we had a case of this Heineken beer and Rabi is a good beer drinker and I was a good beer drinker in those days, so we played bridge and drank beer as we went along. God, pretty soon the beer was all gone. They had to go out and get another case of beer. I think maybe it wasn't just the four of us. I think four more were nursing themselves on this Heineken. Then we had the centennial at the Academy. It was held in Constitution Hall. The members of the Academy were lined up in order of the year in which they were elected to the Academy. They were all in academic gowns and hoods and so on, lining up and then they marched in finally at the right time. Well, Rabi had been elected just far enough ahead of me that he was in the back of the section that moved ahead of our section so we were right next to each other. He started telling people what great prowess he and I had at playing bridge with these GE fellows. Then he said to me, "Jake, do you still drink as much Heineken beer as we drank that night?" I said, "Hell, no, Rabi if I did that now I'd have to get up about five, six times a night." Oh, Rabi backed away in his Nobel robes and bowed real low to me and said, "I prostate myself before you."

HEITMANN: Well, I have a few quick questions I think will close our interview. One deals with your views on the university. You've outlined what you think the national university should be in the case of Carnegie-Mellon. Just in our reading the papers and your contact with what's taking place in the last couple years, do you think the American university is going to continue to be a great source of innovation, or do you think that as an institution it will somehow be eclipsed by either the industrial research lab on one hand and the government laboratories on the other?

WARNER: I don't think so. I don't think in an important way that industry will ever support a fundamental research lab. The closest to it that we have, in my opinion, is Bell Labs. Bell Labs is a little different from the ordinary industrial lab—after all, they are running a utility. AT&T is a utility. It has to be more applied. There has to be a bottom line at least hopefully in mind with a DuPont research lab and a Monsanto lab and so on. The government laboratories and—well, take the Manhattan Project. We got Los Alamos, Oak Ridge, Brookhaven, and Argonne, the Livermore lab and the Lawrence Berkeley lab. Sure, there is some good basic work going on in these labs because of the expense of the equipment, primarily. I mean the [Enrico] Fermi lab out at Batavia, Illinois, for example—no university can build those five hundred BEV machines, and operate them. So I think some of that sort of thing you have to expect, in government-supported national laboratories. I don't think that taxpayers and Congress will support too much of that. I think it's going to be supported only under a lot of political pressure. A lot of political pressure from the scientists or from the various regions of the country. The

logical thing for research and government laboratories is pretty much like what the Bureau of Standards has been doing for generations. And that is, filling in the vacant places in the tables. That's not really creative research. It may provide data that creative scientists can use, of course. Real basic research has to be research that's trying to prove or disprove something, not just determine the heat capacity of titanium dioxide because that's a hole in the table of specific heats.

HEITMANN: So you see really breakthrough innovation taking place when it's centered in the university.

WARNER: It has to be centered in the university, by and large. Except for say, high particle physics where you can't afford to do it, and don't see any way of doing it in the university.

HEITMANN: One final question. We've mentioned your wife, but I was wondering if you have anything to say about your two sons.

WARNER: The elder one is fifty-four.

HEITMANN: What is his name?

WARNER: William.

HEITMANN: And he's at the University of Minnesota.

WARNER: He's professor of applied mathematics in the Department of Aeronautics and Engineering Materials. He's a good applied mathematician. Haverford, as an undergraduate, his doctorate at Carnegie Tech, then he went on the faculty at Brown for a couple years. They had a good applied math group up there. [William] Prager and his crowd. Then Minnesota hired him. He's been at Minnesota for a long time now. He's a good scholar. He doesn't want to get involved in very much administrative work.

HEITMANN: And you have a younger one.

WARNER: I have a younger one. He's fifty-one this year. Or, he'll be fifty-two this year, I guess. He's a general manager.

HEITMANN: What is his name?

WARNER: His name is Thomas. Thomas Payton.

HEITMANN: He's a general manager.

WARNER: General Manager of a Lear-Siegler plant in Holland, Michigan. It's a plant making heat pumps and other air-conditioning and heating and ventilating equipment. He was trained at Carnegie Tech. About half chemical engineering and half management. He's a good manager, a good production man. Lear-Siegler was started by the Lear of the Lear jet. He's not connected with the company any more, except his name is still in the company. He wanted to go ahead making further developments of the executive jets, so he pulled out of his company. It's apparently a very solid company.

HEITMANN: And probably a very innovative company, as well.

WARNER: My son has just been ordered to double his production. Tom is the one that called me. He has a Watt's line, you know. He called me today, he interrupted us.

HEITMANN: I think we're actually pretty well through with the interview. We've covered almost everything.

WARNER: You haven't covered my last fifteen years as a consultant.

HEITMANN: Why don't you tell me about it then?

WARNER: You can drop this from the interview if you want, too. It's not chemistry, it's higher education.

HEITMANN: But it's part of your career.

WARNER: It started, I guess, from the notice I got of being invited to give the Perrin Memorial

Lectures by Tata Iron and Steel [Company] in India. Somehow, the Ford Foundation knew I'd been in India and so they asked me to go as advisor to a new commission in Pakistan, which was making a five to ten year plan for higher education in Pakistan. I was one of two consultants. The other consultant was Herman [B.] Wells, who was president of the Indiana University. So Herman and I were over there for I don't know how many months, but we helped them to design their educational plan, which the government approved. I was looking after science and engineering, of course, and Herman was looking after business schools particularly, I guess to a certain extent university administration. But anyway, our ideas were accepted and after the government approved it they set up what they call an implementation agency. Part of the plan involved, for example, establishing a new university of technology in East Pakistan and another in West Pakistan. Of course, the plan provided for increasing resources in both science and engineering. They have quite a few. The University of Peshawar is really the only university that is organized like an American university where, say, the engineering college is an integral part of the university in its administration. The other engineering schools in Pakistan are separate colleges that are just affiliated with the university. So there's a difference, you see. The reason the engineers insisted on being in a separate college, I guess, was that when they were in the university they were so overwhelmed by the faculty in the humanities and social sciences in numbers that they never could get anything adopted.

HEITMANN: So, they wanted their own university, really.

WARNER: Yes. Concerning that implementing agency, they asked me to have Carnegie Tech take on the job of being advisory to the implementing agency as far as science and engineering was concerned. So we had a faculty member over there all the time. Then about once every quarter somebody else from Carnegie Tech would go over, the Dean of Engineering or the head of one of our engineering departments.

HEITMANN: And this really began in 1951 or so, when you gave those lectures.

WARNER: This Pakistan thing came later. Still later we got involved as a constituent member of the group that helped build the Indian Institute of Technology at Kharagpur. We had a number of professors over there. Truman Kohman was over for a year. Dave McKinney was over. Jack [John Gaston] Fox from physics, Gary [Gerhard Julius] Derge from metallurgy. All of them were over there for one or two years. I visited a couple of times at Ford's request. Ford had put some money in it. It was mostly really USAID [United States Agency for International Development] money. Then, I was back in Pakistan later advising on the establishing of the Pakistan Universities of Technology. Oh, another thing in that same area I got involved in was in Afghanistan. The University of Kabul had a contract with USAID for building an engineering college. USAID gave the contract to the University of Wyoming or something like that. It did such a poor job that the Afghan government asked USAID to cancel the contract.

The Russians offered to come in and do it. You can imagine what that did to Washington. They invited Jim Killian, president of MIT, and me to come to Washington. Urgent, very urgent. We went down and found out what it was all about. I said, “How about you, Jim? How about MIT doing it?” He said, “Well, we’ve got our hands full already. Why don’t you take it on, Jake?” I went back and got on the phone. I thought I’d have some drop-outs, you know, so I invited about fifteen university presidents to come to a meeting in my office or send a representative and told them briefly just what this was about. I made it for the next Saturday or something like that. They all came and they all were willing to join the consortium. I only wanted about two thirds of that number, you see. Well, you couldn’t turn them down. So within a month we had our team in Afghanistan, at Kabul, and took over this job. I just couldn’t imagine that red tape could be cut so fast in Washington. This was national honor at stake. Boy, they really cut the red tape. I had just the right man to head our team in Kabul.

HEITMANN: When was this? Do you remember the date?

WARNER: I could find out, but I don’t remember, exactly.

HEITMANN: Were you retired then?

WARNER: Yes. I was. It was just about the time I started in Southeast Asia. Our ambassador to Afghanistan at that time, what was his name? He later came back here. He wrote me after our team was in there. He wrote me telling me what a wonderful job we had done, what a good thing this was for the U.S. Then a year or two later, he came back from Afghanistan and temporarily headed up this outfit that Admiral [Arleigh A.] Burke had started—the National Security Associates, affiliated with Georgetown University, studying national security and all that. He temporarily, this ambassador, and I can’t recall his name, was temporarily taking Burke’s place as head of that, well you’d call it one of the brain trusts. Although it was already a little on the conservative side. Probably as much on the right as what’s the name of that other study group in Washington was on the left.

HEITMANN: Brookings?

WARNER: Brookings, yes.

HEITMANN: Brookings is pretty well on the left.

WARNER: Yes. Well, left of center, I guess. I think they do good work. I’ve been around

some of these right wing fellows a good bit. I was on their research advisory board, because the Scaifes gave them a good bit of financial support. Anyway, I found out what his name was and when I went to the first meeting, I took a copy of his letter along. When I was introduced to him I handed him that letter he had written me. We had a great time over it. About that time Harry [Harold Clayton] Case, who was the Ford representative in Pakistan, was moved to be their representative in the Philippines. They wanted to spend a lot of money on building the University of the Philippines as a graduate university. Carlos Romulo was president of the university. He was president of the U.N. Security Council when it was first organized. Actually, he was president of the University of the Philippines all the time I was working over there. The Ford Foundation loved him. Later, he became foreign secretary. He's still foreign secretary under the dictator [Ferdinand E.] Marcos in the Philippines now.

HEITMANN: Right! I know who you're talking about now.

WARNER: That's a present from him to Louise and me. I've done a lot of work over there. We really greatly improved the University of the Philippines. I'm sure it's still the best graduate university in all of Southeast Asia.

HEITMANN: What did you do? Did you do work on curriculum reform?

WARNER: That's right. In different places. Part of it was just looking at all of the universities that were involved at least in science and engineering, in the Philippines. I started out by visiting most of them, like Silliman University, the New Mindanao State University in Mindanao. I visited a number of Catholic universities such as Xavier University, on the north coast of Mindanao, and the University of San Carlos, at Cebu City. That's the place where Magellan was killed. There's a hotel, a dandy modern, air-conditioned hotel, named the Magellan Hotel. This university, San Carlos, is older than Harvard.

HEITMANN: I didn't know that.

WARNER: St. Thomas in Manila is older than Harvard, also. They were founded by the Spanish when they took over. The San Carlos one is run by the Fathers of the Divine Word. I had never heard of them. It's an Austrian order. They are scholarly men. I was surprised that the rector, I guess they call him a rector, is a scholarly anthropologist. He's using the Philippines as a laboratory. He's publishing work. He used to send me reprints of his papers. He hasn't recently.

HEITMANN: But you concentrated on the physical sciences and engineering.

WARNER: Physical sciences and engineering, yes.

HEITMANN: And you evaluated first and then made recommendations.

WARNER: San Carlos has a new campus outside the city, which is a new engineering college, completely new. A lot of the money for it was given by the Austrian and West German governments. Ford hasn't put very much money into that place, but they liked it and they gave a little help. I think mainly for the library. They apparently have a good library, about what you'd call early civilization in the Philippines and general civilization in the Philippines and the civilization built on the rice terraces up north. It's a great place and then there's the University of the Philippines in Quezon City, just outside Manila. Silliman University is at Dumaguete—that's on the bottom end of Negros Island, the big, middle island. Cebu City is on that one. At Dumaguete, Silliman University was started by the Presbyterian Church and now I think it's supported by the National Council of Churches. It's quite a good undergraduate university. It has, for example, a very good music department, music conservatory. Well, they were wanting to branch out and get into agriculture and also engineering curricula. They wanted Ford help for this. Well, my recommendation was that Ford turn them down. They had better do well at what they were doing. There were enough engineering colleges in the Philippines. So my visit there didn't do Silliman University any good. I think it was the right answer. Then there are, besides the University of the Philippines, literally dozens of universities in the Manila area that are private enterprises. There is a very good Jesuit University in Manila. It doesn't have engineering, but it has chemistry, physics and mathematics. It has probably the best meteorology group in the Philippines.

HEITMANN: Why is that, do you think?

WARNER: Well, the Jesuits, you know they just started it. They started a little observatory and got some priests that were meteorologists and were interested in it. A physics professor did a very good job putting an elementary physics course on TV. It's a high-grade place. There's another run by the Jesuits which is not nearly as highly developed down in Davao City, which is a city down in the southeast corner of Mindanao.

HEITMANN: So, you did a lot of work in the Philippines, in Pakistan, Afghanistan, India—any other places?

WARNER: Well, all of Southeast Asia. I was the American representative on the Board of Senior Advisors of a study sponsored by UNESCO and International Association of Universities

and started out as being a study of nine Southeast Asian countries, of which the Philippines was one. Ford Foundation was putting some money into that; that's the reason I was the American representative, I'm sure. But this was Burma, Singapore, Indonesia, Malaysia, Thailand, the Philippines, South Vietnam, and originally, Laos and Cambodia had been in it. Well, they were so far in the war at this time that they dropped out so we had only seven rather than nine countries. Well, we met only about two times a year while that study was going on at our staff headquarters at the University of Malaysia. We even had a Russian on this Board of Senior Advisors, a geologist.

HEITMANN: That's part of UNESCO, right?

WARNER: Then it was after that the Philippine work came on. I spent a lot of time in the Philippines. We originally went to Australia, Harry Case, and I, hoping we could get the Australian universities involved in Southeast Asia. Get them to take on the job of building the engineering college at the University of the Philippines, as a graduate and research institution. Part of it was to develop something like the Mellon Institute, cooperation between the university and the industries. But to get graduate work started, we were supposed to take promising young people on the faculty and bring them over to America and put them in good universities to get a doctorate, and then to go back. We also sent senior professors from here over there, to help guide them and reform their undergraduate curricula and get graduate work going. Building up their library if necessary and building up their research equipment. Altogether Ford put over six million dollars into the University of the Philippines. Well, the Australian universities were meeting; their vice-chancellors were meeting in Canberra. We went down and told our story to them. They didn't turn us down, but it was pretty obvious that they'd have to deliberate about it maybe a year before they'd make up their minds. That didn't suit the Ford Foundation. The result of that was, we both were coming back to the United States from Australia and Harry, who was coming back to New York to report to the Foundation said, "I think the only way to solve this, Jake, is for you to go back and find some way to do the job." I came back and organized a consortium of universities, but organized also a non-profit administrative agency called Educational Projects Incorporated to do the administrative work on this. We had Iowa State, Minnesota, Purdue, Indiana, Case Western Reserve, University of Rochester, Penn, Pitt, Carnegie Tech, Maryland, and Northwestern.

HEITMANN: These people would not only provide advice, but they would send faculty.

WARNER: Yes.

HEITMANN: And also train young Filipinos.

WARNER: That's right. The young people we brought over here for graduate work, if there was a suitable place for them to go, in this crowd that's where we would send them. Indiana University was in this, because the same consortium did a job of building science and engineering at the undergraduate level at the new Mindanao State University and Indiana provided us with a biology teacher and also with a math teacher.

HEITMANN: You say this is probably the most noteworthy of all the things you did in retirement.

WARNER: I think the most helpful, yes. I don't know how you judge the value—sometimes you have to ask for advice and sometimes you have to actually implement a plan. This was implementing a plan. That was a great experience.

HEITMANN: And a really useful service.

WARNER: Yes. And the university agricultural school was down at Los Banos. The forestry school and the Ag school were not on the main campus of the university, they were down on a big fresh water lake that butts into Manila and down on the southeast corner of that lake is Los Baños. That's where the agriculture school and the forestry school were. That's the location of the famous International Rice Research Institute. It is supported jointly by Ford and Rockefeller. When I was there, they already had developed strains of rice with double the yield and actually, although I don't know what has happened since Marcos has become dictator, the Philippines had become a rice-exporting country instead of a rice-importing country.

HEITMANN: Exporting to all those neighbors down there.

WARNER: That's right. After that finished, that Philippine thing, right at the end of it, before I came back home, they asked me to spend a month in Thailand to make a survey of their engineering education. That was my last gasp for the Ford Foundation. The Philippine stuff wasn't quite wound up. Still, I guess for two more years there were exchanges of people, but there wasn't much new to do. The thing could be done by this educational projects group without any interference from me. Then I came back. I spent much more intensive time with the Mellon family foundations when I came back here. I stopped all this consulting business when I turned eighty. I then went back to an office on the campus for two years. I intended to do some serious writing about how I thought a good center of learning ought to be managed and governed. I got a lot of stuff in draft form. My son, Bill, has been my favorite critic of what I have to say.

HEITMANN: And also someone who is very close to the field. He could give you pretty good comments on your ideas.

WARNER: He knows how faculty feel about everything. But my wife started failing. Tech was nice. Cyert said if I would do this he would be glad to give me an office and secretarial help and a free ride everywhere. I did. I had a nice office and a secretary. I wish I could have finished that, but I doubt that I ever will, now. I must have my cataracts taken care of. I might be so much more interested in reading that I would get interested in it again, but I don't know, I think I'm too old. My wife's health took precedence over everything else. So that's the way it went.

HEITMANN: Yes. Well, thank you very much. Do you feel that this material on this tape could be used and consulted by scholars?

WARNER: Doesn't it have to be cleaned up?

HEITMANN: A bit, not too much.

WARNER: If I said anything that would make me appear an egotistical ass or something like that, I don't want my colleagues or any other people to see it and think I believe I know the answers to everything.

HEITMANN: I'll tell you what, we're through then.

[END OF INTERVIEW]

NOTES

1. Gilbert N. Lewis and Merle Randall, *Thermodynamics and the Free Energy of Chemical Substances* (New York: McGraw-Hill Book Company, 1923).
2. Peter Debye and Erich Hückel, "The Theory of Electrolytes I. Lowering of Freezing Point and Related Phenomena," *Physikal Zeitung*, 24 (1923), 185-205.
3. Warren K. Lewis, William H. McAdams, and William H. Walker, *Principles of Chemical Engineering* (New York: McGraw-Hill Book Company, 1927).
4. Cyril Norman Hinshelwood, *The Kinetics of Chemical Change in Gaseous Systems* (Oxford: Clarendon Press, 1926).
5. Peter Debye, *Polar Molecules* (New York: Dover Publications, 1929).
6. Joel Henry Hildebrand and Robert Scott, *Regular and Related Solutions* (New York: Van Nostrand Reinhold Co., 1962).
7. H. Falkenhagen, *Quantentheorie und Chemie* (Leipzig: S. Hirzel, 1928).
8. John C. Warner and Fred B. Stitt, "Conversion of Ammonium Cyanate into Urea. Mechanism Kinetic Salt Effect," *Journal of the American Chemical Society*, 55 (1933), 4807-12.
9. John C. Warner and W. J. Svirbely, "The Solubility of Biphenyl in Non-polar Solvents," *Journal of Chemical Physics*, 2 (1934), 590.
10. John C. Warner, "Objectives of the Curriculum for the Professional Training of Chemists," *Journal of Chemical Education*, 18 (1941), 560-65.
11. John C. Warner, "Is Chemical Education Too Specialized?" *Chemical and Engineering News*, 34 (1956), 4786-89.
12. John C. Warner, "Education and Research in American Universities—Some Candid Comments," *Chemical and Engineering News*, 35 (1957), 64-65.
13. Thomas McCutcheon, Harry Seltz, and John C. Warner, *General Chemistry, Theoretical and Descriptive* (New York: Van Nostrand Company, Inc., 1931).
14. Warren L. McCabe and Julian C. Smith, *Unit Operations of Chemical Engineering* (New York: McGraw-Hill Book Company, 1956).

INDEX

A

Ablard, James Elbert, 31-32, 56
Adams, Roger, 26, 72
Aerojet, 56
Alexander, Thomas Rush, 43
Allied Chemical Company, 33
American Association for the Advancement of Science (AAAS), 25
American Chemical Society (ACS), 17, 36, 39, 41, 47, 70-72
Ames, Iowa, 48-51
Ammonium cyanate, 30
Ammonium sulfate, 17
Ann Arbor, Michigan, 38, 42
Armco, 45, 66
Arrhenius Theory, 12
AT&T, 74
Atlantic Richfield Company (ARCO), 17

B

Baker, Dale, 72
Baker, Thomas, 24-25, 28, 59
Baltimore Sun, The, 24
Bancroft, Wilder D., 13
Barrett Company, 16-18
Bartlett, Paul, 39
Batavia, Illinois, 74
Beal, James Harvey, 59
Bell Laboratories, 74
Benedict, Manson, 49
Berkeley Springs, West Virginia, 72
Berl, Ernst, 25
Berl, Walter, 25
Bethe, Hans, 52
Black, Art, 67
Blenko, Walter, 62
Bloomington, Indiana, 8, 15, 21
Blough, Roger, 62
Bovard, James Moorhead, 60, 62
Bozeman, Montana, 32
Brindage, --, 15
Briscoe, Herman T., 15
Brookings Institute, 78
Brooklyn Polytechnic Institute, 46
Brown University, 39, 75

Brown, Oliver W., 10, 12-15
Buckmeister, --, 63
Bull, Robert W., 16
Bureau of Mines, 26
Bureau of Standards, 75
Burke, Admiral Arleigh A., 78
Burton thermo-cracking process, 20
Burton, Milton, 20, 48-50
Bush, Vannevar, 53

C

Cabot Corporation, 58
California Technical Institute (Caltech), 13, 30, 39, 49
California, University of, Berkeley, 13, 18, 48-50, 66, 74
California, University of, Los Angeles (UCLA), 43-44
Carnegie Corporation, 24-25
Carnegie Institute of Technology (Carnegie Mellon University), 10-11, 14, 21-28, 30-31, 34, 36, 41-47, 49, 52, 54-58, 60-65, 74-77, 81
Carrick, --, 16
Carter, Lolly, 21
Case Western Reserve University, 81
Case, Harold Clayton, 79, 81
Catalytic cracking, 20
Cebu City, Philippines, 79-80
Chapel Hill, North Carolina, 46
Chemical Abstracts, 72
Chemical and Engineering News, 71
Chicago Institute of Metals, 51
Chicago, University of, 7, 13, 37, 47-49, 51-54
Chipman, John, 66
Cincinnati, Ohio, 63, 73-74
Clairton, Pennsylvania, 17
Cohen, Edwin, 39
Colon, Angel Alberto, 56
Columbia University, 10, 38-39, 50, 64
Connor, Ralph Alexander, 72
Cope, Arthur Clay, 72
Cornell University, 10, 16-17, 19, 39, 42, 52
Cosden Oil Company, 18-20, 22
Cottrell patents, 24
Cyert, Richard Michael, 64, 83

D

Daniels, Farrington, 30, 36-37, 42, 49
Davao City, Philippines, 80
Dayton, Ohio, 47
Debye, Peter Joseph William, 13, 28, 42, 52
Deischer, Claude Knauss, 40
Dennis, Louis M., 16
Dennis-Bull process, 16
Dennison, David Mathias, 14
Derge, Gerhard Julius, 77
Detroit, Michigan, 58
Dickinson University, 57
D-limonene, 36
Dodge, Major --, 17
Doherty, Robert Ernest, 27-28, 32, 34-36, 45-47, 54, 59
Dravo Corporation, 67-68
Dresden, Saxony (Germany), 1
Dumaguete, Philippines, 80
DuPont, E. I. de Nemours & Co., Inc., 15, 33, 44, 50, 74
Duquesne Club, 62
Dutcher, John B., 10

E

Eagle, Sam, 32
Easton, Pennsylvania, 2
Educational Projects Incorporated, 81
Elder, Frank R., 38
Electrochemical Society, 38
Ellis patents, 19
Emery, Alden Hayes, 71
Estermann, Immanuel, 25
Eyring, Henry, 39-40

F

Fairless, Benjamin, 61-62
Fathers of the Divine Word, 79
Ferguson, Alfred L., 38
Fermi, Enrico, 74
Firth Sterling Steel, 58
Flintcote Company, 46
Ford Foundation, 76, 79, 81-82
Fort Wayne, Indiana, 11, 21
Fox, John Gaston, 77
Francis, Charles Kenworthy, 18, 20
Franklin and Marshall College, 45

Frye, Robert, 15
Fugassi, James Paul, 30-31, 36-37, 43, 47
Fuoss, Raymond Matthew, 39
Furman, Nathaniel H., 40

G

Gabler, Raymond T., 32
General Electric Company (GE), 28, 44, 73-74
Georgetown University, 78
Georgia Technical Institute, 22
Goebels, Ingrid, 58-59
Goshen High School, 5-7
Goshen, Indiana, 1, 3, 5, 9
Graham, Herbert Winfield, 66
Greenwalt, Crawford, 50
Gulf Oil Chemicals Company, 26, 33

H

Hamerschlag, Arthur A., 23-25
Hammett, Louis Planck, 39, 55
Hancock Report on Organization of the American Chemical Society, 70-71
Hanover College, 10
Harned, Herbert Spencer, 13, 22, 40, 42
Harvard University, 8-9, 13, 15, 30, 37, 39, 45, 57-58, 79
 Business School, 59
 Medical School, 39
Haverford College, 75
Hazard, Leland, 59
Heidelberg, University of, 10
Heinz, Henry John, 62
Henke, Clyde O., 15
Hildebrand, Joel H., 13, 28
Hill, J. Bennett, 16-17
Hinshelwood, Cyril Norman, 28
Hiroshima, Japan, 51
Hirschfelder, Joseph, 37
Hitler, Adolf, 52
Hogness, Thorfin Rusten, 37
Holland, Michigan, 32, 76
Hope College, 32
Hückel, Erich, 13
Hufford, Mason E., 10

I

Illinois Technical Institute, 40
Indian Institute of Technology, 77
Indiana University, 6-10, 12, 14-15, 17, 21, 30, 77, 81-82
International Association of Universities, 80
International Rice Research Institute, 82
Iowa State University, 51, 81

J

James, Doc, 46
James, Joseph Hidy, 22, 40, 44
Johns Hopkins University, The, 13, 18, 24-25
Johnson, Warren Charles, 37
Jones and Laughlin Steel, 66-68
Jones, Webster, 43-44, 46
Journal of Organic Chemistry, 72
Journal of Physical Chemistry, 72

K

Kabul, University of, 77-78
Kahlenberg, Louis, 13
Kampur, India, 77
Kennedy, Joseph, 51, 53
Kharasch, Morris Selig, 37-38
Killian, Jim, 77
Kilpatrick, Martin, 40
Kilpatrick, Mary, 40
Kinetics, 13, 28, 30-33, 41, 44, 56-57
Kirkwood, Jack, 38-39, 73
Kistiakowsky, George Bogdan, 39, 56-57
Kocomo, Indiana, 58
Kohman, Truman Paul, 49, 77
Kolthoff, Izaak Maurits, 37
Koppers Company, Inc., 26
Kraus, Charles August, 39, 42

L

LaMer, Victor Kuhn, 39-41
Latrobe, Pennsylvania, 58
Lear Siegler, 76
Leiberknight, Dutch, 27
Leohtner, Max, 66
Lewis, David, 63
Lewis, Gilbert N., 13, 28
Lewis, Warren K., 14

Lipscomb, William, 37
Livermore Laboratory, 74
Los Alamos, New Mexico, 39, 48, 50-51, 65, 74
Lukens, Hiram Stanhope, 18
Lutheran Hospital, 11
Lyons, Robert E., 10

M

Madrid, University of, 56
Magnetics, 67
Malaysia, University of, 81
Manhattan Project, 45, 47-48, 52, 56
Manila, Philippines, 79-80, 82
Manns Choice, Pennsylvania, 72
Marcos, Ferdinand E., 79, 82
Margaret Morrison College, 23
Marvel, Carl S., 38
Massachusetts Institute of Technology (MIT), 13, 39, 49-51, 66, 72, 77-78
Mathers, Frank C., 10, 15
Matthews, Joseph H., 37
May, Clarence E., 10
McCabe, Charles Law, 57-59
McCabe, Warren Lee, 45-46, 57
McCutcheon, Thomas Potter, 40
McKinney, David Scroggs, 31-32, 43, 77
McNutt, Paul, 21
Mehl, Robert Franklin, 45, 57, 66
Mellon Institute, 26-27, 81
Mellon, Andrew William, 63
Mellon, Paul, 27
Mellon, Richard B., 63
Mellon, Richard King, 27, 60, 62-63
Merck and Company, 65
Merck, George, Jr., 44
Michigan, University of, 3, 14, 45-46, 66
Middletown, Ohio, 66
Mindanao State University, 82
Mindanao, Philippines, 79-80
Minnesota, University of, 37, 64, 75, 81
 Department of Aeronautics and Engineering Materials, 75
Moelwyn-Hughes, E.A., 40
Monrad, Carl Corydon, 46
Monsanto Company, 47-48, 53-54, 65, 74
Montana State University, 18, 32
Murphy, Walter Joseph, 71

N

Nagasaki, Japan, 51
Nappanee, Indiana, 1
Naptha, 18
Nathanson, Jonas Bernard, 27
National Academy of Sciences, 39, 49-50, 57, 73-74
National Council of Churches, 80
National Science Foundation, 64
National Security Associates, 78
Naval Ordnance Lab, 56
Negros Island, Philippines, 80
New Mindanao State University, 79
Newman, Melvin Spencer, 44
Northwestern University, 81
Norton Company, 58
Norton, Frederick Harwood, 49
Notre Dame University, 48
Noyes, A. A., 13
Noyes, W. A., 13, 72

O

Oak Ridge Laboratories, 48-51, 53-54, 58, 65, 73-74
Office of Naval Research, 56
Office of Scientific Research and Development (OSRD8), 56
Onsager, Lars, 39
Oppenheimer, J. Robert, 48
Ordinance Department, U.S. Military, 17
Oxford University, 40

P

Pakistan Universities of Technology, 77
Panic, Milan, 68
Parsons, Charles Lathrop, 71
Pauling, Linus Carl, 30
Pennsylvania, University of, 10, 13, 16-18, 22, 40, 43, 45, 72, 81
Perrin Memorial Lectures, 76
Peshawar, University of, 77
Pharmacia, 71
Phenol, 16
Philadelphia, Pennsylvania, 16, 62
Philippines, University of the, 79-81
Pittsburgh Golf Club, 61
Pittsburgh Plate Glass Company (PPG), 59, 67-68
Pittsburgh, Pennsylvania, 7, 14, 17, 23, 26, 41-42, 58-59, 61-63, 67, 70

Plutonium, 47-52
Prager, William, 75
Price, Bill, 62
Price, Charles, 40
Princeton University, 39-40, 45
Procter and Gamble Company, 15
Puerto Rican Industrial Development Corporation, 56
Puerto Rico, University of, 56-57
Purdue University, 15, 81

Q

Quantum chemistry, 13
Quezon City, Philippines, 80

R

Rabi, Isidor Isaac, 73-74
Rath, Frank, 68
Research Corporation of America, 25
Rhodes, Dusty, 17
Roberts, George Adam, 58
Rochester, University of, 46, 81
Rockefeller Institute, 39
Rohm & Haas Company, 72
Romulo, Carlos, 79
Ryerson, Lloyd, 37

S

San Carlos, University of, 79-80
Sanders, Harry, 9-10
Sanders-Egbert Company, 3, 9
Scaife, Alan Magee, 63
Scaife, Sarah, 63, 70
Scatchard, George, 39-42, 73
Schlesinger, Hermann Irving, 37
School Science and Mathematics, 7
Seaborg, Glenn T., 48-51, 53
Seitz, Fred, 45, 47, 53, 58, 73
Seltz, Harry, 9, 22, 27-28, 40, 42-43, 45, 47, 56
Sharpie, Bob, 58
Shedlovsky, Theodore, 39
Sherwood, --, 19-20
Silliman University, 79-80
Simon, Herbert Alexander, 63
Skibo Castle, Scotland, 24
Smith, Cyril Stanley, 51, 53

Smith, Edgar Fahs, 17, 40
Smith, Elliott Dunlap, 59
Smith, Lee, 16
Smith, Lee Irvin, 37
South Bend, Indiana, 3, 9
Southern Illinois University, 16
Spang and Company, 67-68
Spedding, Frank Harold, 48, 51
St. Louis, Missouri, 53-54, 63
St. Thomas University, 79
Standard of California, 32
Standard of Indiana, 46
Steinmetz, Charles P., 28
Stempel, Guido H., 47
Stern, Otto, 25, 27, 45
Stever, Horton Guyford, 64
Stitt, Fred B., 30
Svirbely, William J., 31-32
Swarthmore College, 8-9
Szilard, Leo, 52

T

Tata Iron and Steel Company, 76
Taylor, Hugh, 40
Teledyne, 58
Teller, Edward, 52-53
Texas A & M University, 15
Texas, University of, 15
Thomas Hochwalt Laboratory, 47
Thomas, Charles Allen, 47, 53-54
Tishler, Max, 44
Tome School for Boys, 24
Trinitrophenol, 16
Tuttle, Malcolm, 19

U

UCAR (Union Carbide Corporation), 58
UNESCO, 80, 81
Union League Club, 63
United Nations Security Council, 79
United States Agency for International Development (USAID), 77
United States Congress, 74
United States Department of Agriculture
 Eastern Laboratory, 16
 Western Regional Laboratory, 30

United States Department of Defense, 62, 73
United States Naval Research Lab, 45
Universal Oil Products, 71
Updike, --, 21
Upjohn, See Pharmacia
Uranium 235, 39, 48, 50-51, 53, 56
Urey, Harold Clayton, 18, 39
US Steel, 26, 58, 61-63, 68

V

Vanadium Alloy Steel, 58
Van der Meulen, Peter Andrew, 17
VJ day, 54
Volwiler, Ernest, 72
Votaw, Verling Milton, 15

W

Warner, George Washington, 5, 7
Warner, John C.
 brother, 3-5
 brother (DeMain), 4-5, 9, 55
 father (Elias), 1, 3-5, 9-10
 grandfather (John Plank), 2, 4
 grandfather (William Warner), 1-2
 grandmother (Elizabeth Enders Warner), 1
 grandmother (Lydia Mehl Plank), 2, 4
 grandson (William Warner), 3
 Horatio Alger Award, 73
 mother, 2-5
 sister, 4
 son (Thomas Payton), 76
 son (William), 1-2, 75, 82
 uncle (John Christian Mehl), 2
 uncle (William Warner), 2
 wife (Louise Hamer), 2, 9, 21-22, 37, 54, 60, 69, 72, 75, 79, 83
Washington and Jefferson College, 43, 72
Washington, D. C., 64
Washington, Indiana, 8
Weidlein, Edward Ray, 26-27
Wellesley University, 53
Wells, Herman B., 15, 77
Werner coordination compounds, 43
West Virginia, University of, 22
Westinghouse Electric Corporation, 51, 62, 68-69
Wigner, Eugene Paul, 52-53

Wigner, Mary, 53
Williams, Rod, 56
Williams, John Warren, 37, 42
Wilmington, Delaware, 15
Wilson, Charles, 62, 73
Winstein, Saul, 43, 44
Winstrom, Leon Oscar, 32-33
Wisconsin, University of, 10, 30-31, 37, 42
Wöhler reaction, 30
Works Progress Administration (WPA), 68
World War I, 16
World War II, 15, 28, 30, 33-34, 39, 42-43, 45-46

Y

Yale University, 13, 39-40, 59
Yenne, Harlan, 8
YMCA, 19-20