CHEMICAL HERITAGE FOUNDATION

MANSON BENEDICT

Transcript of an Interview
Conducted by

James J. Bohning

at

Naples, Florida

on

24 January 1991

(With Subsequent Additions and Corrections)

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY
Oral History Program

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Manson Benedict, interview by James J. Bohning at Naples, Florida, 24 January 1991 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0088).

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

MANSON BENEDICT

1907	Born in Lake Linden, Michigan on 9 October

Education

1928	B. Chem., Cornell University
1930-1931	University of Chicago
1932	M.S., physical chemistry, MIT
1935	Ph.D., physical chemistry, MIT

Professional Experience

1929-1930	Research Chemist, National Aniline and Chemical Co.
1935-1936	National Research Council Fellow, Harvard University
1936-1937	Research Associate in Geophysics, Harvard University
1937-1938	Research Chemist, National Aniline and Chemical Co.
1938-1943	Research Chemist, M. W. Kellogg Company
1943-1951	Director, process development, Hydrocarbon Research Inc.
1951-1952	Chief, Operational Analysis Staff, Atomic Energy Commission
1951-1957	Scientific Advisor, National Research Corporation
	Massachusetts Institute of Technology
1951-1969	Professor of Nuclear Engineering
1958-1971	Head, Nuclear Engineering Department
1969-1973	Institute Professor
1973-	Institute Professor Emeritus
1979-1985	Director, Burns & Roe, Inc.

Public Service Activities

1948-1958	Reactor Safeguard Committee (later Advisory Commission on Reactor Safeguards), U.S. Atomic Energy Commission
1958-1968	General Advisory Committee, U.S. Atomic Energy Commission
1964-1988	Advisory Council on Radiation Protection, Massachusetts
1973-1975	Energy Research and Development Advisory Council, Federal Energy Administration

Honors

1947	William R. Walker Award, American Institute of Chemical Engineers
1963	Industrial and Engineering Chemistry Award, American Chemical Society
1966	Perkin Medal, Society of Chemical Industry, American Section
1966	Founders Award, American Institute of Chemical Engineers
1968	Robert E. Wilson Award, American Institute of Chemical Engineers
1969	Arthur Holly Compton Award, American Nuclear Society
1972	Enrico Fermi Award, U.S. Atomic Energy Commission
1975	John Fritz Medal, American Institute of Chemical Engineers
1976	National Medal of Science
1976	Founders Award, National Academy of Engineering
1983	Glenn Seaborg Award, International Platform Association

ABSTRACT

Manson Benedict begins the interview with a discussion of his family background, including the highlights of his father's career in chemistry. He recalls how his early enthusiasm for chemistry was promoted both by his father's work and his summer jobs with Calumet and Hecla Copper Company. He then tells of his dissatisfaction with his Cornell University education, his year at National Aniline, and his decision to enroll at the University of Chicago to obtain a broader liberal education during which he explored economics and socialism. After a colorful description of a summer's work on a fruit farm in Washington state, Benedict describes his enrollment in a graduate physical chemistry program at MIT. He then discusses his National Research Fellowship at Harvard and his decision to work at Kellogg, where he developed the Benedict-Webb-Rubin equation. He describes his significant role in the Manhattan Project, and touches on his subsequent appointment to the Atomic Energy Commission. He concludes with his return to MIT to develop a nuclear engineering curriculum, the accomplishment of which he is most proud.

INTERVIEWER

James J. Bohning, Assistant Director for Oral History at the Chemical Heritage Foundation, holds the B.S., M.S., and Ph.D. degrees in chemistry. He was a member of the chemistry faculty at Wilkes University from 1959 until 1990, where he served as chair of the Chemistry Department for sixteen years, and chair of the Earth and Environmental Sciences Department for three years. He was Chair of the Division of the History of Chemistry of the American Chemical Society in 1986, and has been associated with the development and management of the Foundation's oral history program since 1985.

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8 National Aniline and Chemical Company
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10 University of Chicago
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29 Harvard University
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34 National Aniline and Chemical Company
Studies kinetics of oxidation of benzene to maleic anhydride.

37 M. W. Kellogg Company/Polymerization Process Corporation (POLYCO)
Develops Benedict-Webb-Rubin equation of state for gases and continuous flow calorimeter. Works on separation of hydrogen from gas mixtures.

46 Manhattan Project
Works for Kellex, a subsidiary of Kellogg, as head of process development. Charged with developing gaseous diffusion cascade and designing a plant for separating uranium at Oak Ridge. Also involved in building and operating the plant.

54 Hydrocarbon Research, Inc.
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INTERVIEWEE: Manson Benedict

INTERVIEWER: James J. Bohning

LOCATION: Naples, Florida

DATE: 24 January 1991

BOHNING: Dr. Benedict, I know you were born in Lake Linden, Michigan, on October 9, 1907. Can you tell me something about your parents and your family background?

BENEDICT: My mother [Lena Manson] was born in Syracuse in 1872. She assisted her father, who was in the jewelry business. At the time she wanted to go to college and despite her father's serious objections, she accepted a New York State Regents Scholarship and was admitted to Cornell University. She studied for a couple of years before lack of funds required her to terminate her education after two years of study under Professor Hiram Corson, an eminent authority on the poet Robert Browning, of whose poetry she was inordinately fond.

My father was born in 1876 in Pittsburgh, Pennsylvania, of German immigrant parents who in a moment of misguided patriotism decided to give him the first name of Centennial because of the year of his birth. It was a name which he hated and as a result he always went by his second name, which was Harry. So he signed himself "C. Harry Benedict." He attended the public schools of Pittsburgh and received a scholarship which enabled him to study chemistry at Cornell University under Professor Louis Munroe Dennis, an eminent inorganic chemist. After a year of graduate study at Cornell, through Dennis's assistance, he received an offer of employment as metallurgist for what at that time I think was just called the Calumet and Hecla [Consolidated] Copper Company, in the upper peninsula of Michigan. So without any prior knowledge of what he was letting himself in for, he took this job in a rather remote and somewhat isolated region in the upper peninsula of Michigan.

The copper company there had been in operation for some fifty years and had acquired an enormous pile of copper tailings which still contained some one percent copper because of the inefficient nature of the extraction processes which had been used in the past. The processes which had been used in the past were all based on the difference in density or specific gravity of particles containing copper and those which just contained ordinary sand, and when the copper content of the sand got down to something like between half and one percent, there wasn't a sufficient difference in density to make it possible to provide more efficient separation. So the company decided that some chemically based process would be required to complete the extraction of the copper and engaged my father to develop this process.

Well, he put his knowledge of chemistry from Cornell to good use, and found that acid extraction of the copper would not work very well because native copper is not soluble in acids without some oxidant. However, the copper itself was soluble in solutions of cupric ammonium carbonate, which could be made from native copper itself. So he developed a leaching process, which involved extracting the copper with cupric ammonium carbonate and then taking the leach liquors which contained the extracted copper, but in cuprous form, distilling off the ammonia to recover copper oxide, and reusing the ammonia to make more cupric ammonium carbonate. That part of the process is out of my mind now, but at any rate it was based on the extraction of the ore with cupric ammonium carbonate and the recycle of the ammonia (1). This enabled the company to recover some thirty million pounds of copper which otherwise would have gone to waste and was an important factor when World War I started.

My father's involvement in chemistry and his providing chemicals for my brother and me to work with in a basement laboratory caused me to become very enamored of chemistry, and I had no doubt whatever that I would use chemistry as my professional career. After graduating from high school in 1923 and spending a year improving my undergraduate education and getting old enough to go to college by attending Shadyside Academy in Pittsburgh, Pennsylvania, I entered Cornell University as an undergraduate in the fall of 1924.

BOHNING: Then you had all of your public school education in Michigan?

BENEDICT: All of my public school education was in Lake Linden, Michigan, that's right. But I did spend one year at Shadyside Academy in Pittsburgh, which was pretty much a matter of marking time. It enabled me to grow up enough so that I was tall enough not to have to wear short pants when I went to Cornell [laughter], and acquire a certain feeling of self confidence that enabled me to live away from home. Because of the year in Pittsburgh (I had spent weekends with my father's brothers in Pittsburgh) that was kind of a transition period between being a small boy in a small town in Michigan and being a more mature person in the larger world.

BOHNING: Did you have any other chemistry experience besides what your father was doing for you in your own laboratory, such as in high school?

BENEDICT: I did have a succession of summer jobs, mostly through the assistance of my father in getting employment with various divisions of the Calumet and Hecla Copper Company. The first job I had was assisting in the laboratory in taking samples of coal, which was delivered in about a hundred shiploads per year at the company's docks. We used a procedure called "coning and quartering" to obtain a small sample of coal from a hundred tons or more which was delivered to the docks. We examined the coal by measuring the heat of combustion and we measured the sulfur content in the coal and determined whether it met the company's specifications. I also took samples of copper ore and copper concentrates at various stages in the company's processing of the copper from coarsely crushed ore down through fine sand and tailings, and assisted in the analysis of the materials and the leach liquors from the leaching process for copper content.

This was good experience and it just increased my interest in chemistry, which had been initiated by the basic chemistry laboratory that my father equipped for me with a ChemCraft set and additional real acids and alkalis from the company's laboratory, which were not permitted to be sold in the ChemCraft sets. [laughter] (You're probably familiar with what the ChemCraft sets consisted of.) So, there was never any doubt in my mind that I wanted to study chemistry, and I went to Cornell where my father himself had graduated and where he had met my mother. I spent all four years of undergraduate education at Cornell and did my undergraduate research under the same professor with whom my father had worked, Professor Louis Munroe Dennis.

BOHNING: I am interested in what Cornell was like at the time you were there, what the curriculum was like, and the courses you took.

BENEDICT: I was so completely enamored of chemistry that I signed up for the bachelor of chemistry program. The bachelor of chemistry program was the particular favorite of Professor Louis Munroe Dennis, the head of the chemistry department. He filled it so completely with chemistry subjects, that I received a very poor liberal education. Very little history, literature, mathematics, or more advanced physics was required in the program. But the courses were such things as qualitative analysis, which was really quite helpful because it was a good way of learning inorganic chemistry; quantitative analysis, which would have been useful if I had worked as an analytical chemist in industry but wasn't all that valuable as a knowledge of chemistry; and organic chemistry, which was valuable, taught by Professor William [Ridgely] Orndorff, a co-author of what was then the standard text in organic chemistry, Remsen and Orndorff (2). I don't know if you're familiar with that book or not.

BOHNING: Yes.

BENEDICT: It didn't involve very much modern organic chemistry because Orndorff was an old man at that time. Physical chemistry was taught by Wilder Dwight Bancroft, who had very little respect for J. Willard Gibbs and many of the more advanced ideas in physical chemistry. [laughter] There were a lot of required courses in such subjects as gas and fuel analysis and other things which might have been helpful to me had I been an analytical chemist in industry, but were very poor substitutes for a broader liberal education or a broader scientific education. The physics courses I had at Cornell were also very primitive and involved practically no introduction to quantum mechanics, which was just then becoming an important branch of physical science. So it was really a rather poor and one-sided chemistry education, and contained so very little of the liberal arts subjects, that I was really very poorly equipped to become an effective member of modern society.

BOHNING: Did you do any research with Dennis?

BENEDICT: Yes, I did some research with Dennis which was very interesting and frustrating. Dennis was very fond of me, partly I guess because he remembered with pleasure the association he had with my father twenty-five years earlier, but also partly because I was so wrapped up in chemistry and was such a good student in the chemistry subjects that I was taught. In my senior year he offered me an opportunity to do research on a problem which had challenged him for the some thirty years he had been at Cornell, the production of carbon monosulfide. Dennis was this great believer in the regularities of the periodic table, and reasoned with some justification that since carbon produced carbon monoxide, a gas with a boiling point of about -185°, carbon dioxide, a gas with a boiling point of about -78°, and carbon disulfide, a liquid with a boiling point of about 35°, that by analogy there ought to be a carbon monosulfide with a boiling point somewhere around that of carbon dioxide, about -75°. He'd had a number of students trying to produce this gas without any great success. But he was very fond of me, I was a good student, and he decided to assign me for my senior research which was part of the chemistry program, the production of carbon monosulfide.

So, with Dennis's assistance, I tried various reactions that might have seemed successful, such as taking thiocarbonyl chloride and passing it over molecular silver to remove the chlorine. That didn't react at all. I reacted carbon bisulfide with carbon, and that didn't react at all. I tried to decompose carbon bisulfide vapor by passing it through an electric arc. I designed a small glass vessel in which I inserted two carbon electrodes, put liquid carbon bisulfide in it, cooled it down to

-110° Centigrade, the freezing point of carbon disulfide, and struck an arc between the carbon electrodes and took the gas which came off into a cold trap cooled with liquid air. I got a white condensate, which certainly contained some vaporized carbon bisulfide, but which I hoped would also contain carbon monosulfide because its condensing temperature would have been 80° or 90° Centigrade higher than the liquid air temperature. Then I was going to vaporize this mixture and actually distill the carbon monosulfide away from the carbon bisulfide. So when I came to heat up the mixture, instead of getting anything off of the contents of the vessel that I'd condensed the reaction products in, it blew up with a flash of light. [laughter] So it was clear that I had something new, but it wasn't the stable compound that Dennis predicted carbon monosulfide would consist of.

When it blew up it was converted into a brown reaction product, which I took out and analyzed by combustion analysis, and it had one atom of carbon to one atom of sulfur. It was quite apparent that I had produced an unstable species, carbon monosulfide, which when it was allowed to warm up, polymerized into a polymeric form with the same chemical formula. I was highly elated because I was convinced that the first product I had made was carbon monosulfide. I had some ideas, which probably wouldn't have been successful, about how to separate this from the unreacted carbon bisulfide and prove by spectroscopy or some other means that it was carbon monosulfide. But Dennis would have none of it. He was convinced that carbon monosulfide would be just as stable as carbon monoxide and that the product that I had made was something that he hadn't wanted and wasn't interested in. So he told me to give up that line of research and try various other reactions, none of which were successful.

The result of all this was that I spent a year at Cornell after I got my bachelor's degree trying some of Dennis's proposed reactions, none of which worked, and becoming more and more convinced that I was on the wrong track, that chemistry wasn't all that fascinating after all, and that the world, which was by then going into a severe economic depression, had more needs for somebody as a social scientist than as a chemist and that I was on the wrong track all along. So, after a year of unsuccessful research under Dennis, I gave up chemistry temporarily.

BOHNING: Did you spend that one year just doing the research for Dennis?

BENEDICT: Yes.

BOHNING: Did you take any courses?

BENEDICT: Yes, I did take some courses.

BOHNING: At the graduate level?

BENEDICT: No, they were some of the undergraduate courses that I had not taken before.

BOHNING: I see.

BENEDICT: Let me see what I have to say about that in my notes. There's some interesting stuff in here that I should have told you. May I go back?

BOHNING: Certainly.

BENEDICT: One of the reactions Dennis had me try was reacting thiophosgene, $CSCl_2$, with molten sodium. That didn't work. But, as I wrote here, I had a nasty disposal problem that I solved in a singularly inappropriate way. I took my fifty grams of unreacted sodium to the center of the bridge over Fall Crick and dropped it into the water. [laughter] The hydrogen from the reaction exploded with a loud boom, and the sodium burned with a bright yellow flame and a cloud of sodium hydroxide. [laughter] To dispose of the thiophosgene, a couple of my colleagues and I took sealed glass bottles containing around a liter to the northwest corner of a field beyond Forest Home, which was an outlying suburb. We thought a southwest wind was blowing towards some woods, and we laid the bottles tenderly on the ground and at a safe distance threw rocks at them. After they were broken we walked back to the car a hundred yards away. To our dismay we found that the wind had shifted and was blowing the smelly, toxic stuff towards us and a nearby house. [laughter] These were some of the things that an immature chemist did. [laughter]

BOHNING: How did you support yourself in that fifth year at Cornell, after you had your degree?

BENEDICT: Partly through my parents, and partly through the summer employment I'd had at the Calumet and Hecla Copper Company's laboratories. That summer employment was really excellent experience. The first year, I simply was sample boy in the laboratories and took samples. I took samples of the copper ore and tailings and solutions in the copper ammonia leaching plant that my father had developed and put into operation for the company (3). I took the samples to the laboratory and analyzed them for copper and other materials.

This ties into my Cornell education, though. Professor Dennis at Cornell was an authority on the chemistry of germanium. He had done a lot of his own personal research on that. I was interested to see if there was any germanium in the copper material. We found traces of it, but never enough to have involved an extraction process for removing the germanium from the copper ores.

There were some minor constituents of the copper ore that were different from the native copper which the company was geared up to separate. I was interested in getting samples of some of these. There were copper arsenides, and these were processed in the company smelter. I can't remember how it worked, but at any rate there were some by-product materials at the smelter, which might have contained traces of the element germanium that was of interest to Dennis. So I got some samples of these and took them to the laboratory there and had them analyzed and found that they did indeed contain traces of germanium. I got samples of copper arsenides that were in the ore in the first place and found that they had minor amounts of germanium in them also. Dennis was looking for a domestic source of germanium, because most of the germanium that he had worked with in the past came from Germany. But none of these were of high enough concentration to have been of any interest to the company to have extracted separately for Dennis.

[END OF TAPE, SIDE 1]

BENEDICT: I haven't thought of this for a long time. This was back in the 1920s, so it's sixty-five years ago! [laughter]

BOHNING: Yes, but it's fascinating, it really is.

You were spending that fifth year at Cornell when the Depression came, and that's when you took your temporary break.

BENEDICT: That's right. It was the fifth year at Cornell, and I was trying to produce carbon monosulfide and had this really serious disagreement with Dennis who wasn't at all interested in my pursuing the research that I had started and would, I guess, eventually have convinced both him and me that I had temporarily isolated carbon monosulfide, the material which was later analyzed spectroscopically while still in the vapor stage and proved by people that knew more about spectroscopy than either Dennis or I did that it was carbon monosulfide, but in a transient form (4). So I gave up that line of research.

BOHNING: Is that when you went to National Aniline?

BENEDICT: Yes. I became very disenchanted with what I'd been doing. In the summer of 1929, after my unsuccessful year of graduate study at Cornell and I hadn't been able to produce carbon monosulfide to Dennis's satisfaction, my father and I became convinced that I'd spent enough time in school and an industrial job would help me decide what I wanted to do with my subsequent life. So, through father's assistance, I obtained a job with the National Aniline and Chemical Company in Buffalo, New York, where I went to work in the fall of 1929.

BOHNING: How did you find that job? Were you looking at other places, too, or was that your father's influence?

BENEDICT: That was my father's influence. I was still pretty immature and I didn't have a very good idea about how I went about getting a job. This seemed like a good way of getting my feet on the ground.

I had applied to Princeton University to study physical chemistry under Hugh Taylor, because I realized that my Cornell education was so heavily slanted towards inorganic chemistry that the more fundamental aspects of chemistry were something that just weren't properly taught at Cornell at all. (I'm digressing a little here but I think it's pertinent.) The physical chemistry at Cornell was the brainchild of Wilder Dwight Bancroft, a physical chemist who had no patience whatever with the more quantitative aspects of physical chemistry. He didn't teach thermodynamics, à la Gibbs at all, and was more interested in colloid chemistry, rather than in more fundamental aspects of chemistry which were just then becoming really vital topics. I guess that was when J. Willard Gibbs was making his contributions and these weren't taught at all at Cornell. So I didn't get any modern chemistry and quantum mechanics and quantum mechanical explanation of so much of the chemical properties, and had a rather one-sided chemical education.

After my unsuccessful year of research on carbon monosulfide at Cornell, father and I decided that I had spent enough time in school and that an industrial job would help me decide what I wanted to do. So he got me a job with the National Aniline and Chemical Company in Buffalo, New York, rather than my taking an assistantship which I had been offered and accepted at Princeton, where I'd hoped to study physical chemistry under Hugh Taylor, which would have really been a very good education indeed. [laughter]

BOHNING: Yes.

BENEDICT: I went instead to Buffalo, New York, and started work at thirty-five dollars a week at the National Aniline and Chemical Company plant in south Buffalo. Well, I'm going to read this to you, because it's so typical. "It's hard to imagine a more industrially polluted place than that part of Buffalo in these pre-environmental protection days, plus Buffalo Creek on the west side of Abbott Road where steel mills whose blast furnaces belched sulfurous clouds of ore dust, and whose sewers poured rust-stained water into the creek. Across Abbott Road was General Chemical's plant, whose sulfuric acid towers emitted choking fumes of sulfur dioxide and whose nitric acid converters discharged brown clouds of nitrogen dioxide that quickly corroded lampposts and auto bodies." It was a horrible part of the world, it really was.

BOHNING: That's incredible.

BENEDICT: "I had three months of night shift work in the analytical laboratory, where I worked either on the nitro bench or the indigo bench. [laughter] At the indigo bench I got my hands and clothes stained with partially processed indigo, and on the nitro bench I inhaled too much dichloronitrobenzene and had to be seen by the company physician because my fingernails turned blue."

BOHNING: My gosh.

BENEDICT: Yes, that was quite an experience. But I used my shift work to good advantage by spending the days at the Grovenor Library. I don't suppose you're familiar with Buffalo geography?

BOHNING: Not that well.

BENEDICT: The Grovenor Library was sort of a research library where you could browse more conveniently than you could in the public library. I discovered a lot of the books that I had not been permitted to take in courses in literature at Cornell. So the combination of having shift work at National Aniline and the availability of a good library of classics of literature and poetry enabled me to complete by self-education some of the liberal education I had been denied at Cornell because there were too many courses in analytical chemistry. [laughter] That proved to be useful supplementary education. But, at the same time, the Depression worsened and there were long lines of people. I don't remember exactly what the bread lines were in those days, but at any rate I knew that the world was in an economic depression. I became very dissatisfied with my own lack of a liberal education at Cornell, which was obviously not going to help me deal with the serious problems of the world, so I decided that I'd use my savings from my year's employment at National Aniline to go back to school and round out my own liberal education.

How did I choose the University of Chicago? I gave up my employment in Buffalo in August of 1930, and went back to northern Michigan to spend a month with my family there while I decided what I was going to do next. I'll read this now because it is pertinent here. "I realized my Cornell education had been so heavily concentrated on chemistry, that I was relatively illiterate in the humanities. I was acutely conscious of my need for a philosophy of life. My Cornell philosophy professor, Arthur [Edward] Murphy, had moved to the University of Chicago, and through him I gained admission there as a master's degree candidate in philosophy with the mistaken notion that by studying philosophy I could gain a personal philosophy of my own. On the last week of September I went to Chicago and found a room at five dollars a week in a rooming house run by Mrs. Roy Swift at 5412 Kimpark Avenue, just down the street a little ways from the campus of the University of Chicago. Roy Swift, a one-time semipro baseball player, was a stout, lazy man with a job in Mayor [William Hale] Thompson's Chicago political machine. Mrs. Swift was a sweet, little, hard-working housewife who earned additional pin money by making children's clothes for another Mrs. Swift of the Chicago Packing Company. Mrs. Swift was very kind to me and allowed me to use her kitchen during a period when I experimented with meals of boiled rice and beans to see how cheaply I could feed myself because I was trying to put myself through Chicago on my limited savings from prior employment. After I gave up the experiment, the Swifts invited me to a welcome dinner of porterhouse steak, potatoes, peas, and strawberries." [laughter]

I'll skip some of this. "To obtain a master's degree in philosophy I was required to start with a course in the history of philosophy. The first lecture showed me that the subject would be difficult, very technical, and quite irrelevant to giving me a philosophy of life. [laughter] So I dropped the course and the plan to obtain a master's degree in philosophy, and decided instead," and I say parenthetically, wisely, "to spend a year getting some of the liberal education I had missed at Cornell. I signed up for some stimulating and valuable courses taught by eminent professors. The novelist Thornton Wilder, visiting professor for a year, lectured on the classics through translation and guided us in reading Aeschylus, Sophocles, Euripides, Dante, and Cervantes. Professor Ernest [Watson] Burgess, head of the sociology department, taught an introductory course in sociology for graduate students, a subject of increasing relevance as the Great Depression deepened. Professor Frank H. Knight, a wonderful man with an encyclopedia of information on history of cultures, gave a course in economic history titled, "Evolution of Economic Institutions and Ideas." I liked the classes, and I did fairly well in them. I wrote a sensible paper on utilitarianism, a subject about which I now know nothing whatsoever. [laughter] In the winter I took undergraduate classes in economic theory and types of economic organization and a graduate class in statistics. Economic theory proved valuable because it introduced me to the topic of elasticity of supply and demand at the substitution ratios, concepts I used to advantage many years later in 1951, in setting price scales for plutonium and enriched uranium when I worked with the Atomic Energy Commission." It's really surprising how much some of the things you do without clear understanding as a young man can sometimes prove to be helpful to you in later life. "These courses helped still later in the 1960s when at MIT I introduced a course in the economics of nuclear power. A course on types of economic organization was a special delight. It was given by Paul [Howard] Douglas, later United States senator. It dealt with the literature and practice of utopists, from Plato to Thomas More, and to Henry George's Junior Republic and [Robert] Owen's New Harmony, Indiana. I remember Douglas laughing on how Owen's high ideals floundered in the Indiana mud." [laughter] Excuse me for reading this.

BOHNING: That's fine.

BENEDICT: It's better than my imperfect memory.

"The class in statistics was valuable in acquainting me with correlation coefficients and methods of curve fitting, which I used ten years later in developing the Benedict-Webb-Rubin equation of state for gases (5). In the spring term I took a class in complex variable theory. The complex variable class proved valuable many years later when I had to use complex numbers to teach controlled stability in nuclear reactors," which at that time I had no conceivable notion that I'd ever get involved in. It's really amazing how some of the courses you take in college can, if you took them seriously, be quite valuable to you in later life. "For Murphy's class I wrote a term paper comparing the inductive approach of the Austrian physicist [Ernst] Mach with the pragmatic, operational approach of the American physicist Percy Bridgman, with whom I worked five years later." [laughter] It's another one of those coincidences.

BOHNING: That's amazing.

BENEDICT: In retrospect, the courses that I took at the University of Chicago were valuable and relevant in later life even though at the time I took them I had no notion as to where they were leading. That's why a liberal education is really such an important thing for a young person to have, because you can count on it so many times later even though at the time it doesn't seem to be particularly pertinent. Well, there's more here that I'm going to read you if you don't mind.

BOHNING: Please continue.

BENEDICT: "To get a better understanding of city life, I joined occasional Saturday trips to Chicago neighborhoods, arranged by the Fellowship for Reconciliation. The first of these trips took us to the Russian neighborhood in Chicago. Some trips were to skid row on West Madison Street where derelicts were housed and to stockyards where animals were slaughtered were pretty depressing. On the West Madison trip I had my first exposure to street corner communist agitators, who railed against the evils of Herbert Hoover and capitalism and were all too evident in the long lines of unemployed seeking jobs or shelter.

On one of these trips, I made two friends, Agnes Jacques and Saunders MacLane, who were kind and helpful to me during this difficult year in my life. Agnes Jacques was a thirty-five year old woman who worked as secretary for Paul Douglas, and served as secretary of the Chicago Society for Promoting Cultural Relations with Russia, a substitute for the then non-existent Soviet consulate in Chicago. She was fluent in Russian and conducted summer tours of the Soviet Union. I was interested in learning more about Russia, was vaguely considering spending a year working there, and wanted to learn Russian. So Agnes kindly offered to teach me the rudiments of the language. During the year we became good friends, and over tea sweetened with cherry jam, a Russian delicacy, she listened sympathetically to my uncertain search for a goal in life. One day late in the year I found Agnes in a very gloomy frame of mind and wanting someone to tell her troubles to. I was surprised that such a seemingly cheerful and efficient woman should harbor a dark pessimism that made mine seem like sunshine itself. I found it reassuring that so well-adjusted a woman should find me a suitable person to share problems with. I took her for a row in Jackson Park Lagoon and tried somewhat ineffectually to cheer her up. I never saw her again, and often wondered what the future held for her. While in Chicago I met Saunders MacLane." I don't know if you're familiar with him, but he's a very eminent American mathematician.

BOHNING: I don't know the name.

BENEDICT: I think he became home secretary for the National Academy of Sciences. At least he had an important position with the National Academy of Sciences [council 1959-1962, 1969-1972; vice president 1973-1981]. I'll read this again too. "He was a graduate student of Professor [Gilbert Ames] Bliss in the mathematics department of the University of Chicago. Like myself, he was interested in the many facets of city life and joined me on numerous excursions. On one occasion we went to a Russian bookstore and argued the merits of capitalism against two communist-favoring store clerks. We invented a game of three-dimensional chess played with two boards and two sets of chessmen. We took one of my fellow roomers to dinner and to the Chicago Opera Company performance of Die Meistersinger von Nürnberg. He and I bought four tickets to the second balcony for Tristan and Isolde. He escorted a girl from Syracuse University and I, Marjorie Eiger, a girl I met in sociology. Saunders drove us, both clad in rented tuxedos, in a borrowed, dilapidated and dirty car. [laughter] After what I described to my parents as an indescribably beautiful and moving opera, we had a post-opera supper at an old-fashioned German hofbraühaus." I'm sorry, but I just have to go on here.

BOHNING: That's fine.

BENEDICT: "After Professor Bliss described how mathematics lectures were conducted in France, Saunders called at his office the next day dressed in a tuxedo and escorted the startled professor to the classroom over whose door he had tacked a sign reading 'Salle à conférence.' He marched Bliss down the aisle, arranged the chalk, and poured the professor a glass of water." [laughter] There's just so much of this, but I don't think I ought to fill your record up with it. You're more interested in my professional education.

BOHNING: Yes, but this is really fascinating.

BENEDICT: Well, here's some more, then. "In the spring Saunders and I went on a two-day walking trip that I described as follows: 'A clear, but cold and windy Sunday morning saw us headed down Fifty-Seventh Street for the Illinois Central, accompanied by two pounds of bread and one pound of cheese, a half-pound of bacon, one pound of apples, and four pork chops. We took the train to the end of the line, and headed out into the open prairie in the direction that promised to get us fastest out of the railroad yards and squatters' shacks. About every quarter mile we passed a farmhouse with its red barn, clattering windmill, silo, straw pile, and grove of trees, all identical in composition but pleasingly different in details. Less frequently a country graveyard with unkempt graves and crazy, leaning headstones furnished opportunity for meditation. At noon we bought a quart of milk from Mr. Christiansen, lit a fire in a ditch by the road and broiled our pork chops. Near sundown we inquired of farmers if they would put us up for the night. We were beginning to get desperate when after four refusals, and the day was getting colder and darker, the Cagwins——father and mother and three fine boys——took us in, gave us supper and let us sleep on their davenport. We walked back the next day, cooked another roadside lunch, this time under a bridge in true hobo style, and arrived at sundown, having covered forty-two miles in two days.' [laughter] Saunders helped me sort out my conflicting ambitions and tried, not too successfully, to resolve my perplexities by strict mathematical logic."

Well, here's some more. "Two other friends I made through my sociology class in Chicago were Marjorie Eiger and Saul Alinsky from France. Marjorie was the daughter of a wealthy Jewish family with a home near the university. She had visited Russia with Agnes Jacques the preceding summer." (Agnes Jacques was the girl who was the local representative for the Friends of the Soviet Union, or whatever it was called.) "Marjorie was the most intelligent and sophisticated girl I had met, and I was attracted to her. She, Saul and I went to a performance of [Anton Pavlovich] Chekhov's Seagull. Subsequently, I was invited to tea at the Eiger mansion, where she indicated my future company would be welcome." [laughter] "I wrote my parents, 'The experience of spending an afternoon talking charming nonsense to a dangerously informed and personable female has its disquieting moments.'" [laughter] "After I took her to a hotel for tea, I wrote, 'Marjorie likes sterner stuff than I can produce. She and Saul have attended burlesque shows, but I will be pleased to furnish the mild and melancholy.' So I took her to a speak-easy and a lecture on Russia. She and I passed out handbills in front of a Yellow Dog Hat Shop without being arrested. When I said goodbye to Marjorie at the end of my year at Chicago, she said to me, 'The trouble with you, Manson, is that you're too timid.'" That gave me something to think about. [laughter] Oh, my.

[END OF TAPE, SIDE 2]

BENEDICT: Here's another paragraph I'm going to have to read you.

BOHNING: Okay.

BENEDICT: "In the winter term I met two bright, interesting girls in my economics class, Dorothy Jones and Rose Marcus." I'll skip about Dorothy, but "Rose was the daughter of a New York garment manufacturer and had a strong interest in labor problems. She was my companion on numerous weekend trips to see Chicago, such as to the Wilson Company's meat packing plant. She and I passed out handbills in front of the Wilson Company's plant, urging the underpaid and overworked factory girls to join a union. Although against police regulations, we weren't arrested. We went to movies and read Morris [Raphael] Cohen together." Morris Cohen was a philosopher, and I think he wrote a book on the philosophy of science, but I'm not sure (6). "Although I liked Rose, our friendship remained just a friendship. Once, after walking with her along the beach in Jackson Park, I quoted to her, 'A primrose by the river's brim, a primrose merely was to him." Her name was Rose. [laughter]

Well, here's some that's relevant. "In the winter of 1931, the economic depression sank to its worst low. The faculty and students in the social sciences were properly concerned and explored alternatives to capitalism. I read both Karl Marx's Das Kapital and Lenin's State and Revolution, but found them heavy going and uninspiring. 'But Marx,' I wrote my parents, 'how Marx does give vent to his hatreds. A combination of ponderous German wit, peevishness, and vilification makes interesting reading.' Of Lenin my comments were, 'His picture of the perfect communist society where the state is abolished and order is maintained by the social pressure of the armed workers, where sentimental intellectualism is anathema, and when deviations from type are ruthlessly coerced into lying is a bit depressing.' Nevertheless, I joined the university's socialist club and attended meetings of the League for Industrial Democracy. In the socialist club I hoped to find a cause I could support, but I wrote to my parents instead, 'I'd be pleased if I could work up a bit of enthusiasm for socialism, but alas, the field is damp on which fall the sparks of socioeconomic reform propaganda.' For the socialist club I passed handbills to people watching the fly-over of six hundred War Department planes. The handout read, 'We have a War Department, but why not a Peace Department?' Finally, in May I resigned from the club, writing my parents, 'I take no delight in meddling with the status quo.'" [laughter]

BOHNING: Did any of those activities create a problem for you in World War II when you were involved with the Manhattan Project?

BENEDICT: Yes, as a matter of fact. You're a very knowledgable person. I don't remember whether I got around to writing that up or not, but I'll try to remember what it was.

BOHNING: Well, that's one thought I had as you were describing all of this.

BENEDICT: [laughter] Yes, it surely did. In fact, I'd better tell you about that now before I forget.

BOHNING: All right.

BENEDICT: These timid ventures into socialism in 1930 came back to haunt me in 1941, when I was engaged in developing a process for extracting uranium-235 from natural uranium for the Manhattan Project. When I was required to submit a history of my previous life experiences before I could be granted security clearance to continue research, I of course gave them an account of where I had been and the courses I had studied and things of that kind, but quite properly neglected to mention I had attended meetings of the socialist club at the University of Chicago and related matters. [laughter] I was very much afraid that the investigation by the FBI would turn up the fact that I had dabbled in socialism a number of years earlier and had even considered going to the Soviet Union to see firsthand how communism worked.

"Many years later, when I was assigned by the M. W. Kellogg Company, for whom I was then employed, to work on the design of a gaseous diffusion plant for separating uranium-235 and was obliged to obtain security clearance in order to continue work, I was very much afraid that the FBI, who conducted these investigations in those days, would find out that I had attended meetings of the socialist club at the University of Chicago and had at that time even considered and made an initial contact with the Soviet chargé d'affaires in the consulate in the city of Chicago to obtain a visa to visit the Soviet Union and see how socialism operated firsthand. If the FBI ever found this out, it apparently either didn't appear in their permanent record of my past life, or wasn't regarded as a disqualification to grant me security clearance many years later.

These things all came to the fore in 1951, at the time that I had just been offered and accepted a position to teach nuclear engineering at MIT. And at the same time I was offered an opportunity to start an office of operations analysis for the then new U.S. Atomic Energy Commission. I had to submit an account of my previous life history in order to be granted a security clearance by the then new Atomic Energy Commission, and was very much afraid that these dabblings in socialism that I had undertaken ten years earlier would be cited against me and I'd be denied the clearance, denied the opportunity to work in the office of operations analysis, and would not have access to the classified information I'd need in order to obtain a good personal background for teaching a subject of unclassified matter at MIT. Fortunately, for some reason, either the FBI didn't find out about my dabble in socialism, or they didn't regard it as a detriment because I was granted security clearance. I could have access to classified information from which I made unclassified excerpts, had them submitted for approval by the Atomic Energy Commission, and was able to work them, first into notes for my class, and later into a textbook that Tom [Thomas H.] Pigford and I published, called Nuclear Chemical Engineering (7)." Are you familiar with that book?

BOHNING: Only by title.

BENEDICT: Let me show you; I have a copy.

BOHNING: All right.

BENEDICT: There were two editions. The first one was published in 1957. But I was enabled to write it by having to develop a curriculum in nuclear engineering at MIT——and to teach a subject that I had developed myself, called nuclear chemical engineering. After I'd been there for a year, I was joined by a man who had been an assistant professor in the chemical engineering department there, Tom Pigford, and we wrote this book. That's the first edition, published in 1957. After we'd offered the course for a number of years, we brought out a second edition in 1981.

BOHNING: That's very recent.

BENEDICT: Yes, it's still used. I still get some royalties from it.

BOHNING: There must have been considerable change between those two editions.

BENEDICT: Oh, there was. You see, what happened between the two was that there was a lot of declassification after 1957 that enabled us to provide a much more complete curriculum. And there were a lot of new developments, too. In fact, in 1957 there weren't any nuclear power reactors operating in the United States at all, and by 1981, it was the second largest producer of electricity after coal in the United States.

BOHNING: That's fascinating [looking at book]. Could we come back to Chicago and how you left Chicago. I'm assuming you went to MIT directly after you left Chicago?

BENEDICT: No. Let me get my bearings straight here. There was a very interesting interlude in my life in between them. At Chicago I had met and enjoyed the company of a graduate student in economics, which was the field I was studying, named George Shaw Wheeler, who had recently married another graduate student in economics named Eleanor Wheeler. I was really at loose ends at the University of Chicago because I found that I wasn't adept at the social sciences at all. I didn't really know what I wanted to do with my life. The Wheelers were kind to me. They invited me to their small apartment for an occasional meal, and we discussed the state of the world, which was economically pretty bad in those days.

I guess George made the suggestion that since I didn't know what I wanted to do, and wasn't particularly adept at the social sciences, why didn't I just take the summer off and do some non-mental work that might enable me to get my act together again? His parents ran a family fruit farm on the Columbia River in central Washington state, and they needed summer help. George wrote to his mother, Jeanne Wheeler, and asked whether she'd be willing to have me come out as an unpaid summer hand whom they would provide room and board for, but I would simply like to spend the summer while I decided what I wanted to do next. The Wheelers were very kind and did offer me this opportunity to come out. I accepted and went out to Washington state to take this job. But I had just about used up all the money I had saved from my brief employment in Buffalo at the National Aniline and Chemical Company (after spending a good deal of it to get my education at the University of Chicago), and I didn't want to call on my parents for more financial support, so I decided I'd hitchhike out from Chicago to the Columbia River Valley in Washington. That was a very interesting experience. I don't know whether that was in any of the chapters. Here we are. Do you mind if I read you some of this?

BOHNING: No.

BENEDICT: "In the June of my year at the University of Chicago, when I was at loose ends as to what I wanted to do, there were two events that led to a summer that cured my uncertainty and the depression that I was feeling because I wasn't getting anywhere in my plan for a life's career. I read Somerset Maugham's Of Human Bondage and I had dinner with George and Eleanor Wheeler, fellow students whose family had a fruit farm in the Columbia River Valley in Washington. In Maugham's hero Philip's release from a cruel infatuation by a summer of carefree farm work, I saw a way for me to take the advice of a psychiatrist that I'd been seeing to manipulate my environment by following another doctor I'd been seeing's advice that I spend a summer away from intellectual activity. When I mentioned my idea of a summer of farm work to George and Eleanor Wheeler, he kindly arranged for me to help in his family's orchards by doing some of the summer work he would have done had he not been obliged to remain at the University of Chicago. So I decided to hitchhike to the farm on the Columbia River in Washington owned by George's grandmother, Mary Shaw." I know that this is a lot of detail.

BOHNING: That's fine. It's fascinating.

BENEDICT: It's such an interesting and helpful experience, so I am going to read you a bit more.

BOHNING: Please do.

BENEDICT: This is better than my sixty-year-later memory. Quite incidentally, Mary Shaw was the name of my subsequent wife's own grandmother. [laughter] Of course, at that time I didn't know anything about that. "I asked my parents to send me a rubber poncho, army blanket, and knapsack I had used at camp ten years ago. I packed it with work clothes, a sweater, a spare pair of shoes, soap, shaving gear, a toothbrush and toothpaste. I ordered a copy of [John Maynard] Keynes's A Treatise on Probability to be sent to me in Washington for summer reading." [laughter]

"Early on the morning of July 7," this was 1931, "I shouldered my knapsack and took the Chicago elevated through its western terminus in Western Springs. I waited for a ride. In those days hitchhikers were considered safe company for motorists, and east of the Rockies I had no trouble getting rides. I spent the first night in Cedar Rapids, Iowa, where a kind farm family gave me a sumptuous handout for ten cents and allowed me to spread my poncho and blanket in their hayloft. I spent the second night, July 8, on the ground in a park in Omaha, Nebraska and the third night, July 9, in a tourist camp in North Platte, Nebraska, where I got some soon to be useful advice on how to hop a ride on a freight train." [laughter] "After North Platte, the country was wilder and rides were harder to come by. In desperation I went to the railroad yards, where a freight train was being made up, and climbed aboard between two boxcars." [laughter] What a crazy thing to do. "I traveled 110 miles that way until I was discovered and ordered off at the next stop. I rode the last sixty miles to Cheyenne on July 10 with a mulatto in an asthmatic model T whom I felt sure would have held me up had I not assured him that I'd ride with him again the next day after getting ten dollars I expected at the post office." [laughter]

"On July 11 I was able to get only one ride, and that only fifty-one miles to Laramie. There had been so many holdups that no one wanted to pick up strangers in that sparsely settled country. On July 12 I waited all day on the outskirts of Laramie for a ride across the Wyoming badlands, but motorists wouldn't give strangers a ride across that wild country. By late afternoon, four of us were still waiting for a ride. One of the men, Shorty Armbruster, was an experienced hobo. He had been a brakeman on a railroad until his wife died. He suggested that we join him and hop a ride on a westbound freight. I bought some oranges and went with Shorty and the others after dark to the railroad yards where a westbound freight train was being made up. With Shorty's advice on how to avoid railroad dicks, we climbed into the empty compartment at the end of a refrigerator car that would hold ice when in service, pulled down the hatch, and waited for the train to start. Fortunately, our compartment wasn't inspected and after midnight the car was coupled to the train and we started off——hopefully bound west. When we were well on our way, we opened the hatch, saw the last quarter moon on our left, and knew we were headed in the right direction." [laughter]

I'm sorry, but I just have to read you some more of this. "After I shared my oranges Shorty passed around some Lighthouse religious tracts, one of which quoted from the Rubáiyát [of Omar Khayyám]:

'Earth could not answer; nor the Seas that mourn
In flowing Purple, of their Lord forlorn;
Nor rolling Heaven, with all his Signs reveal'd
And hidden by the sleeve of Night and Morn.'"

That suited my melancholy mood very well. [laughter] "At the next division point, Green River, the train stopped for what seemed forever and we were afraid we'd have to leave our ride, but it started again. In the morning, when we came down from the desolate Wasatch Mountains into the green, irrigated farmland around Ogden, Utah, we knew what Brigham Young meant when he said to his little band of Mormon followers, 'This is the land.' I left the train in Ogden and went to a cafeteria for my first meal in twenty hours. The raspberries and cream and beef stew were the best food I'd ever tasted.

On July 14 I got a ride with a farmer in a model T Ford truck to a farm where the irrigated land gave way to sagebrush and desert." This was in southern Washington state. "Again, rides were scarce. Finally I decided that I might as well walk to the next town, and started down the hot road. Soon I came upon a car whose lady driver was having trouble changing a flat tire. Despite the mistrustful objections of her twelve-year-old son, the lady, Mrs. Henry East from Houston, allowed me to change the tire and offered me a ride west with them. I convinced them that Baker, Oregon, would be a good place for them to spend the night. I didn't tell them that I had a letter of introduction to a friend of my grandmother in Baker and that it would be a good place for me, too. At Baker I had a cordial reception from grandma's friends, the Hoskins, who kindly gave me a bed and bath for night." Now listen to this. "I told them about my travel adventures and had what was for me a new experience, the wide-eyed interest of their teen-aged daughter." [laughter] "In the morning on July 15 I drove east to Umatilla, Oregon, took a car ferry there across the wide Columbia, and rode with them to Pasco, Washington, where our routes diverged. One more ride took me as far as Richland, Washington. Richland, on the west bank of the Columbia, was thirty miles south of Hanford and three miles south of White Bluffs, where the Wheelers lived. Rides were scarce across the unirrigated desert between Richland and Hanford, so I decided to spend the night in Richland and to take the bus the next morning to Hanford."

BOHNING: Isn't that the same Hanford where the plant is now?

BENEDICT: That's the plant I'm coming to. Well, I'm going to read you some more of it because it was all so unexpected and so interesting and it's still so vivid in my memory. "In 1931, long before the Manhattan Project, Richland," (which was the headquarters for that branch of the Atomic Energy Commission years later), "was a sleepy little town with a grocery, a hardware store, and a post office on the main street near the river and just a few houses with irrigated fields on a side street running west. I walked down the side street to its end looking for an empty field to sleep in. A sunburned, frail old lady stopped me and asked what I wanted. When I told her that I was looking for a place to sleep, she asked me to wait until her husband came in from irrigating and she would see. Mr. Messur was equally old, frail and sunburned, and kindly invited me to sleep on the alfalfa in their barn. We spent a pleasant evening talking. Their son had managed a department store in Laramie, where I had stopped on my trip west. In the morning they invited me to have breakfast with them, gave me a carnation to wear, and told me to come back soon. I thanked them warmly and insisted on paying them for their hospitality. I wished I could have rewarded the Messurs as the gods did Pyramus and Thisbe." Are you familiar with Pyramus and Thisbe?

BOHNING: No, I'm afraid I'm not.

BENEDICT: Well, it is a story in Greek mythology where one of the gods disguised himself as a human and went around looking for some honest and considerate humans. He found Pyramus and Thisbe, who were an impoverished Greek peasant couple who took him in and befriended him; so as a reward, he assured them of an immortal life in the hereafter.

"On July 16, 1931, I spoiled my record of free transportation by paying two dollars and fifty cents for the bus ride from Richland to Hanford." [laughter] "There, Mr. Wheeler, who was the father of George and Eleanor, my student friends in Chicago, met me with his model T truck and drove me the last three miles to the Shaw fruit farm."

[END OF TAPE, SIDE 3]

BENEDICT: "I couldn't have come to a better place for a happy, healthy summer. The Wheeler household was a matriarchy presided over by George's grandmother, Mary Shaw, the widow of the man who started the farm." Let me stop parenthetically here to tell you that when I met my wife Marjorie Benedict, I discovered that her grandmother's maiden name was also Mary Shaw. [laughter] It seemed like a strange coincidence. "She was a Scotch lady close to eighty years old with a strength and wit rarely found in one so old. Her son-in-law, Frank N. Wheeler, did most of the work around the farm. He had been a bricklayer in Tacoma who came out to the desert country when his health failed. He was a bluff, hearty, sensitive, well-educated man, but a rabid socialist who preferred the news in Oscar Ameringer's The American Guardian to the Seattle Post Intelligencer, the local newspaper. Mrs. Wheeler, Jeanne, was a quiet, efficient, cheerful woman, who helped her mother keep house and supervise work in the fruit packing shed. Three of the Wheelers' six children were on the farm that summer——Helen, fifteen; Donald, seventeen; and Rose, twenty-four. Rose and her husband, Whit Moreford, and the three sweet Wheeler grandchildren lived in a bungalow on a farm near the river. The rest of the family lived in Mrs. Shaw's three-story house at the other end of the farm."

There's a description in here of the farm that I'll skip. "The Wheeler land sloped gently down to the beautifully clear, blue, cold Columbia River, which flowed majestically by." Are you familiar with that part of the world? Have you been up there?

BOHNING: No, I've never been up there.

BENEDICT: Then I'll read this. "The contrast with the lush, green, irrigated farmland was unforgettable. All day long the sun beamed down from the clear, piercing blue sky. Sunrises and sunsets were perpetual surprises. On exceptionally clear days we could see snow-covered Mount Rainier, a hundred miles west, pink in the early morning sunlight." Oh, what a scene!

"The days were hot. But there was a dry breeze and the nights were cool and dewless. I was comfortable sleeping out of doors on my poncho on the farm's alfalfa pile. I was there for three months and I wasn't rained on one night.

We worked six days a week and on Sunday we rested, read or swam. I learned how to harness and drive a team of horses, crank and drive a model-T Ford, run a sprayer, and pick and pack fruit without bruising it. On a typical workday, I was awakened at five-thirty by a rooster crowing in the trees over my haystack. After a swim in the nearby river, I had a hearty breakfast at six-thirty and started work at seven. Dinner was served at noon and we rested or read during the midday heat until two in the afternoon. We worked until six sharp when we had supper, and worked until dusk. After that I had another short swim and went to the haystack at nine-thirty.

Work was varied. On one day I dug a hole for a power pole. On another I weeded the strawberry patch; I also pitched hay. During July and August, while the fruit was maturing, most of

Donald's and my time was spent spraying fruit. During harvest time I picked fruit from tall ladders, putting delicate peaches into baskets and stronger apples into bags on my shoulders. I helped Mrs. Wheeler run fruit through the washer, sorted it by size and packed it lovingly in a geometric array in boxes. On Sundays and in the evenings the Wheeler children, their neighbor friends and I would picnic, sing around a bonfire, or go swimming in the river. I enjoyed the company of Louise Shin, a neighbor's daughter, and jokingly called her 'the light of my life.' On Sunday Donald and I rode across the river, climbed the steep bluffs and hunted for rattlesnakes in the desert." [laughter] "Shortly before the end of summer, to climax my adventures, I swam across the Columbia"——that's a good mile, and a strong current——while one of the local son-in-laws rowed beside me for a rescue that wasn't needed. Because of the swift current I touched the west bank three miles below where I left the east." [laughter] "The Wheelers had a good assortment of books and a Victrola with some fine old recordings. My most vivid recollection is of listening to Gladys Swarthout sing 'Che farò senza Euridice?' ['What shall I do without Euridice?'] from [Christoph Willibald Ritter von] Gluck's Orfeo [ed Euridice] and then going outside to see moonlight on the far-off hills with the haunting melody running through my mind." Are you familiar with that?

BOHNING: Yes.

BENEDICT: It's a beautiful aria.

"By the end of August most of the fruit had been harvested and it was time for me to leave. Two months of carefree, healthy work had made me strong in body and sound in mind. My adventures in traveling across the continent and becoming a productive farmhand gave me a self-confidence that, in a sense, made a man of me. My black moods were gone forever. Without much conscious effort to reach a decision, I knew that I was destined to be a chemist, and that even though the world might have more need of a social scientist, that was not my bent. In addition to the personal well-being the Wheelers gave me, they showed me a kind of American life now fast disappearing——the family farm. Even though the Wheelers scarcely stayed solvent financially, they led a healthy, productive, self-reliant life in beautiful surroundings. I didn't exaggerate when I wrote my parents that White Bluffs was 'a little bit of Eden,' and that my summer there 'will always be a bright spot in my life.'" And indeed it was.

BOHNING: That's excellent.

BENEDICT: Well, there's a sequel to this that's very poignant. George Wheeler was an economist, and when the war started, there was an agency in Washington that was set up to sort of streamline American industries for the most efficient productions in the conduct of the war. He went to work for that agency, whose name I can't remember. That led him into government service of various sorts. He was subsequently assigned to be the economic attaché after the war at the U.S. embassy in Prague, the capital of Czechoslovakia. And while there, and I don't know if the fact that he had temporarily joined the communist party earlier in life or something——at any rate it led to his either actually being fired from his job or his being insecure and knowing that he would lose his job. So he defected to the Soviet Union and wrote some kind of a letter that was widely publicized, criticizing the U.S. government for its conduct of the war and showing that he was far more sympathetic to the Russian way of life than he was to the U.S. way of life.

I was horribly afraid that my application for clearance by the U.S. Atomic Energy Commission would be denied when it was found that I had been a friend of George Wheeler and that I had worked for his parents' fruit farm in Washington where his father was a rabid socialist. [laughter] I, of course, had told the interviewers for my clearance all the places that I had worked, but I hadn't mentioned the fact that Mr. Wheeler was a socialist or that his son had defected to the Soviet Union. [laughter] I had just accepted this job at MIT with the expectation that I would use my previous employment by the Atomic Energy Commission and my security clearance to obtain a personal knowledge of classified technology, which I would then mentally declassify to the extent of writing some notes, which I would then submit for clearance before teaching my class. But at any rate, MIT hired me because I had this contact with the Atomic Energy Commission as prior employment there, and access to classified information, even though I wasn't allowed to use it in teaching. The Wheeler defection occurred just after I had reported to work at MIT. I was living in mortal terror that the very diligent investigators would discover that the Wheelers for whom I had worked in the state of Washington were the parents of the George Wheeler who was then in the newspapers as a government official who had defected to the Soviet Union. [laughter] My wife knew what was bothering me, and she did something that would never have occurred to me, and which was very kind of her——she served me a good, stiff drink. [laughter] But the government never found out that my farm employment in the state of Washington had been as the result of a friendship with a guy who had given up the U.S. diplomatic service in favor of a lifelong exile to the Soviet Union. The coincidences that occur in a person's life!

BOHNING: Yes.

BENEDICT: You know, if you live as long as I have, all sorts of these strange things happen.

BOHNING: Did you ever have any contact with him after he defected, in later years?

BENEDICT: I did make a business trip to Czechoslovakia once. I was in Prague and I looked in the telephone directory for George Wheeler, but I can't remember whether I found his name or not. At any rate, I never had any further direct contact with him. I lost track of him altogether. He was a brilliant man; it's just a shame that he couldn't use his talents in this country.

BOHNING: How did you decide on MIT in 1931?

BENEDICT: Well, I was working in Washington state as I told you, and had the opportunity to get my wits back together again. The realization that I should study chemistry rather than economics led me to realize that I needed to go to a school where I could get a better education in chemistry——physical chemistry——than I'd had at Cornell. My brother, Bill [William S.] Benedict, had gone directly from Cornell to MIT and was studying physical chemistry there, so it seemed a natural thing to do to go there also. That was, of course, one of the best involuntary decisions that I ever made, because it was my MIT education in chemical thermodynamics that enabled me to do the professional work that gave me a professional reputation.

BOHNING: Can you tell me something about that MIT department? You were in chemistry, not chemical engineering?

BENEDICT: That's right.

Well, the chemistry department for the first year I was there was under the directorship of Frederick G. Keyes. Let me read some more. "The first year I was there, I registered for four courses, three of which were absolutely splendid. The fourth, The Kinetic Theory of Gases, was taught by Frederick G. Keyes, the head of the department, whose administrative duties didn't give him enough time to present class material properly. I took advanced calculus, taught by Jesse Douglas, a shy, brilliant mathematician who made infinite series and multiple integrals crystal clear. I wrote of his class, 'I'm just beginning to discover what mathematics really is, and I'm learning with regard to mathematics what I suspected of most of everything I had been taught at Cornell, that I had been taught only what someone thought I would need in the usual pursuit of chemistry, instead of teaching anything for its own sake.'"

Which was perfectly true. "John [Clarke] Slater, head of the physics department, gave lectures in Introduction to Theoretical Physics that were perfectly organized and introduced me to the beauties of Lagrange's equations in mechanics and Maxwell's equations in electricity and magnetism." That first year at MIT really was a turning point in my own professional education. "Best of all were Louis [J.] Gillespie's inspiring lectures on thermodynamics and chemistry." Have you ever heard of Gillespie?

BOHNING: No, I'm afraid I haven't.

BENEDICT: He was an unusually brilliant and versatile man. "The text was J. Willard Gibbs's "Equilibrium of Heterogeneous Substances" (8). Do you know that?

BOHNING: Yes.

BENEDICT: "Gillespie led us through that classic paper page by page. It was wonderful. It was my first experience with reading a text where every word counted, and where you knew if you studied long enough, that you would fully grasp the author's meaning. That class, more than anything else, provided the tools I needed for the successful research I did on my first important industrial assignment six years later." (Which was the development of the Benedict-Webb-Rubin equation of state.) "In the spring term I took the second part of Slater's Theoretical Physics lectures, and L. H. Rice's class in Modern Algebra. The latter's treatment of line integrals, determinants, and matrices were also useful in my subsequent research. I started research on my master's thesis, which was a small part of an extensive, multi-year, many-student research program directed by Professor James A. Beattie, on precise methods for measuring Centigrade temperatures on the international temperature scale, and relating these measurements to the absolute temperatures required for strict application of the second law of thermodynamics (9). My research was on an experimental realization of the fixed points of the Centigrade scale, which in 1930 were the ice point (the equilibrium temperature between ice and water at a pressure of one atmosphere), and the steam point (the equilibrium temperature between water and its vapor pressure at normal atmosphere). My research consisted of building equipment and establishing the fact that they measured these two temperatures within the required precision." There's lots more of this, but I don't think you want all this detail, do you?

BOHNING: Yes, I'm interested in the people you were associated with at MIT, and the kind of work that you did.

BENEDICT: Well, I'll read you some more. "By using the resistance of a platinum resistance thermometer for reference, I found that it was difficult to reproduce the temperature of the ice point as customarily measured by better than a thousandth of a degree because of the above factors." That was due to the variation in the barometric pressure, and the difficulty of saturating the water with air before conducting the measurements. "By using a specially designed shielded and cleaned vessel containing only pure water, pure ice, and water vapor free of all other gases, I found it possible to reproduce the triple point of water within three ten-thousandths of a degree Centigrade. The temperature of the triple point relative to the ice point, determined by measuring the change in resistance of a platinum resistance thermometer, I found to be .010, plus or minus .00005° Centigrade. I compared this with the temperature difference between the triple point of pure water and the equilibrium temperature of ice in contact with air at atmospheric pressure, which I calculated by Gibbsian thermodynamics, and found it to be .00990, plus or minus .00005° Centigrade."

BOHNING: What sort of a person was Beattie?

BENEDICT: That's a good question. I'm not sure that I've actually written anything about him, but I should have. He was a rather shy man and it was hard to get to know him well. He was a brilliant thermodynamicist and a very excellent experimentalist too, which was a splendid combination for a chemist. He was a rather indifferent lecturer; he was a rather shy person, and like myself he didn't speak in a very loud voice, and his lectures were kind of hard to assimilate for that reason. Gillespie was a much better lecturer than Beattie was. But doing experimental research under Beattie was a splendid education because he was a master at experimental technology and he taught me how to do careful research and be accurate in recording the results. It was really a great education. He was also helpful to me in getting me the National Research Fellowship that I took at Harvard after I graduated from MIT, where I had the unexpected good fortune of doing it under Percy Bridgman, whom you doubtless are familiar with, who was both a master of experimental research and a really splendid philosopher of science. He wrote two books. One of them was on the philosophy of physics, and the other was on experimental physics (10). At any rate, he was a fine person to work under because Beattie had been a very careful experimentalist using the most expensive kind of equipment you could imagine——platinum resistance thermometers and gas thermometers and things of that kind. Bridgman, on the other hand, was what you might call a quick and dirty experimentalist. He was most ingenious at developing his high-pressure equipment, carried experiments to much higher pressures than anybody before him had, in the simplest kind of equipment you could imagine, and built on almost a shoestring. It was really a splendid combination to work for Beattie, a precise experimentalist with elaborate equipment, and Bridgman, a highly original experimentalist with primitive but quite appropriate equipment.

BOHNING: So your Ph.D. thesis was with Beattie?

BENEDICT: It was with Beattie on the comparison of the platinum resistance thermometer with the gas thermometer (11).

BOHNING: I think that shows up in a number of these early publications.

BENEDICT: That's correct, yes. Well, there's a long account in here of my research with Beattie that I won't bore you with, but I do think you'd find it helpful to have copies of some of this.

BOHNING: Oh, yes. At this point were you pretty well set in your own mind now as to where you were going with your life?

BENEDICT: Oh, yes, I was going to be a chemist. And while I was at MIT, I had the good fortune to meet the girl who has been my wife now for fifty-six years. Marjorie Oliver Allen was a graduate student in physical chemistry who came to MIT a year after I had come there and while I was doing research on the gas thermometer in room 6231 on the second floor of the Eastman Research Laboratory at MIT. The chemistry department at that point assigned the students to do research with different professors. The students had no choice in it at all. Marjorie was assigned to do research with George Scatchard on the freezing point of aqueous solutions. Her laboratory was in room 6232, across the hall from mine at MIT. Her research required her to shave a large number of pounds of ice everyday in a hand-cranked ice shaver. I didn't think that was a job for a girl to do, so I shaved the ice for her. [laughter] That led to our becoming good friends, and subsequently, after I got my Ph.D. degree and before she got her Ph.D. degree, we were married and we lived happily ever after. Did you meet her briefly this morning?

BOHNING: Yes.

BENEDICT: A very happy marriage.

BOHNING: That's wonderful.

BENEDICT: The fact, of course, that she knew chemistry made her a much more understanding companion, when during the war I had to work fifty-seven hours a week, and the only time I saw her was when I came home after working at night until nine-thirty or ten o'clock and having to go directly to bed. So, it's been very helpful that she is a chemist.

BOHNING: Did she get her Ph.D. subsequently?

BENEDICT: Oh, yes. She got her Ph.D. with George Scatchard, and worked for a year and three months under Edwin [Joseph] Cohn at the Physical Chemistry Laboratory of the Harvard Medical School, where she worked on the freezing points of aqueous solutions.

[END OF TAPE, SIDE 4]

BENEDICT: She had a paper published before our first child came along (12). [laughter]

BOHNING: I want to talk about your postdoctoral work, too, because that also had some effect on your going back to work at National Aniline.

BENEDICT: That's right.

BOHNING: Is that when your first child was born?

BENEDICT: That's correct, yes.

BOHNING: What I wanted to look at next, was what you did at Harvard. Did Beattie arrange this?

BENEDICT: No, he didn't. It was Keyes who urged me to apply for the National Research Fellowship. I can't remember whether my wife had already received her offer of appointment at the Harvard Medical School or not, but at any rate, I wanted to continue my postdoctoral research in Cambridge so that we weren't obliged to move. Whether it was just because we had a lease that hadn't expired and I'd have to pay a penalty, my wife's postdoctoral appointment at Harvard Medical school, or whether it was some other reason, I can't remember. Whatever it was, I thought that the experience I'd had at MIT would prove useful at Harvard. I had an interview with Percy Bridgman, who had developed equipment for studying the properties of gases at high pressures but was not able to make very accurate measurements because his pressure measuring devices were quite primitive and weren't accurately calibrated. So I borrowed one of the MIT free piston gauges, and used it to obtain a more accurate calibration of Bridgman's own equipment, and then used that for measurements of the PVT properties of nitrogen and argon. I measured the pressure volume temperature properties of nitrogen and argon at temperatures below 25° Centigrade and above their freezing point temperatures at pressures up to 6000 atmospheres. We published papers on my work on nitrogen (13), but I never got the work on argon written up for publication. There are a couple of papers on that list that were published on the PVT properties of nitrogen.

BOHNING: Yes.

BENEDICT: May I see that list (14)? Yes. This first set of measurements were done at pressures that were too low to be measured accurately by Bridgman's rather crude method of measuring pressure, which was a resistance gauge, a magnet and wire. So I borrowed from MIT one of their free piston pressure guages, had that moved up to Harvard, and used that for pressure measurements.

I developed a rather ingenious procedure for doing the density measurements. I had to make it out of nickel because steel became brittle at these low temperatures. So I made a small pressure vessel not more than a half inch in diameter and three inches tall attached to a nickel capillary tube, which I silver-soldered together and then immersed in a thermostat at the appropriate temperature and pressed nitrogen (I also did measurements with argon) into it at pressures that were measured on my free piston pressure gauge. I then sealed off the contents of this small nickel pressure vessel by clamping shut the small nickel capillary tube that had connected it to the pressure gauge, cutting off the capillary and silver soldering the end of the capillary shut, weighing the assembled nickel and nitrogen or argon, after it warmed up to room temperature, cutting the capillary, releasing the gas, and then taking the cut end and the rest of the vessel and weighing it again, getting by difference then the mass of the nitrogen that had been contained in it. It was a laborious, tedious way of proceeding, because I could only make one measurement a day, but in that way I was able to map the PVT properties of nitrogen and argon at lower temperatures than the rest of Bridgman's equipment would have enabled me to measure it. I could get down to the triple point temperatures of -185° Centigrade. And those were then published in this paper number three on this list (13).

Then I extended the measurements to higher temperatures. Using the data I had obtained from these measurements by a sort of gravimetric method as a fixed point, I was able to measure the change in density from that particular condition of temperature and pressure to other pressures and temperatures between -175° Centigrade and +200° Centigrade by measuring the motion of a piston in a piston compressor. I was able to obtain the relative specific volume or relative density to the fixed point that I had already measured at these lower temperatures. That enabled me then to publish the densities of nitrogen between -175° and +200° Centigrade, and pressures between 1000 and 6000 atmospheres. That got published in 1937 (13).

BOHNING: Were there any safety problems in working at these very high pressures?

BENEDICT: Oh, yes. We had to be very careful. We had what I guess you could call an explosion barrier if you want to [laughter], a steel sheet between the equipment and where we were taking the measurements, and conducted everything by control from outside of the steel sheet. But I never had anything blow up on me. The quantity of gases used was rather small. It's just a gram or two so that it wouldn't have been a very serious explosion, at best. But it was a really splendid experience because Bridgman was "a master of experimental research" on how to get pertinent but not necessarily the most accurate results with a minimum amount of specialized equipment. It was really a very good experience for me.

BOHNING: How many other people were working with Bridgman at this time? Was that high pressure laboratory a big group?

BENEDICT: It wasn't a big group but there were several other people there who went on and had eminent careers. One of the persons who I had worked with and who was by then a postdoctoral research associate at Harvard was a fellow named [Albert] Francis Birch, who later became a professor of geophysics at Harvard and used many of Bridgman's techniques to measure the properties of geothermal solutions at high temperatures and pressures. Chauncey Starr, who later became a vice president of the Atomic Energy Research Department at North American Aviation, became a prominent figure, and I saw him many years later out in southern California, when he was with UCLA. These were people that had prominent careers in chemistry after that experimental work with Bridgman.

Well, after I had done this work on nitrogen and argon with Bridgman and had married, and my wife had a professional appointment at the Harvard Medical School, I didn't want to leave the Cambridge vicinity because my wife would have had to give up her job at the Harvard Medical School, so I was offered a postdoctoral appointment as a research associate in geophysics by Professor Louis C. Graton of the Harvard geology department, who was starting research on physical-chemical properties of solutions important in mineral deposition at the conditions deep in the earth--at high temperature and high pressure. He had one man, formerly a student of Bridgman, who was working for him named Francis Birch. He's a very eminent American geophysicist.

Birch was sort of in charge of a laboratory which was being operated for the Harvard Committee on Geophysical Research, I guess it was called. I better look it up. Here it is. "When my National Research Fellowship with Bridgman ended on August 1, 1936," my God, that was fifty-five years ago [laughter], "I moved to the Gordon McKay Laboratory to start research for Harvard's Committee on Geophysical Research on solubility relations of aqueous solutions at high temperatures and pressures. This committee was interested in the conditions under which minerals might be deposited from aqueous solutions at high temperatures and pressures, and wanted experimental methods on the phase equilibrium under these rather difficult conditions." Well, let me read a little bit of this. "Theory predicts the existence of two types of aqueous solutions. In one type, such as the solution of quartz and water, the solubility of quartz is so low and the vapor pressure of water is so high that as the temperature is raised, critical phenomena occur in which liquid and vapor phases become identical in properties. In another type, such as potassium chloride and water, the solubility is so high and the vapor pressure of the solution so low that critical phenomena do not occur. When a mixture of potassium chloride and water is heated in a closed vessel under appropriate conditions, three phases--solid potassium chloride, saturated liquid solution, and vapor--can coexist at all temperatures between the eutectic, which is below 0° Centigrade and the melting point of potassium chloride, 770° Centigrade. The system of potassium chloride and water was the one I selected to prove out the experimental equipment and the experimental method (15). Later, the equipment was used by Norman [B.] Keevil and others to measure systems of greater geological significance," because nobody cares very much about aqueous solutions of potassium chloride. I'd have been better advised if I'd have used aqueous solutions of sodium chloride because this would be of geologic significance. But the reason I chose potassium chloride was it had a higher solubility in water, and I figured that its triple point temperature at which vapor, liquid and aqueous solutions all occurred simultaneously would be more nearly within my experimental reach before the equipment became too weak because of its impaired elastic properties at high temperatures. At any rate, I did make measurements on potassium chloride and water. I'll show you the triple point curve that we had developed, at least I think I will. This wasn't very smart; I should have written the table of contents first.

BOHNING: Is that paper number six (14)?

BENEDICT: Yes. Here it is. "We obtained the measurements at these points here, and I was able to chase the temperature up to 600° Centigrade before the steel became too weak to go further. But I did show that the pressure at which salt, aqueous solution, and vapor were in equilibrium, went up through a maximum and started down again because of the fact that the solubility of potassium chloride and water was increasing at such a high rate that the vapor pressure of the solution stayed below the critical pressure of water and one could predict that this curve would continue to the triple point of potassium chloride in the absence of water altogether." It was kind of pioneering research at that time. Later, another fellow came along, Norman Keevil, and worked with many other solutions that were more significant in geologic interest. But it was good research, and I was able to use experience I'd had both with Beattie and with Bridgman to conduct the research successfully.

BOHNING: On some of the other things you've done you also show a combination of both developing out a process and then designing the experimental work and collecting data to verify it.

BENEDICT: Yes, but I ceased to be an experimentalist, though, long ago. [laughter]

BOHNING: I was going to ask you how you felt, because you're designing and building equipment before you collect the data. Did the actual building of and constructing the equipment give you the most satisfaction?

BENEDICT: No. I am ashamed to confess that I think I'm really more at home with using my mind than using my hands, and much of the successful experimental work I did was really highly dependent on very able machinists or professors who knew experimental techniques, rather than my own ability as an experimentalist. I had a very good machinist at MIT named Charles Gallagher, who was a very irascible Irishman, but was awfully good at designing and making equipment. At Harvard I had the assistance of Bridgman's two people. They were more than machinists, they were good people who knew how to do experiments even though they were used normally mainly to make the equipment. A fellow who was most helpful to me was named Charles Ames, who made the high pressure equipment I used to conduct these experiments on potassium chloride. For the work on nitrogen, I made my own equipment because I used Bridgman's pressure guages and I made my own small, weight piezometer.

BOHNING: I'm interested in your leaving Harvard and going back to National Aniline.

BENEDICT: Well, I guess I'd better read this to you. "Because of the addition to our family and Marjorie's enforced unemployment..." She had been working previously at the Harvard Medical School and together we had a quite comfortable joint income of some $4500 a year, but at any rate we needed more income than my own $200 a month salary as a research associate in geophysics at Harvard. "Well, the first opportunity I had for work at higher pay after my one-year employment at Harvard ended on September 1 was an offer from Professor Keyes at MIT of a postdoctoral fellowship to work on low-temperature calorimetry. In retrospect, I realize that I made the right decision when I declined the offer, concluding that I had needed more diversified experience than just continuing as a research physical chemist at a university. I wrote Professor [Jacob] Papish at Cornell about a faculty position at Cornell, and Dr. Leo I. Dana, supervisor of a former associate of mine at MIT, at Linde Air Products Company about an industrial position at their research laboratories near Buffalo." I'm going to read you some more of this.

BOHNING: Sure.

BENEDICT: "Chauncey Starr recommended me for a position in the Brown Paper Company's research laboratory in Berlin, New Hampshire, where he was temporarily employed. In a hurried, one-day trip I drove to Bridgman's summer home in the White Mountains, left with him a manuscript of my papers on nitrogen, and had a successful interview with Dr. [Milton O.] Shur of Brown Paper Company, who offered me a position doing fundamental research on the hydration of cellulose at a salary of $250 a month. [laughter] But I had also made other applications. I made a three-day trip to Ithaca and Buffalo. In Ithaca I had no success in an application for a teaching position at Cornell. In Buffalo, I drew a blank with Linde Air Products Company, to whom I'd also applied for work, but was welcomed royally by Frederick [H.] Kranz, for whom I had formerly worked at the National Aniline and Chemical Company. Kranz insisted that I see Dr. [Wesley] Minnis, then director of research for National Aniline. He offered me a position in National's research laboratory at $275 a month, which was $75 more than I was making at Harvard, to do fundamental research on the kinetics of oxidation of benzene to maleic anhydride in an attempt to improve the yield of that National research product. On my return home, my wife and I weighed the pros and cons of my two job offers. To work in Berlin near the White Mountains would be much pleasanter than in grubby Buffalo [laughter], but Berlin was professionally isolated, cellulose chemistry wasn't exactly my dish, and the higher National salary was a factor. So we decided to shuffle off to Buffalo." [laughter]

[END OF TAPE, SIDE 5]

BENEDICT: I'm glad I wrote this up because I couldn't make it nearly so detailed or colorful in just a casual memory. "I gave Harvard notice on August 1." This was 1937. "I was disappointed that after three years of splendid advanced education at MIT and two and a half years of productive research at Harvard, the most promising job I could get was with the company that I might have been working for all along if I hadn't have gone to graduate school." [laughter] "In mid-August we closed our apartment in Ware Hall, put our furniture in storage and took a twenty-five day vacation. We drove with our daughter Mary to Cayuga, where I showed Mary to her admiring maternal grandparents, left our car there and with Mary traveled to Lake Linden by train, where she was met by her paternal, Benedict, grandparents." After our vacation in Michigan I rented a room in Cambridge. I did spend six weeks at Harvard completing the successful runs on potassium chloride solutions that I showed you the reprint of. "Professor Graton, for whom I worked at Harvard, was sorry to have me leave, but understood the financial circumstances that led me to change jobs. Happily, the work I started on, high-temperature aqueous solutions, was carried much farther later by a fellow named Norman Keevil. Finally at the end of September, six productive years in Cambridge came to an end. I said goodbye to my friends, took the train to Cayuga, picked up Marjorie and Mary, my daughter, and drove off to the next chapter in our lives in Buffalo.

Marjorie had rented us a new home at 150 Anderson Place for $67.50 a month." [laughter] That was a lot of money in those days. "On the second floor of a house just converted into a two-story duplex we had the luxury of living room, dining room, kitchen, two bedrooms and enclosed front porch. As we hoped to have a live-in maid, Marjorie had obtained furniture for a second bedroom from her family and had purchased a crib for Mary. The rest of our furniture, which had been temporarily in storage, was scheduled to arrive shortly after us.

Our entry into our new home was not auspicious. Our uncooperative landlord, Mr. Wolkin, hadn't turned on the heat and most of the apartment was bare. Mary must have sensed the desolation, for she erupted in uncharacteristic wails." [laughter] "When we got Mr. Wolkin to fire up the furnace, we found that he used the cheapest soft coal. When our furniture arrived on the same day we did, Marjorie made our new curtains and we were soon settled in the most spacious home we had ever enjoyed. With our new affluence we decided we could afford a live-in, part-time maid who would help Marjorie get dinner and babysit Mary in the evening. We had the good fortune to find Mrs. Haley, a kindly widow who worked for a dry cleaner days and was glad to make her home with us. I wrote, 'Mrs. Haley is a jewel, a pearl of great price and worthy of better things than working for us for a dollar a week and board.' I'm ashamed now to think of that dollar per week." [laughter] Do you want to know about my research at National?

BOHNING: Yes.

BENEDICT: "At National my research laboratory was in a new building on the east side of Abbott Road used for control and research on new catalytic processes National was introducing. I worked for Mr. C. [Carolus] S. Woodwell, who was in charge of the catalytic department. This department was already in large-scale production of phthalic anhydride made by oxidizing naphthalene vapor with air over a vanadium pentoxide catalyst. National was just starting production of maleic anhydride made by oxidizing benzene, also over a vanadium pentoxide catalyst. Whereas the yield of phthalic anhydride was high, that of maleic anhydride was much lower, with apparently unavoidable production of several by-products. My assignment was to study the kinetics of the oxidation of benzene by measuring the rate at which the several oxidation reactions took place over different catalysts, in the hope that a catalyst could be found which would selectively produce maleic anhydride.

I had the help of a good analytical laboratory, was assigned a capable chemist assistant, Don Gleeves, and could depend on the nightwatchman, Mr. Collope, to maintain operating conditions in runs that lasted over the night. The experimental technique I used was to pass a mixture of benzene vapor and air at a set rate and temperature in a series of runs over catalyst beds of progressively greater length, while determining the yield of unreacted benzene, intermediate products hydroquinone and benzoquinone, by-product fumaric acid, the desired product maleic anhydride, and carbon dioxide. I developed and solved the first order differential equations expressing the rate at which the different reactions took place as a function of residence time over the catalyst and used the rate constants to characterize the various catalysts tested and find the residence time and temperature resulting in the highest yield of maleic anhydride. It was straightforward, but rather unimaginative, chemical kinetics research.

In early December I returned to Cambridge to give a report on my past year's work on potassium chloride solutions to Harvard's Committee on Geophysical Research. My lecture with numerous slides was well received by professors Graton, Bridgman, [Harlow] Shapley, [Reginald A.] Daly, [George B.] Kistiakowsky and Birch, friends Blaney [Ernest B.] Dane and Dennison Bancroft, and Norman Keevil and Dave [David R.] Briggs, who were continuing my type of research with other substances. Professor Graton arranged for publication of my paper on this work in the Journal of Geology (15)."

BOHNING: But you were at National Aniline not even a year before you moved on to Kellogg.

BENEDICT: That is correct. And the reasons for that are stated later here. "By January, we felt happily and permanently settled in Buffalo. But on January 20 I had a totally unexpected letter from Howard Dimmig, director of research for the M. W. Kellogg Company, asking if I would be interested in a position undertaking research on liquid vapor equilibria of hydrocarbon mixtures. Professors Beattie and Keyes had apparently recommended me highly for the position. After I asked Dimmig for more information, he wrote that the position would involve planning research to be done under fellowships by graduate students at MIT and Caltech, directing additional research in Kellogg's petroleum research laboratory in Jersey City, and correlating all the data to provide Kellogg and associated oil companies with means for predicting the behavior of hydrocarbon mixtures and high-pressure distillation columns. As I could see that the thermodynamics I had learned at MIT and the high-pressure techniques I had used at Harvard would be directly relevant, I was interested and went to New York and Jersey City for interviews on February 5. There I learned from Dimmig that Kellogg had a well-equipped research laboratory and library in Jersey City, and that the research would be adequately funded by Polymerization Process Corporation, abbreviated POLYCO, a consortium of Kellogg, the Standard Oil Company of Indiana, the Phillips Petroleum Company, and the Texas Company. Those companies had found that at the higher pressures then being used to produce gasoline by catalytic polymerization, the simple ideal-solution relations accurate enough for designing low-pressure distillation columns broke down completely. A salary of $300 per month was more than the $275 National Research was paying me. Marjorie went with me to New York to look for housing. Friends suggested we consider living in Radburn in Bergen County, New Jersey, because of convenient rail commuter service to Jersey City." Are you familiar with Radburn at all?

BOHNING: No. I know the area, but I don't know that town.

BENEDICT: It was one of the first model communities; it was a planned community. Well, I wrote that here. "When he drove us there, we found that Radburn was a pleasant, rural, planned model community, one of the first in this country. We liked the country setting, the community center, the stores, the swimming pool, and the tennis courts, and made a $25 down payment on one month's rent and $55 for the vertical, two-story and basement half of a two-family attached brick house at 23 Randolph Terrace and decided to take the Kellogg job to start work on March 21, 1938."

BOHNING: And this is where the Benedict-Webb-Rubin equation came in.

BENEDICT: That is correct, yes. Do you want any of the living accomodations there?

BOHNING: Sure.

BENEDICT: "We persuaded Mr. Wolkin, our landlord in Buffalo, to cancel our lease with a $100 penalty, and our maid Mrs. Haley went to live and work for my National Aniline employer Frederick Kranz. On March 24, the movers took our effects from 150 Anderson Place, and we drove off with Mary beside us and her baby carriage in the rumble seat. We spent that night in Cayuga with Marjorie's parents and drove to New Jersey on Saturday through Ithaca and Binghamton. It was a cold, dark day with occasional hailstorms and we were delayed by several detours so that we were three hours late arriving for our appointment with the movers. Fortunately, an agent had let them into our house and they were busily unloading our furniture. The electricity and gas had been turned on and the ton of coal my brother had ordered for us was in the basement coal bin. After grubby Buffalo, living in Radburn seemed idyllic. Our house was at the east end of the built-up section of the town and looked out on tennis courts and swimming pool and beyond them fields and woods. The community center and essential stores were within walking distance, as was the station on the Bergen County branch of the Erie Railroad, which ran commuter trains to Jersey City, where my employment was. Although the soil of our yard was poor, it supported grass and shrubs, and a few spring bulbs left by previous renters soon gave us flowers. We could see the top of the Empire State Building fifteen miles away from our front porch. I found commuting by rail, which I have since done for over fifty years, very pleasant and productive." Incidentally, when I go back to Massachusetts in the summer, I still commute daily into MIT, where MIT is still providing me with an office and a secretary for writing more of this personal history.

BOHNING: Where do you commute from when you go into MIT?

BENEDICT: Wayland, Massachusetts. I use the railroad station at Wellesley Farms. Are you familiar with that geography?

BOHNING: I lived in Cohasset on the south shore for several years.

BENEDICT: Oh, Cohasset's on the south shore. Did you commute to the South Station?

BOHNING: No, I drove in on the Southeast Expressway.

BENEDICT: You made a bad choice. [laughter] Commuting by rail is much more satisfactory, I've found. "I found commuting by rail, which I have done for over fifty years, and still do in the summer, very pleasant and productive. The morning ride gave me time to plan the day's activities and the evening ride time to sort out what I had learned during the day. The short walks from home to the Radburn Station, and from the Westside Station to Jersey City and the Kellogg Plant at the foot of Danforth Avenue were good exercise. The only drawback was my reluctance to get up in time, so that I frequently had to finish my toast on the way to the station, to the amusement of my Radburn neighbors." [laughter] That's still a problem. Now I have to drive to the station from home, and I have to eat the toast with one hand while I steer the car with the other. [laughter]

"I started work for the petroleum research laboratory of the M. W. Kellogg Company on Thursday, 31 January." This must have been 1938. "The company had been formed by Morris W. Kellogg about thirty years earlier to manufacture steam boilers. It had branched out into heat exchangers and distillation towers, sold equipment to oil and chemical companies all over the world, and designed and built oil refineries and synthetic ammonia plants. The company's main office was in the Transportation Building at 225 Broadway in Manhattan. The Jersey City plant occupied about twenty acres on navigable Newark Bay. It consisted of several large, brick buildings with heavy-duty cranes for fabricating large process equipment. There was the pilot plant laboratory for demonstrating Kellogg-developed processes, an excellent welding and machine shop, a good analytical laboratory, and an office building containing an unexpectedly complete library of chemical books and periodicals." It was really a splendid company to work for. It was small enough so that you got to know the supervisors all the way up the line, including the officials of the company, and yet it was productive and advanced enough to have a good laboratory and a good research library. So, I couldn't have done better. "The Jersey City laboratory staff was young, dynamic and capable." I'll give you the names of some of my supervisors. "The librarian, Hungarian Mr. Velossi, was invaluable in getting and translating obscure periodicals for me." To do this equation of state work I needed papers from all over the world——Russia, of course he couldn't translate Japanese, but Italian and French and German, and he was very helpful. "There was an excellent pool of male stenographers supervised by Joe Calabrese, and Harry [S.] Blumberg directed metallurgical research. One of my first assignments was to visit the Kellogg offices in New York to learn what kind of physical-chemical data Kellogg and its oil company associates needed for process design.

My mentors there were Walter [E.] Lobo and his deputy, Leo Friend." Did you ever hear of either of those people?

BOHNING: No, I haven't.

BENEDICT: Walter Lobo was a brilliant chemical engineer——the perfect man to put in charge of the company's chemical data, collecting, organization. Well, I'll read it. "Walter was a brilliant, curly-haired chemical engineer and graduate of MIT who was in charge of furnace design under the M. W. Kellogg data books. Leo Friend produced the numerous charts and tables that made the data book one of the most valuable Kellogg assets." Have you ever seen any of these corporate data books?

BOHNING: No.

BENEDICT: They are really fantastically valuable. The companies glean the literature and then they condense from it the information that's the most valuable to the company and put it in the form that the company can most readily use.

BOHNING: I see.

BENEDICT: "From Friend and Lobo, I learned that what Kellogg and its associates needed for distillation equipment design were K-values. A K-value, I learned, was the ratio of the mole fraction of a compound in the vapor phase to the mole fraction of the same compound in the liquid phase," with which the vapor is in equilibrium. If you have a K-value and you know the composition of one phase, you can calculate the composition of the other phase just as the appropriate ratio or product. "The reason such a ratio was used was that at a given temperature and pressure, this ratio had been a constant independent of phase composition. The problem Kellogg and its associates were having was that at the higher pressures then starting to be used in oil refineries," these were above about 40 atmosphere, "K-values were no longer constant. My job was to find a method for calculating how K-values depended on pressure, temperature and mixture composition from the known properties of the pure hydrocarbons making up the mixture." The thought was that if you had enough information on the pure compounds, and perhaps a few experiments on the mixtures, you could forecast what the rest of the properties were without having to measure all the properties of all the mixtures at all compositions which they could be at.

"Back in Jersey City I was given the invaluable assistance of George Barlow Webb, who had graduated from MIT in chemical engineering; he was well versed in thermodynamics and distillation. We reviewed the literature on thermodynamic properties of light hydrocarbons from methane through butane that were the principal components of the mixtures for which Kellogg and its POLYCO research partners were designing distillation equipment. We tabulated the amount of data then available on liquid vapor equilibrium mixtures of these hydrocarbons. The two U.S. laboratories then measuring liquid vapor equilibrium were the Whiting Research Laboratory of Standard Oil Company of Indiana, and [W. N.] Lacey and [Bruce H.] Sage of the chemical engineering department at Caltech. It was decided that I should visit the Whiting Laboratory of the Indiana company, Dr. Carl Hackmuth of Phillips, one of the Kellogg partners in this research, Dr. Donald Katz of the University of Michigan and Professors Sage and Lacey at Caltech to learn what experimental research on liquid vapor equilibrium was in progress and what methods of correlating such data might be suitable." So, I took a long trip to Indiana, to Phillips Petroleum in Bartlesville, Oklahoma, and to Caltech.

BOHNING: Did you see Katz at that time?

BENEDICT: Yes, I saw Donald Katz at the University of Michigan.

BOHNING: Three years ago I did an interview like this with him (16).

BENEDICT: Is he still alive and kicking?

BOHNING: No, he passed away a year ago last year.

BENEDICT: Oh, dear, I'm sorry to hear that.

"I had a good deal of help from a fellow named Webster B. Kay, who was working for the Standard Oil Company of Indiana, a man who did subsequent published research on liquid vapor equilibrium; and a chap named Wayne C. Edmister." He has written a book on chemical thermodynamics (17). "Wayne and his lovely Russian-born wife Margaret became lifelong friends, and Webb, who later took his experimental research to Ohio State University, continued for many years to provide data on other hydrocarbon mixtures that I was able to include in later correlations," so it was a very valuable association I developed through that contact. I visited Donald Katz at Ann Arbor. "I went to Bartlesville, Oklahoma, where I visited Carl Hackmuth at the Phillips Petroleum Company research laboratory. Hackmuth educated me on the kind of data Phillips needed to design equipment for recovering propane and butane from high-pressure natural gas," so that I knew what pressures and temperatures were important there. "I went out to

Los Angeles and visited Sage and Lacey at Caltech, where I saw Sage's impressive laboratory set up for measuring the PVT properties in liquid vapor equilibrium of hydrocarbon mixtures and discussed with him mixtures he'd already worked on and the ones he planned to study, and selected the systems to be supported by POLYCO" (that was the company that was supporting our research at Kellogg). "We decided that POLYCO would support measurements on pure isobutane and mixtures of methane and isobutane." And, parenthetically, "Professor Sage and his wife were very kind to us, and drove us to the summit of Mount Wilson, where we looked through a sixty-inch telescope." [laughter] And, incidentally, let me show you my telescope. This has nothing whatever to do with our discussion. My wife gave me this.

BOHNING: Yes, it's a Questar.

BENEDICT: Are you familiar with Questar?

BOHNING: Somewhat, yes.

BENEDICT: It's really a three and a half inch, cadiotropic reflecting telescope.

BOHNING: That's excellent.

BENEDICT: I think I'm taking too much of your time and I'm not giving you lunch. What would you like to do?

BOHNING: If you want to take a break, that's fine.

BENEDICT: All right, why don't we do that.

[END OF TAPE, SIDE 6]

BOHNING: You have given me some information about Webb, but I don't think you said anything about [Louis C.] Rubin and who Rubin was.

BENEDICT: Rubin was my supervisor. He got into it very indirectly. I was hired to work for Kellogg by a vice president named Howard Dimmig, who at that time was the Kellogg representative on the Polymerization Process Corporation's committee of representatives of the companies that were funding this research, which were the Standard Oil Company of New Jersey, the Standard Oil Company of Indiana, the Phillips Petroleum Company, and the M. W. Kellogg Company. But he was transferred to the New York office and the directorship of the laboratory was changed to Louis C. Rubin, who was my laboratory supervisor. I don't want to sound mean here, but his involvement was strictly a matter of company policy because he had nothing whatever to do with any of the work. He didn't conceive of it in the first place. He wasn't the person who oversaw the research, but he was simply my boss at Kellogg. It was required that when I published a paper under the Kellogg flag, my supervisor's name would go on it also. So Louis C. Rubin became a coauthor of our first paper and of our subsequent papers.

George Webb was a young chemical engineering graduate from MIT who joined the Kellogg staff shortly after I did. Kellogg was awfully good about providing all the kinds of help I could possibly use. It was a marvelous company to work for. They recognized that once I had this idea of correlating the thermodynamic properties with what later became known as the Benedict-Webb-Rubin equation of state, that I would need help because there were a lot of other mixtures——the pure hydrocarbons, for that matter——to be tackled in the same way. So they assigned George Webb to work with me.

They couldn't have chosen a better man because George was taciturn and unflappable. He was Scotch and from New England. And he undertook the most tedious, apparently unrewarding kinds of research. I did my share of it, too, but he was my full partner in that, in looking up data and in interpolating to the even values of the density or pressure or whatever else we needed the data for, and then by old-fashioned hand-cranked calculators, using the method of least squares, finding the most suitable parameters in the Benedict-Webb-Rubin equation of state to fit all these data points.

It was terribly tedious and unrewarding work. We spent a couple of years at it. First we finished the first four normal hydrocarbons (methane, ethane, propane and butane), and we published on that (18). Then we extended it to twelve hydrocarbons altogether, including three olefins——ethylene, isobutene and propylene. The other five were isobutane, normal pentane, isopentane, normal hexane, and normal heptane (19). We tried to fit it to the aromatic hydrocarbons such as benzene and toluene. We struck out there because it doesn't work for them very well. So we had to limit it to the paraffinic hydrocarbons and the olefins. But most of the mixtures that Kellogg was dealing with in the distillation separation of light hydrocarbon mixtures didn't get up to six- or seven-atom compounds, so we didn't run into any cyclic compounds to speak of. It was very successful in dealing with paraffins and olefins.

After that proved so satisfactory, and it was evident to Kellogg that applied physical chemistry was helpful in solving many of their problems, I was given a whole series of other problems, which on the whole we solved quite successfully. The most interesting to me were the ones dealing with azeotropic and extractive distillation (20). There I had to arrange to correlate the properties of mixtures of hydrocarbons with volatility enhancing agents. For extractive distillation we used phenol, and for azeotropic distillation we used methanol.

We had it set up to measure liquid-vapor equilibrium in ternary mixtures such as methane, toluene, and normal heptane (21). There we found that the Benedict-Webb-Rubin equation of state wasn't adequate to deal with it because the non-ideality of the mixtures required that we not calculate the parameters in the equation just from those of the pure hydrocarbons. In the equation there were some multi-suffix parameters that were relevant to the mixture but not relevant to pure compounds. These had to be determined by additional experimental measurements on the mixtures themselves. But once those measurements were available, these additional parameters could be evaluated and the equation was still fairly useful for correlating the data. I guess the Kellogg Company has gone on and used it in its future work, although I've lost track of it, of course.

BOHNING: Wasn't this problem brought about because of the need for more toluene for TNT as the war effort was developing.

BENEDICT: That is correct, yes.

BOHNING: Did you not also work on a continuous flow calorimeter?

BENEDICT: I guess the Kellogg Company realized that I had capabilities that I didn't know I had myself, because I'd never done any continuous flow calorimetry before at all. It was quite important in their cracking and reforming process to know what the heat duty of the reactors were. They needed information on the heats of cracking and heats of reforming of a number of the hydrocarbons that they were processing. That was before the National Bureau of Standards had completed all the fine work that [Ferdinand G.] Brickwedde and some of his associates had done to provide much of these data, and they had to be determined experimentally.

Even though I'd had no prior experience in calorimetry, I was assigned the task of getting these data. Fred Keyes from MIT was a consultant to Kellogg, and he was familiar with work which one of his and my MIT chemistry professors, Sam [Samuel C.] Collins, had done on low-temperature calorimetry and recommended that a Collins-type, vacuum-insulated calorimeter be built for the experimental work that we would do in measuring the heats of cracking or reforming for hydrocarbons at temperatures up to 400° or 500° Centigrade. I was convinced right from the beginning that at temperatures this high the Keyes and Collins type calorimeter, which depended on vacuum insulation, wasn't going to work because of radiation heat transfer at these high temperatures. They had hired one of Keyes's research assistants who was a very able laboratory technician but not a highly educated physical chemist to build a Keyes type, vacuum-insulated calorimeter to try to make these measurements. And the heat losses from the calorimeter at the high temperatures at which they operate just swamped the heat effects that they were looking for, and it was obvious that it wasn't going to work.

So I persuaded the company to let me build a flow calorimeter that had some of the best thermal insulation one could have had, on a somewhat larger scale so that the heat losses wouldn't be that serious, a fraction of the heat of reaction that we were looking for. We built it and operated it quite successfully on thermal cracking and reforming processes. Later we extended it to build a calorimeter which contained small amounts of cracking or reforming catalysts for catalytic processes too. That work was done late in my career at Kellogg, just before they took on the wartime gaseous diffusion project, and I never had time even to negotiate with the company for permission to publish that. So that never got published.

BOHNING: Before Kellex was formed you also did some work on a diffusion process with hydrogen, separating hydrogen from gas mixtures (22).

BENEDICT: That's right. That's what's known as serendipity——accidental, happy results from unexpected chances. That was after the Kellogg Company had found that I was pretty good at unraveling the basis for various novel separation processes. The work I'd done on azeotropic and extractive distillation sort of solidified me with the company. They decided that they had a need for a process for extracting hydrogen from the products of cracking, because hydrogen was subsequently needed for hydroforming or some other process. I don't remember how they got onto the track there. I'll have to consult my autobiography here if it goes up that far. "Recovery of hydrogen from refinery gases by mass diffusion." Yes, here it is. I'll read you some of this, because it's all pertinent.

"In the summer of 1941 I was assigned to a new research problem which indirectly led to my being given six months later the most significant engineering development problem of my entire life. M. W. Kellogg company was developing processes for cracking or reforming hydrocarbons which needed an inexpensive source of impure hydrogen. As hydrogen was a minor constituent of many refinery gas streams, a process was desired that would extract hydrogen from them.

"In 1939, C. G. Maier of the U. S. Bureau of Mines had published an account of his research on a process he proposed for concentrating hydrogen in gas streams by taking advantage of its high diffusion coefficient into steam, from which it then could be separated by partial condensation (23). Maier called the process "atmolysis." Rubin and [Percival C.] Keith asked me to set up equipment and run it to determine how the purity and yield of hydrogen and consumption of steam depended on the conditions under which the separation could be conducted. I designed an apparatus in which one could make the appropriate measurements and developed a theory for the process and showed how the degree of separation of hydrogen from methane could be controlled by the amount of steam that was allowed to flow into the mixture while the hydrogen counter-diffused against the steam." It proved to be quite successful in correlating the process.

"But we found that the consumption of steam per unit of hydrogen extracted was so high as to make the process unattractive under the economic conditions then prevailing. I worked out enough of the theory of the process to show that this was a necessary consequence of the diffusion rates, and I proposed the name 'mass diffusion' for the process by analogy with thermal diffusion, which was by then already known as a way of separating gas mixtures. The combination of my assignment to this project, the prompt and definitive though unfavorable result we secured, and my choice of the name 'mass diffusion' was a fine example of serendipity——happy chance.

Six months later when Keith was asked by Eger Murphree, then chairman of the U.S. OSRD, to undertake exploratory research on the gaseous diffusion process to produce uranium-235, Keith concluded erroneously that my work on mass diffusion made me well qualified to develop gaseous diffusion." [laughter] "In fact, except for the word diffusion in common, there wasn't the slightest similarity between the two processes." I've had more damned good luck in my life, first meeting my wife at MIT and then working for Kellogg, Keith, and getting assigned to the gaseous diffusion process.

BOHNING: I think Kellex was formed in 1942, and the war had started in 1941. Kellex was a subsidiary that was set up specifically for the Manhattan Project.

BENEDICT: It started in early 1942, that's correct.

BOHNING: You held the title of "head of process development."

BENEDICT: Development, that is correct. There was also a process design group that was headed by a fellow named Charlie [Charles C.] King, a very capable chemical engineer from MIT.

BOHNING: I was going to ask you about Ralph Landau because I've interviewed him recently (24). He was in some respects involved at Kellex.

BENEDICT: Yes, but in a very minor way. He was brought in to assist in the production of what we subsequently called "special chemicals." These were substances which would not react with fluorine but which could be used as coolants or lubricants in the gaseous diffusion process, in case there was a leak of the very strongly fluorinating process gas, uranium hexafluoride, into the mixture. So his problem was to develop efficient processes for producing these hitherto nonexistent compounds, such as perfluoroheptane and perfluoroxylene.

Before Ralph came into the company, we in the process development section of the company recognized that it would be a serious loss of process gas and possibly an explosion hazard if ordinary hydrocarbon coolants were to leak into the process gas, uranium hexafluoride, which was almost as strongly fluorinating as fluorine gas itself. So, having been told that cost was no object, we recommended that the company obtain through the Manhattan Project compounds which had never even been made before, but which we assumed would exist, namely perfluoroheptane and perfluoroxylene, respectively, C_7F_{16} and C_8F_{16}. We predicted that these compounds would have boiling points sufficiently different from uranium hexafluoride, so that if they leaked into the process gas, first of all there would be no reaction (which was an important criterion), but secondly that they could be separated from it by partial condensation with the recycle of both the coolant and the process gas. Whereas if they had exactly the same boiling point as uranium hexafluoride, there might be no opportunity to get them out; they would go along and would concentrate, either with the product, which we definitely didn't want, or with the tails, which would plug up that part of the plant.

So, we looked up what was known about the physical properties of some of the lower fluorocarbons. C_2F_6 was probably known by then, and C_3F_8 may have been known. We found out what the relationship of their boiling point was to those of the corresponding hydrocarbons and we predicted that if C_7F_{16} was made, it would be the first of the normal fluorocarbons to have a molecular weight higher than that of uranium hexafluoride and would therefore concentrate in the tails with the U-238 hexafluoride. Whereas, if we used one of the lighter fluorocarbons, it would concentrate with the heads, the product, and would so dilute the gases being processed in that part of the plant that it would to all intents and purposes plug the separating units.

So without these compounds ever having been made before, we specified that for the successful operation of the plant we had to have two million pounds——that was the inventory of coolant in the plant——of C_7F_{16}. General [Leslie R.] Groves turned the problem over to the Du Pont Company. I remember going down to Wilmington at his request. It was more of a suggestion, it wasn't exactly a demand, but it was a communication that we badly needed an enormous amount of a compound that had never been made before. Dr. Harold W. Elley, who was the director of research for Du Pont, didn't bat an eye. He said, "Well, we'll see what we can do about it." By the time the plant went into operation they had the required amount of this compound that had never been made before. At first they made it by the reaction of fluorine with heptane, which was a very dangerous reaction. Subsequently they found new ways with some other less aggressive fluorinating agents. And later still, in fact it was a Du Pont suggestion, they found that another compound, C_8F_{16}, which could be made from xylene, would do just as well and would be easier to make.

BOHNING: As head of the process development group, what were you responsible for in terms of the K-25?

BENEDICT: One of my divisions was responsible for the process design of the plant. That was in charge of Charles C. King, a very able chemical engineer from MIT. I was also in charge of developing a appropriate theory for the gaseous diffusion cascade, which was handled by a very capable theoretical chemist named Elliott [W.] Montroll, who went on to a distinguished career as a theoretical chemical spectroscopist. Montroll and his associates, one of whom was Joe [Joseph] Lehner, developed the highly advanced and very appropriate mathematical theory of how a cascade of some four thousand stages would respond to time-dependent disturbances in its performance——whether a cascade would magnify the disturbance, leading ultimately to a disruptive or destructive surge in pressure, or whether the cascade would damp out these fluctuations as it went through successive stages.

The British at that time had proposed a construction of a plant which had operated at such low pressures that it worked in the laminar regime, where viscous flow took place and where their theorists could prove that the cascade would be stable because of the fact that the flow in the viscous flow is proportional to the square of the pressure difference. Whereas in turbulent flow, which we had in our plant, the flow is proportional to the first power of the pressure difference and they were certain that our cascade would be unstable because of the fact that disturbances would propagate up the plant instead of going down the plant.

So we had to develop a means for stabilizing the flow in the plant, which involved the use of pressure-control valves similar to the ones which are used in natural gas plants. The sensor determines the pressure in the plant and there's a valve whose control element is set to open if the pressure is too high to relieve the pressure, and to close if the pressure is too low to allow the pressure to build up. We specified that this kind of control be assigned to the plant. Our British colleagues, who were enamored of working at very low pressures at which laminar flow took place and at which the cascade would itself be stable, were convinced that this wouldn't work. The people at Kellogg were convinced from their use of pressure control valves and other less complicated equipment that it would work, but they needed proof. I was instructed to develop the theory for this and prove that our system would work. I wasn't a theorist and this called for solution of multivariable partial differential equations with time dependence and space dependence and a lot of other things, and it was no task for me at all.

[END OF TAPE, SIDE 7]

BENEDICT: At that time of the war, research mathematicians were being drafted for war service. They had very little to contribute to the production of munitions, but they were just the right people to solve these problems for our company. So a couple of very capable and very resourceful people were hired by the company to tackle these problems. One of them was a mathematician, Joseph Lehner, and the other one was a research physical chemist with a good knowledge of mathematical theory, Elliott Montroll. They solved the equations masterfully and proved that the control scheme that the company was proposing to use would work and would lead to a stable cascade, which subsequently proved to be the case. I had practically nothing to do with that except to tell these fellows what a gaseous diffusion plant consisted of and let them go to work.

BOHNING: But you were at Oak Ridge when it was finally being put up.

BENEDICT: Oh, yes, I was down there. First of all, when the plant was under construction I was assigned to describe to the operating company for the plant (Kellogg was the designer), which was a branch of the Union Carbide company called Carbide and Carbon Chemicals Corporation, what the rationale was for the design of the plant and what separation they could anticipate in the first stages that they built. It was things of that kind that we made based on very limited separation data that John [R.] Dunning and his associates had developed at Columbia University on pieces of barrier a few square inches in size. We developed correlations that predicted what the effect of pressure and temperature would be on the separation performance of the plant, specified the conditions of pressure and temperature under which the plant should operate, and gave Carbide an estimate of how many stages were needed, and how the plant would operate initially, both in steady state and during the start-up period.

When the plant was started up and a building of ten or twelve stages was in operation, I was assigned to go down there at first for limited visits and later for nearly full-time employment to monitor the start-up of the plant and the separation performance and to compare the separation performance with what our predictions were for that part of the plant. It was very exciting! It was a marvelous experience for a young chemical engineer to be assigned not only to the design of the plant, but also to be intimately involved with the start-up of the plant. It's an experience that very few chemical engineers get these days because if they're in the design office, they stay in the design office, and if they're in the operating division, they never see the details of the design and what the reasons were for making certain design decisions. But in my case I had the unusual advantage of making both the design decisions and then seeing where we were right and where we were wrong.

As I've written, it was terribly exciting to be down there when the plant was started up. The plant's equilibrium time was some ninety days, or something like that, so it would take a awfully long time for the plant to reach its full steady state isotopic distribution. But during the start-up stages, things happened more quickly than that. It was terribly exciting to see that as stages came on the line they were beginning to perform not just as well as we had predicted, but better than we had predicted. So, there was no doubt that even from operation of the first hundred or hundred and fifty stages of a four-thousand stage plant, that the final plant would do the job.

The plant was designed for one kilogram of U-235 per day, and it produced about three, because of the fact that the first data we had on diffusion barriers were inaccurate for a rather interesting reason. The first measurements of separation performance were made with mass spectrometers, from which measurements were taken with the isotopic U-235 content of the gas on one side of the barrier, and of the gas on the other side of the barrier. They were put through a mass spectrometer alternately. First one, say the high-pressure side where the U-238 concentrated, and then the low-pressure side where the U-235 concentrated. The measurements were made and separations of perhaps about two-thirds the theoretical separation were reported, which by the way was only four-tenths of one percent——a minuscule amount.

Well, it turned out that the mass spectrometer had a memory effect and that it retained some of the low-enriched material that went into it, in addition to the high-enriched material it was asked later to analyze, so that it reported a separation that was smaller than the real separation. We were very lucky that the plant at first designed for one kilogram of U-235 per day, separated better than we predicted. Not many designers have that advantage. [laughter] It ultimately produced something like three kilograms per day because the barriers were so much more efficient than the limited data which we had been given allowed us to predict. Another example of happy chance.

BOHNING: You must have known Al [Alfred O.] Nier then.

BENEDICT: Very well. Al Nier was not only an associate, but he was a very good friend and we kept in touch with him for many years later. Unfortunately, we've lost track of him lately. We used to correspond at Christmastime, but we don't anymore and I don't remember just why. He had a very distinguished subsequent career at the University of Minnesota, which was his home state.

I can't say it was luck, but it was probably good selection on the part of the people on the Manhattan Project that someone like Nier was assigned to take on the analytical problems of the gaseous diffusion plant and some of the other isotope separation plants, too, because Nier was an absolute genius for experimental research. He designed several types of mass spectrometers that were very important in the operation of the plant. The one that he was brought aboard for first, to analyze the uranium hexafluoride at different stages of enrichment, was an isotope separation one. But he used his brilliant chemical experimental instincts to develop mass spectrometers to determine the chemical composition of the gas——not its isotopic composition, but the amount of nitrogen. I guess it was mostly the amount of nitrogen that had leaked into the uranium hexafluoride because of the use of nitrogen-sealed gas compressors. That was to determine when and where in the plant the side stream would have to be taken off to extract the nitrogen, because nitrogen being lower molecular weight than uranium hexafluoride, would concentrate in the small upper stages of the plant and plug the operation of that part of the plant.

Nier developed what were called line recorders, which were spectrometers specifically designed to quickly determine the nitrogen content of a gas and instruct the people operating the purging systems, which were located all the way up the cascade, how much more gas to process in order to keep the nitrogen content down. Nier was a very important factor in the successful performance of the plant, both as an operating unit, without regard to isotope separation, and also for the determination of how successful it was in producing highly enriched uranium.

BOHNING: Could you comment about some of the people you interacted with during the Manhattan Project? Edward Teller, for example.

BENEDICT: Oh, yes! [laughter] Oh, boy! Well, I've written up some of what I've had to do with Edward Teller. Let me see if I can find that. It's better than my memory. Oh, dear, I'm afraid the chapters that I want are in my secretary's office in New York. So I'll have to depend upon memory.

Teller was assigned by General Groves to go around to all of the materials processing plants and determine whether there were any problems that might lead to a critical mass. This was, of course, a quite legitimate concern. We who were designing the gaseous diffusion plant, for example, and I guess even Union Carbide who was supposed to operate it, weren't to be given information on how to determine how much U-235 at what isotopic concentration would represent a hazard and what associated materials such as hydrogen in the neighborhood might make the hazard worse. All these things were highly classified in those days and on the basis of compartmentalization of information were not to be distributed indiscriminately to the individual contractors of the plant.

Edward Teller was the leader of this group. He was instructed to come and discuss with us the proposed design of the plant and look at the flow sheets we had developed. We made the process flow sheets, and the engineering department made some other kind of flow sheets that specified the size of pipe that was to be used to connect the plants, the horsepower of the compressors that were to be used, and things of that kind. He was to determine where and whether in the plant the critical mass of uranium-235 would develop. The Kellogg company was not to be given any of this highly classified information because it might teach somebody in the company with the wrong kind of clearance how to make an atom bomb.

I had the, well, I won't call it a privilege, but I had the assignment of leading Edward Teller through all the flow sheets for the process design of the plant. And Edward Teller was the most——well, autocratic isn't quite the right word. In my later chapters I had an adjective to describe him which was better. But he was a know-it-all, and you couldn't tell Edward anything. [laughter] It was awfully difficult working with him, and it was only because he had a much more able young man also with him, whose name I can't remember, that we were able to convey the right kind of information to him so that he in turn could tell us where in the plant we'd have to reduce the size of the pipes or reduce the amount of uranium hexafluoride that was condensed in cold traps and things of that kind to avoid critical masses developing. He was a very able fellow and a very nice guy.

Oh, here we are. Chapter ten was lent to Clarence [A.] Johnson a year ago. That's why I don't have it. Clarence was my principal colleague at M. W. Kellogg Company. He took charge of the experimental work on separation performance and on corrosion and things like that and I took charge of the theoretical work. But he's got chapter ten and he lives fifty miles north of Naples, so I'm not able to get the chapter for you this afternoon. [laughter]

BOHNING: Is there anyone else that you had to deal with on a regular basis?

BENEDICT: Yes. After the design was far enough along so that we had to transfer the design information to Carbon & Carbide Chemicals Corporation, I went down to Oak Ridge. I had a number of valuable contacts with a fellow we called "Bunny" Rucker, and I can't remember what his initials were. I think they might have been C. N. Rucker, but everybody called him Bunny for some reason or other. He was a very able person. He wasn't the vice president but he was the person in charge of bringing the plant into operation. It was a very smooth transition of information from the design people and myself and others to Rucker and his operating organization. It worked out very well. It was another example of Groves's ability to plan on getting these things properly coordinated. Long before the plant started up, while we were still designing it, I was sent down to Oak Ridge for progressively longer periods of time, just to transfer our design information to the Carbide people, get their reaction upon it, and feed it back into our design planning. Groves did a marvelous job of pulling all that stuff together.

BOHNING: Did you meet with him on occasion?

BENEDICT: Yes. He was a very sarcastic, autocratic person, but we developed a very high mutual regard for each other. I think he had a high opinion of what I was doing and I certainly had a high regard for the success with which he was bringing all these miracles to take place in a short period of time and under incredibly difficult circumstances, because air travel was restricted and the materials we needed were only available with the highest priorities. He did a marvelous job of seeing that the right things and the right people got to the right place at the right time.

BOHNING: You left Kellogg in 1946.

BENEDICT: Well, what happened was that I was assigned to work in Oak Ridge during the start-up of the plant. My boss, P. C. Keith, was a very brilliant and intuitively inspired person who knew that it would be invaluable for the successful operation of the plant to have one of the designers go down and work with the operators, something which Kellogg didn't usually do. It was valuable for me professionally to see how all these almost guesses or inspired decisions that I made as a designer of the plant worked out in practice. He had me assigned to work for the Union Carbide process development organization for about the first six months of the operation of the plant. And, as I've written in my autobiography, this was so inspiring and exciting for me to be down there as the first stages of the plant were in operation, and to see how they performed, not only as well as we had predicted but eventually better than we had predicted.

BOHNING: What happened at Kellogg when the war ended? Did Kellex stay in existence?

BENEDICT: Yes, Kellex stayed in existence, but for some reason the M. W. Kellogg Company didn't want to continue in that line of work. Many of the key people in the Kellogg organization were transferred to Union Carbide to help Union Carbide with the start-up of the plant, and I was one of them. Some of the other people in Kellogg, headed by Al [Albert L.] Baker, who was second in command to P. C. Keith, decided to form their own company to develop processes of military or advanced technological purposes. Many of us were invited to stay with Kellex, which became a separate company from Kellogg, and work for Baker. But I and many others elected to stay with Keith and the project which he had developed in Oak Ridge. We left Kellex and it became almost a skeleton of its former organization, with many of the most original, most able people no longer in it. But it did exist for a considerable period of time and I don't know whether it still exists or not under the name Kellex.

BOHNING: Was the new group that Keith founded called Hydrocarbon Research?

BENEDICT: That's right, yes. Keith formed his own company, Hydrocarbon Research, and wanted many of us, including myself and my principal colleague, Clarence Johnson, to join him. But Groves wouldn't let us do it right away and we both had to continue to work for Union Carbide until their successful operation of the gaseous diffusion plant was assured. Then there was competition for our services between Baker, who was then the head of Kellex, and Keith, then the head of Hydrocarbon Research. I remember having an interview with Morris W. Kellogg, the founder and then chairman of the M. W. Kellogg Company, who was seeking to persuade me to come back. I asked him what my assignments would be and whether I'd have the same kind of opportunities I had at Kellogg before the war, to do the laboratory work on a process, take it through the process design stage, and then follow it through operation, which was of course very professionally rewarding to me. But the Kellogg Company wasn't organized that way. I'd either have to work for their process design department or their process development department or their engineering design department. I couldn't have this opportunity to see all phases of the work, whereas Keith in Hydrocarbon Research would give you that kind of freedom. That and the fact that I had an almost emotional attachment to Keith because of his brilliance, led me to take the job with Hydrocarbon Research rather than with Kellogg. Keith was very good about letting me set up a group at Hydrocarbon Research which was quite similar to the way I had operated at the Kellex Corporation.

BOHNING: You had several patents that came out of this Hydrocarbon Research period.

BENEDICT: Yes. One of them was on mass diffusion (25). Another was on gas absorption (26).

BOHNING: You also did some work on the extraction of deuterium.

BENEDICT: We developed a low-temperature distillation process for extracting deuterium from hydrocarbon refinery gases and from ammonia synthesis gas. Ammonia synthesis gas was a less suitable feedstock because the first and most expensive part of the plant required that the hydrogen be separated from the nitrogen with not too much of an expenditure of energy before one could then go after the separation of hydrogen from deuterium by fractional distillation. But we did design a low-temperature fractionation plant (27). We did the experimental research for that at MIT after I went back there after the war.

BOHNING: After the war you got involved with the Reactor Safeguard Committee of the AEC.

BENEDICT: Yes, that was thanks to Edward Teller, with whom I had had frequent controversy [laughter] while he was overseeing our design of the gaseous diffusion plant, but even so we developed a high regard for each other.

[END OF TAPE, SIDE 8]

BOHNING: What can you tell me about that committee? There were no reactors in those days outside of what the government had, is that correct?

BENEDICT: Well, all the reactors we had in those days were plutonium production reactors or pilot plants to make the design confirmation for the other plants.

BOHNING: What was the purpose of the Reactor Safeguard Committee?

BENEDICT: Well, the first problem that the Reactor Safeguard Committee had to consider was the hazard that the plutonium production reactors at the Hanford site in central Washington might represent, whether in the operation of those reactors there was a possibility of the inadvertent accumulation of a critical mass of plutonium. At that time, as a chemical engineering member of what was then called the Reactor Safeguard Committee, I knew nothing about reactor design. They had physical chemists and chemical engineers and experimental physicists and theoretical physicists. We had Abel Wolman, who was professor of sanitary engineering at Johns Hopkins. He was on the biological or medical side of these things. It was a very good committee. Groves assembled some of the best people in the country for it, and Edward Teller was our chairman. Teller had invited me to serve as the chemical engineering member because he and I had worked together, sometimes at cross-purposes, during the start-up of the gaseous diffusion plant. That was my first knowledge of or exposure to the more important side of the Manhattan Project, namely, the plutonium project. We had known that these sites existed in the West to do something, but because of the compartmentalization we weren't even told what they were producing. We weren't even told that there was such an element as plutonium.

After my first trip out to Hanford to see the world's first transmutation factory, I knew the world would never be the same again. It was really an eye-opener. I must have gone out there either just before or just after I reported to MIT, 1950 or 1951. Do you have my abbreviated biography there?

BOHNING: Yes.

BENEDICT: This is the one I'm looking for. "My prior association with Edward Teller during the Manhattan Project led to my appointment as the first chemical engineering member of the Reactor Safeguard Committee Teller formed for the Atomic Energy Commission in 1947. Our first problem was the possible accumulation of a critical mass in the then being-started-up Hanford reactors, and I was sent out to Hanford to see them.

"The Hanford reactors were located on the west bank of the Columbia River," about fifty or sixty miles north of Richland, Washington, "on <u>exactly</u> the site where back in the summer of 1930

I had slept on a haystack and ran a sprayer and fruit picker in the orchards of a fruit farmer. The contrast between the idyllic, remote, rural setting that I had in this beautiful site on the banks of the pristine Columbia River, and the erection there of the world's first transmutation factory with all the military implications and security safeguards and all the rest of it was such a contrast that showed me how the world had changed irrevocably with the discovery of nuclear fission."

BOHNING: I may be a little ahead with this question, but let me ask it anyway. Given your 1930s experiences in Chicago and then your work on gaseous diffusion for the atomic bomb project, and then moving into nuclear engineering, what was your attitude about the social responsibility of a scientist?

BENEDICT: I think I've even said in my autobiography that I'm ashamed to confess now that I had really no concern or not much thought about the social responsibility or the use of this terribly violent, new explosive that we were developing. During the war, it was simply a challenge to develop the material to bring the war to a brief and successful conclusion. And while I had colleagues who were much more concerned than I was about the future control of this new and violent aspect of society, it was something that I gave very little thought to. I was just concerned with helping end the war and develop this particular means for doing it. I'm ashamed to admit it now, but that was the fact at the time.

BOHNING: Well, Hoyt Hottel was head of fire warfare during World War II and after the war then set up the fire prevention group at NBS in response to his activities.

BENEDICT: And I became a member of the first Reactor Safeguard Committee. [laughter]

BOHNING: How did you make the connection to go back to MIT, within the chemical engineering department? (There was no nuclear engineering department.)

BENEDICT: I'll tell you exactly how it was. (I may even have written it in one of these chapters.) When I was working for the M. W. Kellogg Company in 1943 or so and had been assigned the process design responsibilities for the gaseous diffusion plant, Kellogg had Walter [G.] Whitman (who was then chairman of the chemical engineering department at MIT) as a consultant to come down and view what we were doing. I don't remember exactly what I did that so impressed Whitman, but I knew the process backwards and forwards and I knew all its possible difficulties and how we were going to solve them. I must have explained all this to Whitman in such clear and forceful terms, that it made a lasting impression on him. I don't know whether he was instrumental in getting me appointed to the Reactor Safeguard Committee, but I think he may have been.

When MIT decided after the war that nuclear developments had to be treated somehow in their curriculum, and that the subject was to be taught and research was to be done at MIT, the various department heads——physics and engineering school——were brought together and asked which of them might properly become headquarters for this new activity. Whitman had served in Washington as chairman of some committee or other that had general oversight of some of these processes, and was a very aggressive administrator and a very progressive person. He recognized that there were many elements of chemical engineering in what later became known as nuclear engineering. So he strongly supported having this new activity put in his department and won out over the physics department, which also wanted it and which might have been a more logical place for it because so much of the development was in physics.

Whitman had been a consultant to the M. W. Kellogg Company and came down there on a day when I was starting up a flow calorimeter to measure the heats of reaction in petroleum refining processes. I had never assembled the whole unit before. It consisted of a set of coils through which the hydrocarbon to be cracked or reformed was to be circulated, surrounded by a series of insulating and thermocouple-installed compartments. The whole thing was submerged in either an oil bath or a fused-salt bath depending on the temperature at which we were to be working. I think when Whitman came down it was an oil bath that was to be used around it. We had never assembled the whole thing before. We had the calorimeter in one place and the oil bath beneath it, mounted on a hydraulic lift, but we'd never assembled the unit before. I was so certain that the assembly would proceed smoothly that we waited until Whitman came down so that he could see the calorimeter before it was lost to sight. While he was there I instructed my assistant to throw the switch that would raise the hydraulic lift and bring the oil bath up around the calorimeter. The two fitted together perfectly, and Whitman said that was always what impressed him most about it. [laughter] It's happy chance that directs our lives, and that was one of them.

BOHNING: But you started part-time at MIT in 1951 because you were also involved with something with the AEC.

BENEDICT: That's correct. Even before I went to MIT, I was asked by Marion [W.] Boyer who was then general manager of the Atomic Energy Commission, to start what we decided to call an Operations Analysis group in his office in Washington in order to determine the potential effectiveness of various new processes for the Atomic Energy Commission and how they might be combined together to produce fissionable material in the most efficient manner, either by isotope separation or by transmutation. There needed to be a whole series of processes put together going back all the way to mining, refining of the uranium, production of uranium compounds, and then either production of uranium metal for use in reactors, or production of uranium hexafluoride for use in the gaseous diffusion plant and then the subsequent operation of either the transmutation factories or the isotope separation plants. There were still four in competition——the electromagnetic process, the gaseous diffusion process, thermal diffusion and the gas centrifuge.

Marion Boyer, the general manager of the Atomic Energy Commission, wanted somebody to come down and set up a small office reporting directly to him which would evaluate the potential of these processes, the likely economics, and the most efficient way of combining them all together so that one led to the other and ultimately produced the desired fissionable material at the least overall cost. Whitman was temporarily assigned to Washington, although I forget in what capacity. Even before I reported to work at MIT I was asked to come to Washington and set up this operations analysis staff, which would evaluate these processes, become familiar with their prospective economics, and determine the most efficient way to combine them all together so that with the least cost, the least time delay and the least utilization of scarce uranium resources, the maximum amount of fissionable material (taking into account the relative amount that was needed of uranium-235 and plutonium), how all these things might be operated together in the most efficient way.

I was asked to come down and set up that operation in the first place. I was assisted by a very able fellow from the Y-12 plant in Oak Ridge named Charlie [Charles DeWitt] Thornton. Paul [C.] Fine, who was a theoretical physicist from Los Alamos, brought the precise knowledge of the plutonium processes to our operation, something that I hadn't become familiar with until that time. There was a fourth person whose name I don't remember. We did represent all of the most important components of the Manhattan Project.

We worked together very harmoniously and we helped devise a rule-of-thumb procedure that the Atomic Energy Commission (that's what it was called then, before there was a Department of Energy) could use to decide how much to expand the plutonium production reactors, how many new isotope separation plants to build, and whether they should be the electromagnetic type or the gaseous diffusion type or even the thermal diffusion type. We had sort of a combined operations group. I was there for only about eight months getting it started, but we got it off to a very good start. My deputy, Charlie Thornton, C. DeW. Thornton, took over after I left and made a very successful unit of it.

I think it doubtless saved the government a lot of money because it allocated its resources in an optimum manner among these different processes. We had to estimate how much each one would cost and how much bomb-effective material could be produced. Uranium-235 was only about a third as good as plutonium in terms of mass that was needed for a given bang, so we had to take that into account, and the length of time it would take to start these plants up. We were given different goals as to how much material to be produced at such and such a time, so we developed a combined operations plan which so far as I know the Department of Energy is still using, although its goals are entirely different now, and all of us——myself, and Thornton, and Paul Fine——have long since left the scene.

BOHNING: What was MIT looking for initially? Did they just want to have something about nuclear engineering in the curriculum? Had they thought about going beyond that?

BENEDICT: Whitman and the president of MIT at that time, Karl [T.] Compton, were sure that these revolutionary developments of the Manhattan Project, which were largely engineering based, were bound to introduce a whole new set of concepts and technologies into an engineering education. They felt that it should have a proper place in the MIT curriculum.

It turned out that MIT was the second school to introduce a nuclear engineering curriculum. The first one was at North Carolina State University, where another refugee from the Manhattan Project had gone to teach, a fellow named Clifford [K.] Beck. I don't know if you've heard of him but he was a very able fellow in getting this started, too. They got started about six months before we did and it was quite helpful to me to go down to North Carolina State and see what kind of a curriculum Beck had started himself.

But at MIT I was the first person on the scene and Whitman assigned to work with me after about a three-month hiatus, Thomas H. Pigford, who was my co-author of Nuclear Chemical Engineering (7). Pigford had been director of MIT's practice school at Oak Ridge and was familiar with the process at Oak Ridge. He was a very valuable colleague of mine because he'd had a lot of firsthand experience with these plants as they were started up, both the X-10 plant for production of plutonium on a pilot plant scale, and the K-25 and Y-12 plants for production of uranium-235. It was a very well-conceived idea on Whitman's part to bring Pigford and me together. We had the enthusiastic support of the two deans of engineering, the one that was there when I first came, whose name I can't remember, and his subsequent successor, Carl Richard Soderberg. I don't know if you've heard of him, but he was a very well-known mechanical engineer in the school of engineering. He was a brilliant man. Those deans gave me all the support I needed. They, and the chemical engineering department under whose benevolent stewardship I first served as a division under Walter Whitman, were very helpful and we got off to a very good start.

BOHNING: You had to organize courses, something which had never been done before.

BENEDICT: Well, that's an exaggeration. Cliff Beck at North Carolina State had started some instruction, and I went down there and saw what his courses consisted of and decided what courses we would first offer. As I remember them, they were "An Introduction to Nuclear Engineering," which taught the basic nuclear physics that was needed and then the engineering aspects but on a very elementary scale; "Nuclear Reactor Theory," which made use of the just then being declassified information on how to calculate critical masses and things of that kind; "Nuclear Reactor Engineering," which described the heat transfer and fluid flow and mechanical stress and strain features of nuclear reactors; and "Nuclear Fuel Cycle," the processes which were used to convert uranium ore into uranium hexafluoride and uranium metal and the processes used to refine plutonium after it was produced, after it was removed from reactors, and also how to dispose of radioactive waste, which was an important aspect of the problem even back in those days.

BOHNING: That started in 1951, and only two years later, in 1953, you gave your first M.S. in nuclear engineering.

BENEDICT: Well, by that time the curriculum had increased to perhaps as many as six subjects, and I had in addition to Pigford, a very able theoretical chemist named Mel [Melville] Clark, who was able to teach Reactor Dynamics and other topics that I just wasn't a master of myself at all. And we brought aboard a very able fellow that didn't stay with us long.

BOHNING: Was that [Edward A.] Mason?

BENEDICT: Yes, Mason was my alter ego in terms of chemical engineering and things of that kind. When Pigford resigned to start a nuclear engineering curriculum at University of California, we needed somebody immediately to replace him and Whitman suggested Mason, who had been director of one of MIT's practice schools and wanted to become familiar with nuclear engineering. That started a lifelong friendship and a very valuable multi-year collaboration at MIT before Mason left to become vice president for research at the Standard Oil Company of Indiana.

Then we needed a theoretical chemist, so we brought Mel Clark aboard. We needed a reactor engineer and designer once Dean Soderberg decided it was time that MIT had its own reactor. It was recommended that I interview somebody who was working at Los Alamos, and I did. He declined the job but he recommended one of his associates named Theos J. Thompson, known to everybody as "Tommy" Thompson. Tommy came to MIT and designed our reactor and got it into operation. He taught our experimental courses and really made us a first-rate, well-rounded nuclear engineering department.

[END OF TAPE, SIDE 9]

BOHNING: What kinds of problems did you encounter within the chemical engineering department?

BENEDICT: None. I had no feelings of competition or rivalry or anything like that. It was an entirely different field from the rest of chemical engineering and I guess most of the people in the chemical engineering department were glad that their department had been selected out of the seven different engineering departments in the school of engineering, that chemical engineering would be the one that was given this rather than civil or mechanical or even the physics department itself. There was some rivalry between us and the physics department, because the physicists felt that they were the pioneers, and indeed they were, and they were also the people that felt they knew so much more about nuclear reactors than those chemical engineers that are just sort of on the fringes. And they were right there, too. [laughter] But I had very good collaboration with Clark [D.] Goodman, who taught Nuclear Physics to the physicists and also taught the first courses in Nuclear Physics for Engineers, sponsored by our department. So, it worked out fine. MIT is a very broad-minded school where there isn't a lot of the interdepartmental rivalry that you find in some other universities.

BOHNING: In looking at the chronology, the first M.S. was in 1953. The decision to build the reactor was in 1955. The first Ph.D. was in 1957, and the reactor began operating in 1958.

BENEDICT: Yes. July 1, 1958. I remember it as if it was yesterday.

BOHNING: That's a very rapid sequence of events occurring over a short period of time.

BENEDICT: The enrollment in the department was increasing rapidly during that period of time. The best thing that happened was when the Atomic Energy Commission took over the Manhattan Project, and much of the basic technology was declassified so that it was possible to incorporate the material, which up to that point had required clearance, into the instruction of students. We got not only U.S. citizens but students from other countries who wanted to learn about this new technology. We had students from England and France and Germany and Italy, Japan and Korea, even one from the Philippines.

BOHNING: The department itself was formed in 1958 and you were its first head for something like thirteen years. What were the criteria that allowed that decision to be made, to split you off as a full-fledged department as opposed to being within chemical engineering?

BENEDICT: It was largely internal in my department and it wasn't at my doing. I recognized that our department, to teach nuclear engineering, would have to have people that had a background other than the chemical engineering background that Pigford and I had, the first two beginners of the department. I was partially diversified by having been trained as a physical chemist, but I knew we were badly in need of physicists, and I knew that we needed mechanical and civil engineers, too. I tried to hire people with those backgrounds to go to work in a chemical engineering department, and they would have none of it. Quite properly, they felt that they would be out of touch with their own colleagues.

By that time, Cliff Beck had started a nuclear engineering department at North Carolina State University in Raleigh, North Carolina, and was making a great success out of it. He was attracting a number of students and was having a very good program. With that as an example, I was able to convince the then dean of engineering, Tom [Thomas K.] Sherwood (who had been a colleague of mine in the chemical engineering department), that we needed to have a separate department of nuclear engineering; otherwise, I'd never be able to attract physicists and civil engineers and other people who were needed to teach this new discipline. He agreed, and persuaded the then president of MIT, Karl Taylor Compton, that our enrollments had begun to increase to the point where we could become a separate department and that it was time to split us off from chemical engineering and make a separate department of it. We became a separate division of chemical engineering on July 1, 1957, and we became a separate department on July 1, 1958. I served as the department head for thirteen years, until 1971. My retirement was coming up in 1973 and I felt I didn't want to serve until the very last day. It was time to bring somebody else in before I stepped down. That's when Ed Mason took over for me.

BOHNING: As you started to produce your first students in 1957, what kind of employment opportunities were there for them, especially in those early days?

BENEDICT: It changed a lot from the early days. In the early days there were mostly the Atomic Energy Commission and the very few power companies which were then designing nuclear power plants. Commonwealth Edison was one of our prime sources of employment. We sent a number of students out there. We didn't send very many to the people in the nuclear fuel cycle, even though that was the branch of the subject that I was most familiar with myself. I used to have a record of where all of our students went, but I don't have that anymore. I can't remember some of the companies that employed them.

BOHNING: I asked that because I was curious about how you attracted students to a new program. You said the enrollments were increasing right along.

BENEDICT: The subject was such a popular topic in those days. The newspapers were full of it, and very bright young people felt that there was a real future in that field. But there wasn't, until a lot of the technology was declassified. I can't remember just the year in which that took place, but that made a big difference. Prior to that time, most of the employment was with companies that required security clearance for their students, or with foreign governments or companies like Electricité de France, who needed to have people who were trained in as much of the U.S. nuclear technology as the government was willing to allow us to share with foreign nationals.

BOHNING: When did the private sector power companies enter on the scene to the extent that it affected what you were doing in your department?

BENEDICT: There was a point when the field took off. I think it was in the early 1970s, but I'm just not that sure of it.

BOHNING: What's the current status of the department? Has it fallen on hard times, given what's happened in recent years?

BENEDICT: No, it's maintained its enrollment at about some two hundred students. It's teaching both an undergraduate and a graduate degree. It didn't have an undergraduate program when I was a professor there. The undergraduate program is not very well attended. They get only a few students a year. I'm not in very close touch with it anymore——but I think the graduate program still has something like two hundred students enrolled, which is quite sufficient. In fact, it's not the smallest department of the engineering school. Naval architecture was always smaller than we were and I think still is. The students have no trouble finding employment. Some of them have advanced to vice presidencies and in one case the presidency of a company, so I guess their education served them well.

BOHNING: What kind of support did you have for your graduate students?

BENEDICT: Well, for many years the Atomic Energy Commission had fellowships in nuclear engineering, which were awarded on a competitive basis and which our students were quite successful in obtaining. Sometimes students from other schools would get a fellowship that allowed them to choose their school and they came to MIT.

In addition, faculty members sought and succeeded in getting research projects from various companies, both those that were producing reactors or materials, or had nuclear power plants of their own. Commonwealth Edison, again, was quite helpful in providing us with research funding. I've lost touch with the department now and I don't really know where its sources of support come from now. But in the beginning they were very glad both to provide support for our instruction or our research, and also to have that contact with them which permitted them to come to us and pick our brains.

BOHNING: We've talked a little bit about some of your AEC activity, but maybe we should also talk a little bit about your membership on the General Advisory Committee. You were appointed by President Eisenhower, is that correct?

BENEDICT: Yes, that's correct. "From 1948 to 1958 I was a member of the Atomic Energy Commission's Reactor Safeguard Committee and its successor, the Advisory Committee on Reactor Safeguards. In 1958 Eisenhower appointed me to the General Advisory Committee of the AEC. I served on that committee until 1968 and was its chairman from 1962 to 1964."

BOHNING: What was the function of the General Advisory Committee?

BENEDICT: That was a committee that reviewed all of the activities of the AEC, not just its reactor safety problems, but everything that it did from mining of uranium ore to the production of fissionable materials to the design of power reactors to the conduct of biomedical research to the development of extremely high-voltage accelerators——everything that the Atomic Energy Commission was putting money into. For many years (and it probably is still true although I've lost track of it) it was a wide spectrum of all the fields of research that are conducted in the country. At any rate, it was a very valuable way of keeping my knowledge of nuclear engineering which could easily have become somewhat obsolete from so doing because I was right in touch with the latest developments, both in the governmental and in the civilian field, because we had many opportunities to visit the companies that were running nuclear power plants or producing nuclear materials.

BOHNING: Did you ever have contact with Admiral [Hyman G.] Rickover in this regard?

BENEDICT: Oh, yes! Didn't I ever mention that? Wasn't that in any of this?

BOHNING: No, I didn't see it.

BENEDICT: I've written that up somewhere. I'd much rather find what I wrote than try to drag it out of my memory. But anyway, you've asked the question and I'll tell you what I can remember about it.

MIT got into nuclear engineering largely because Rickover had selected promising naval officers to acquire an education in this new technology and assigned a few of them each year to MIT. (He had other schools that he sent them to also.) They first worked in the physics department because the physics department taught nuclear physics and there wasn't any instruction in nuclear engineering in the engineering school. When they came back to him after having had their MIT education, he would quiz all of them to find out what they knew and what they didn't know. He was appalled to find that even though they went to a preeminent engineering school, their education at MIT was largely nuclear physics. He felt that MIT should provide an engineering education to his chosen naval officers, and conveyed that view with appropriate colorful language, I assume, [laughter] to the dean of engineering, Tom Sherwood, who had been in the chemical engineering department until he became dean of engineering. He recognized that Rickover had a proper objection to the kind of education that MIT was giving these people. Rickover urged Sherwood to see that his naval officers were given appropriate instruction among the different departments of the engineering school that would round out their education in physics. He wasn't against their learning nuclear physics, but he felt they ought to know heat transfer and fluid flow and materials properties and things of that kind as well.

Sherwood called a meeting of the heads of the seven engineering schools, which in those days included chemical engineering, civil engineering, electrical engineering, mechanical engineering, aeronautical engineering, and food engineering. They put their heads together to try to see which one could serve as the environment for starting this new instruction. Walter Whitman had just served as a consultant to the M. W. Kellogg Company, and he recognized this as a golden opportunity for the chemical engineering department to break into a new field. I guess he made the most appropriate or most persuasive pitch for providing a home for this new department and was assigned as its first mentor.

He then cast about for somebody to start this new program. He assigned one of his then junior faculty members, Tom Pigford, to go to Oak Ridge as the professor in charge of MIT's school for chemical engineering practice at the Oak Ridge National Laboratory, feeling sure that Tom would pick up nuclear engineering that way. And he wanted somebody else of a more senior capacity (Tom had just got his doctorate) to serve as Tom's colleague and senior faculty member in the department. He was there when I assembled the calorimeter for the first time and the fact that it fitted together perfectly for the first time made such a lasting impression on him, that when he needed somebody to start nuclear engineering at MIT, he knew that I was the man.

BOHNING: I'm at the end of my list of questions. I had about eight pages here, and I've slowly gone through what I had.

BENEDICT: Good. I didn't realize we'd taken up that many, but I'm glad we covered them all.

BOHNING: Is there anything else along these lines that you would like to add that we haven't discussed?

BENEDICT: Well, the last sentence in this brief narrative autobiography is one that I'd like to emphasize. "I would like most to be remembered for the part I played in educating five hundred men and women who studied nuclear engineering at MIT while I was a faculty member there and who are now contributing to the development and use of this important energy source."

BOHNING: I think on that note we'll close at this point.

BENEDICT: [laughter] Okay, fair enough.

BOHNING: I appreciate your spending all this time with me.

BENEDICT: Well, I appreciate and am flattered that you felt it was worth this much of your time.

[END OF TAPE, SIDE 10]

NOTES

1. C. Harry Benedict, "Ammonia Leaching of Calumet Tailings," Engineering and Mining Journal, 104 (1917): 43-48.

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