CHEMICAL HERITAGE FOUNDATION

DONALD F. OTHMER

Transcript of an Interview
Conducted by

James J. Bohning

at

Polytechnic University

on

2 April 1986, 11 June 1986, and 15 January 1987

With Subsequent Corrections and Additions

"THE REWARD TO THE EDUCATOR LIES
IN HIS PRIDE IN HIS STUDENT'S ACCOMPLISHMENTS.

THE RICHNESS OF THAT REWARD IS THE SATISFACTION IN KNOWING
THAT THE FRONTIERS OF KNOWLEDGE HAVE BEEN EXTENDED."

DONALD F. OTHMER

THE CHEMICAL HERITAGE FOUNDATION
Oral History Program

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(Signature) Donald F. Othmer
Donald F. Othmer

(Date) June 17, 1994

(Revised 17 March 1993)

Donald F. Othmer, interview by James J. Bohning at Polytechnic University, 2 April 1986, 11 June 1986, and 15 January 1987 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0035).

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

DONALD F. OTHMER

1904	Born in Omaha, Nebraska on 11 May

Education

1921-1923	Armour Institute of Technology (now Illinois Institute of Technology)
1924	B.Ch.E, University of Nebraska
	University of Michigan
1925	M.Ch.E.
1927	Ph.D., chemical engineering

Professional Experience

1924	Chemist, Cudahy Packing Company
1927-1931	Chemist and development engineer, Eastman Kodak Company and Tennessee Eastman Corporation
	Polytechnic University, Chemical Engineering Department
1932-1933	Instructor
1933-1937	Assistant professor
1937-1961	Professor and Head of Department
1961-	Distinguished Professor
1931-	Consulting chemical engineer
	Registered professional engineer: New York, New Jersey, Ohio, Pennsylvania

Honors

1958	Tyler Award, American Institute of Chemical Engineers
1958	Barber-Coleman Award,American Society for Engineering Education
1962	D. Eng. (honorary), University of Nebraska
1970	Honor Scroll, American Institute of Chemists
1975	Award of Merit, Association of Consulting Chemists and Chemical Engineers
1975	Golden Jubilee Award, Illinois Institute of Technology
1977	Chemical Pioneers Award, American Institute of Chemists
1977	D. Eng. (honorary), Polytechnic University
1978	Murphree Exxon Award, American Chemical Society

1978	D. Eng. (honorary), New Jersey Institute of Technology
1978	Professional Achievement Award, Illinois Institute of Technology
1978	Perkin Medal, Society of Chemical Industry
1981	Hall of Fame, Illinois Institute of Technology
1987	Mayor's Award of Honor for Science and Technology, New York City
1989	Outstanding Alumnus Award, University of Nebraska
1989	Citation for Improvement of the Quality of Life, Borough of Brooklyn
1989	Award for Significant Contributions to the Polytechnic University
1991	Founders Award, American Institute of Chemical Engineers

ABSTRACT

Donald Othmer begins the interview by sharing memories of his childhood in Omaha, Nebraska, his parents and schooling. He tells of his experiences as a student at the Armour Institute, the University of Nebraska and the University of Michigan. He describes his years at Eastman Kodak, his accomplishments there and his reasons for leaving to produce his own material. He then discusses the early years at Poly Tech and the students he has helped to shape. Othmer also tells of his industrial work over the years. He describes his adventures in Burma and his association with the Government during World War II. He continues by recounting his experimental endeavors, his patents and the inception of the Encyclopedia of Chemical technology. Othmer concludes by discussing the Chemists' Club, his life in Brooklyn and the past and future of chemistry.

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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1 Family, Early Years and Schooling

Grows up in Omaha, Nebraska under modest financial circumstances. Avid reader as an adolescent. Father is in sheet metal business, mother is active in the community. First jobs include paper route, stacking books and delivering telegrams for Western Union. Graduates Omaha Central High School in 1921.

4 Advanced Education

Receives scholarship to Armour Institute (now Illinois Institute of Technology) for two years. Transfers to the University of Nebraska and completes remaining undergraduate work in one year. Attends the University of Michigan and works under Professor Walter L. Badger. Receives his master's degree and Ph.D.; discusses working with Professor Badger and his other professors at the university.

15 Eastman Kodak

Meets recruiter from Eastman Kodak in March, 1927, and accepts position. First problem is how to concentrate acetic acid from its dilute solutions. Discusses the "boil around system" and the development of the Othmer Still. Also involved in working on the development of cellulose acetate, extractive distillation and the uses of methyl tertiary-butyl ether (MTBE). Explains systems used by George Eastman at Kodak Park.

40 Resigns From Kodak, Develops Own Facilities

Frustration over use of the patents he developed and desire to produce his own market for his work. Kodak prefers that he get into management for the company, for which he has no interest. Sets up own facilities and continues work on azeotropic distillation. Also works as a consultant for the American Chemical Products Company in exchange for laboratory facilities. Offer from the Standard Oil Company of New Jersey (now EXXON) to to develop synthetic rubber.

45 Polytechnic Institute, Brooklyn, NY

Teaching position offered in 1932. Chooses Poly for both academic and industrial freedom. Discusses the newly formed chemical engineering department, first classes, first students and the Depression's effect on them. Shares teaching methods and philosophy. Earns chairmanship in 1937.

53 Industrial Endeavors

Works with Tennessee Eastman as a consultant. Hopeful about the sale of his patent, but legal problems occur with Kodak. Gray Chemical Company offers job to develop acetic acid. Helps to build plant in Roulette, Pennsylvania, for production of methanol and acetic acid. Discusses the effect of charcoal and wood chemicals. Buys land in Pennsylvania.

76 World War II

Develops key process in producing RDX. Trained by the Government to evaluate the German chemical industry. Builds a plant to ferment alcohol in South and Central America. Teaches enlisted men at Poly. Class sizes increase dramatically after the war.

88 Burma

Works with TAMS in the early 1950s to design basic needs for the country. Declines to live there, but sends contact to Rangoon. Interested in the human needs and health problems of the Burmese people. Considers Burma "one of my greatest professional experiences." Shares experiences of subsequent travels.

93 Technical Work

Summarizes extracting techniques. Devises method for separation of wood constituents with the Northwood Chemical Company. Two year association to do research with the American Sugar Refining Company. Experiments with temperature differences in the ocean as a source of energy. Desalination for the Saline Water Conversion Corporation. Works with PROUST for expedition of domestic and industrial sewage. Explains applications of Wet Air Oxidation (WAO) and pipeline heating.

111 Patents

Explains the differences between patents (experiments) and "gray matter" patents (studies). Discusses original patents with Kodak. Argues own case with the Patent Office.

117 Experimental Work

Experiments with the affinity series of metals. Works with German engineers Gerhard Holland and Hanz Zimmer. Further work with azeotropic distillation. Discusses the "Othmer Plot."

128 Encyclopedia of Chemical Technology

Developed at Poly with Raymond Kirk and Eric Proskauer. Described as a "public service for the profession." Considered counterpart to Ullmann's German <u>Encyclopedia</u>. Explains process of choosing authors, topics, and content.

INTERVIEWEE: Donald F. Othmer

INTERVIEWER: James J. Bohning

LOCATION: Polytechnic University, Brooklyn, New York

DATE: 2 April 1986

BOHNING: Professor Othmer, you were born on May 11, 1904 in Omaha, Nebraska. Can you tell me something about your father, Frederick and your mother, Freda?

OTHMER: They were very dear people, to me of course. My father was in the sheet metal business, installing furnaces, sheet metal roofs, and dust collecting systems for local grain mills, etc. My mother was very active in the community, particularly in church related work.

I grew up in very modest financial circumstances. In fact, I believe that I have earned every dime I have spent since I was ten years old, first from a paper route which I carried for thirty-five cents a week. The paper route was about two miles long, six times a week, but after a year or so, I was graduated to a dollar a week with one that was three times as many steps to encompass in the several square miles of the then sparsely settled land of northwest Omaha.

Always in the early years, there were many odd jobs done for a pittance: e.g., digging dandelions or mowing lawns for neighbors, or walking a cow to and from a pasture a half mile away. Then it was delivering telegrams for Western Union on a bicycle. I had been able to purchase that from my savings. While in early years of high school, after school I fed a Gordon printing press at fifteen cents per hour, or replaced books in the high school library stacks at ten cents per hour.

As to early thoughts as to physical or chemical phenomena. When I was five or six, I recall that little boys wore black cotton sateen blouses with white celluloid collars. My black blouses had white dots, about one-sixteenth inch, on one-half inch centers. These blouses had lots of washes and, as they were wearing out, I noticed that the cotton of the dots showed fraying of the white threads inside the dots. I commented to my mother that the cloth must have been dyed black; then, the dots were printed with some material which removed the black to make the dots, and this had also attacked the cotton threads to make them less resistant to wear and many launderings.

There was an earlier laughable mistake in my understanding of a common physical cause and effect. There were many trees around our home, and my earliest aerodynamic analysis, if such it be by a two or three year old, was that the movements and fannings of trees caused the winds, rather than vice versa.

My first public speech—if indeed it qualifies—was at a Sunday School Christmas performance for the church in 1907, I believe. With my hand behind my back in a white Buster Brown suit (from the then popular "funnies"), I marched, hand behind my back, down the aisle to the platform, "I'm all dressed up for Christmas, and everything is done. My stocking is not large enough, so I had to borrow one." Then I threw out my hand and waved a long white lady's stocking which I had carried in a role.

Somewhat later, in 1910, I do remember seeing Halley's comet and not understanding why all the fuss for just another star in the southeastern sky, not high above the horizon and somewhat elongated. Many of the neighbors stood out in our front yard. I was up an hour later than usual to get the look, which I was told would have to last me for a long time, and it did for some seventy-six years until I again could see Halley's sparkle.

When I was ten, in going to school I had to cross diagonally or to walk on the street around a rectangular vacant lot of about an acre. Of course, instinctively I realized (as would a dog or many animals) one of Euclid's early conclusions, that the diagonal is the shortest distance between the two opposite corners. I estimated that this length, that of the diagonal (c) of any right triangle, must be equal to the length of the longer side (b) plus one-half the length of the shorter side (a), or $c = b + 1/2a$.

Four or five years later, my high school teacher told me of Euclid, who had noted 2300 years ago that Pythagoras, a century before, also had been making short cuts. Pythagoras said, correctly, that the diagonal was equal to the square root of the sum of the squares of the two sides forming the square angle. I accepted, but many years later calculated the error of my estimate. It was within five percent of being correct for all values when the long side is more than nine times the length of the shorter, but quite incorrect (over ten percent in error) when the longer side is from about 1.5 to 3 times the length of the shorter.

I went to the public schools in Omaha and was fortunate to be able to proceed at a somewhat more rapid rate than most of my peers. I attended Omaha Central High School, which was one of the fine high schools of the country. It was noted in various reviews of all high schools in the country as having had, in the 1920s, one of the highest percentages of students continuing in colleges, ultimately to obtain the Ph.D.s.

My chemistry teacher, Dr. [H.A.] Senter, was by far the best, most enthusiastic professor of all my teachers on through several universities. He received his doctorate at Heidelberg University in chemistry in about 1898. He had been the teacher of my wife's mother, and years after me of my wife, all in the same high school building. (Also Jim Parmalee, editor of Chemical and Metallurgical Engineering, whom I knew in the late 1920s, had been inspired by him in 1908, also others known in the chemical field.) About 1900, he had built the chemistry laboratory at Omaha Central High after the pattern of the laboratory in Heidelberg which [Robert] Bunsen had built some fifty years before. Matches were expensive in 1850; and we, as did Bunsen's students, lighted our Bunsen burners also from a small flame always burning in the center of a bench for four students.

BOHNING: You took your first chemistry then in high school. Did you have any previous exposure to science?

OTHMER: Only physics, except for reading. I was an avid reader in those days and I've often thought that I probably read more books before I was sixteen than I've read since, certainly much more fiction. I've spent much more time in writing in later years. But I did enjoy reading various books on the sciences and whatever came to hand otherwise.

I developed early a considerable mechanical ability and my hobby was all sorts of mechanics of any kind which came to hand, particularly those jobs in woodworking and metalworking, which I was able to do.

BOHNING: Did your father have any influence on that?

OTHMER: Yes. My father had a shop and I had a good time working with his tools, not with his own work, but making various simple furniture and other mechanical work. I remember that at about six, my first mechanical drawing was a half-size drawing of the face and body of a large pendulum clock with an octagonal frame hanging on our kitchen wall.

BOHNING: Until you took your chemistry course at Central High, had you given any thought about what you would do after you left high school?

OTHMER: No, not particularly. As I said, Dr. Senter inspired me to do something in chemistry and I did enjoy the mathematics courses that I took, particularly geometry and solid geometry. I remember my math teacher then asking me what career I wanted to follow and I said I supposed it would be something in engineering. This was before I had met Dr. Senter in my senior year. Of course, the combination was chemical engineering which fortunately, I have been able to follow ever since.

And looking back at the many years since, I have been fortunate in these many tasks of always being able to do just what I wanted to do. By and large I have enjoyed all of my life's work.

Doubly lucky that I have been in this because never as I grew up did I have anyone to talk to about professors in life or their tasks. We didn't have counsellors on such matters in high school in those days; and while I never had any disagreements with my teachers, I never made occasion to chat with them on any subject.

BOHNING: Are there any particular books that you recall that had any influence on you?

OTHMER: No. I don't recall any particular books of the many I borrowed from the library. I read the magazines <u>Popular Mechanics</u> and <u>Popular Science</u> and also others, written for boys; but no real regular scientific literature came my way. I remember a book, by Slosson, I believe (1). I forget the title but it had to do with chemistry and its relation to industry and particularly in relation to the culture of mankind.

BOHNING: You graduated from high school in 1921, I believe, and then went to Armour Institute in Chicago [now Illinois Institute of Technology].

OTHMER: Yes. I had a scholarship that was given by the Board of Education in Omaha. Also, Armour Institute then added a tuition scholarship to that. So I went for two years to Chicago as a student in chemical engineering.

BOHNING: Was that scholarship from the Board of Education directly for Armour Institute or did you have your choice?

OTHMER: Yes, I could have used the Board of Education Scholarship almost anywhere. As a matter of fact, I also had a tuition scholarship from one or two other schools, one of which was Case [Institute of Technology] in Cleveland, but that was twice as far from home, and I knew nothing to make me want to go there, so I went to Armour Institute which was closer.

BOHNING: You were there for two or three years?

OTHMER: I was there two school years, 1921-1922 and 1922-1923.

The head of the Chemical Engineering Department then, Professor [Harry] McCormack, was very typical of professors of chemical engineering in those days. He was an analytical chemist, and his idea of chemical engineering was that one must be trained to be a first class analytical chemist because that was the only type of job involving chemistry as a starting point in industry. Thus a chemist or chemical engineer must start in industry as an analytical chemist. So, if he was a first class analytical chemist, he might have a chance to graduate into something on a larger scale than the milligrams of the analytical chemistry, and ultimately he might get into the design or operation of plants making tons of some chemicals. But, necessarily, in order to get a start, he must be a top-notch analytical chemist.

So, we had what was to me a dreadful lot of analytical chemistry in those two years, and I detested all of it. After the sophomore year, the chemical engineering program called for spending the complete summer in analytical chemistry before entering as a junior. This was to be rock analysis, mainly gravimetric analysis; and I detested the use of the tools as they were then available for analysis of rocks.

(I didn't conclude then, but I have often thought since that each person feels most happy and does his best work when he is working with his own particular size or scale of measures, which he understands best and with which he is most comfortable. Thus, a nuclear physicist or chemist is most happy when he is thinking, calculating, and working with the small components of the atom—always less than Angstrom units—the physical chemist and the theoretical organic chemist with the mutual arrangements of atoms into molecules—possibly up to Angstrom lengths; the biochemist and the polymer chemist with super or macromolecules; the analytical chemist classically in milligrams and parts per thousand, now in micrograms and parts per billion; the usual organic chemist in grams or milliliters—up to pounds or kilograms; and the chemical engineer in gallons and tons, feet or meters. Each technician should be working with those operations and on the scale which he can visualize, feel, think, and work most comfortably. Personally, I have never pictured well or had a feel for micrograms or micrometers on the one end of the scale of values, nor for the astronomer's billions of light years on the other hand.)

With this general feeling and my cordial detest of "slopping around" with filtrates, their drying, weighing and evaluation, I decided I wouldn't take rock analysis in the summer of 1923; indeed if that was chemical engineering, then that was not to be my "cup of tea." I had taken various courses at Armour in mechanical engineering in addition to my chemical engineering. Thus I found that if I were to transfer to the University of Nebraska at Lincoln, I would be able to get my bachelor's degree, or very nearly, within one year.

I prepared carefully a listing of all courses I had taken at

Armour and the work involved in each, and on the other half of the sheet each comparable course at Nebraska with the same detail. This got me credit for everything I asked for from the advisor! (In my later evaluating at Poly the background of hundreds of students entering from other schools, I have never had a single detailed application as I submitted—certainly no student has ever done better in getting what he asked for than I did!) However, I would be short three or four credit hours when applying for a bachelor's degree in the spring.

When I came to register in the fall at the University of Nebraska, I found that I was limited, as at many other schools, to taking only eighteen credit hours a semester or thirty-six hours for one year. As I remember, I needed thirty-nine. So, I read the catalogue thoroughly and found a sentence that said that "entering students, if they have taken certain extra courses in high school, might apply to take an examination in those courses within the first semester after they entered the University, and if they passed, they would be given college credit for these extra high school courses."

Obviously, this was intended for freshmen, and I was entering as a junior at least, but nevertheless, that was the wording; and the dean went along with that general reading. In high school, I had taken Solid Geometry which I had enjoyed; and in it I had received a mark of 104% instead of the usual maximum of 100%. This was possible because I had done some extra problems that were "original proofs" and the teacher always added to the final grade, one, two or three percent for each, depending on the difficulty of those problems. So with a high general grade—the final one could be over 100% but reported as A+-the dean agreed that Solid Geometry was an extra high school course, so I took an examination in it for two credit hours. The unfortunate thing in taking an examination in solid geometry a long time after taking the course, is that you don't remember which theorem follows which.

Ten "originals" were given, to be proven as the examination. I looked them over and couldn't see any starting point for one; however, I thought I could prove the other nine. But, the difficulty was I couldn't remember if any one step had sufficient foundation to be the basis of any earlier step of my proof because I didn't know whether that step indeed had been proved in the overall syllogism of Euclid's order of presentation. So, what I had to do was to prove each of these nine long demonstrations all the way back to the axioms. I did this, and ninety was the grade!

The other examination for one credit hour was in Mechanical Drawing, which I had had in high school and which I had enjoyed. I am still a fair draftsman. Meanwhile, I had taken Engineering Drawing at Armour, so there was no trouble in passing that exam.

I don't know whether the rule is still in the catalogue of the University of Nebraska, but it did enable me to make up the essential three credits which I had to have, so as to be able to continue as a senior and graduate after my third collegiate year.

This senior year was rather filled. I remember working out a display for Engineer's Week. I had developed a little skill as a glass blower for simple laboratory equipment and I formed from 3/4 inch glass tubing, the script Chem Eng with electrodes at either end. This was evacuated, placed over our display and when electric current was applied, gave the first electric sign with continuous letters any one there had seen, a Geissler tube put to work in a useful form.

It was some years later before similar signs became common, with a very small amount inside of neon to give its brilliant red glow, instead of the less bright light which I obtained by simple vacuum pumping. I was too inexperienced to appreciate the possibility of such tubes and signs—but I have often wondered why no older person seeing my primitive display did not capitalize on the concept before the French did at first.

My pal at the University and I had wanted to spend the summer after graduation taking a canoe trip down the Missouri River to New Orleans, starting at Omaha, because we had bought and rebuilt an old canoe and we enjoyed its use. Unfortunately, his father told him that he had to go work, so that cancelled that trip. I also was out of money for the first and only time in my life. My scholarship from the Board of Education had not been available; and I had supported my self at the University by working nights in the library. I was broke, and I had to borrow money, some forty dollars, from my father. I soon paid that back however, because I got two jobs.

One of these, in the daytime, was—true to Professor McCormack's prophecy—as an analytical chemist at the Cudahy Packing Company. This time, I was not doing gravimetric analysis but doing various other types including nitrogen by the Kjeldahl method, and amino acids by the Van Slyke determination. Such semi-instrumentation analyses were not so bad. Besides, the meat packing industry itself was very interesting.

For evenings, I found a job in which my father's friends in the sheet metal workers union were interested. I taught them descriptive and solid geometry in practice, e.g., how to make transition sections between cones and cylinders of duct work, thus to make from sheets what are called "warped" surfaces. These are surfaces which can not be delineated by moving a straight line, as can a cone or cylinder. That was also a very interesting occupation. So, that summer of 1924, I had two jobs, one in the day and one in the evening.

I went down in the summer of 1924 to the house in Lincoln of the Alpha Chi Sigma fraternity, which I had joined at the University of Nebraska. I found a letter addressed to me on the newel post. A fraternity house in the summertime is rather disorganized; and this letter had been waiting for me there for several days. It offered me a fellowship to pay all expenses at the Carnegie Institute in Pittsburgh. It had been dated ten days earlier; immediately I wired my acceptance. Almost immediately I received a response that unfortunately, they had to fill the fellowship almost momentarily because it was only a month until school started. Had this letter been received on time, my education, specialization, and life work would have been entirely different.

Then, I applied to and was accepted by the University of Michigan; and at a later time, they advised me that I was given an assistantship with a small stipend of $250. This was not much for fellowships or assistantships compared with the 1980s. Fortunately, the Board of Education gave me the same $600 per year scholarship which I had had at Armour, and which was now again available.

At the University of Michigan I was the assistant in the Evaporator Laboratory under Professor [Walter L.] Badger. I worked under him for the next three years for both master's and doctorate degrees. W. L. Badger in the 1920s and 1930s was probably the best known chemical engineer in America in the field of heat transfer and process design, particularly in industries where evaporation and crystallization were important.

My work was somewhat of a challenge but most interesting because it involved a great deal of mechanical work in handling rather large equipment and doing necessary maintenance. This included pipe fitting with up to two inch standard pipe, and this is about as large as one wants to cut and thread by hand. As the first assignment Badger said, "Othmer, there are some valves in that header which are leaking around the stems. You'd better go pack them." I had never packed a valve and I was surprised. But I went to the lab's storekeeper and said, "Hans, please give me the stuff I'll need for packing a valve." Well, he gave me the necessary graphite and cotton cord to fill the packing gland and also the wrenches and a pick with which to dig out the old packing.

After seeing the packing cord, pick, and wrenches, I realized what was to be done and I packed the valves. There was a fair amount of similar work to be done because the water in Ann Arbor is extremely hard, and it causes trouble in pipes and fittings due to corrosion and rusting and otherwise. So, I became proficient in maintaining the valves and the large number of other fittings, plug cocks, controls, pumps, evaporators, etc.

In front of one evaporator was needed an assembly which was difficult to make. It was in the form of a two-foot square with tees and pipes going out of three corners. It had twelve 1 1/4" or 1 1/2" fittings and almost thirty threaded joints. I did the job, cutting the threads where necessary, using nipples, of course, with the control fittings and valves. And, just as I was preparing to turn the water on, the boss, Professor Badger, turned up, and he turned on the water. My assembly leaked in almost every joint. I was chagrined. He said, "Othmer, don't worry. Just leave it go. By the day after tomorrow, those joints will all have been rusted shut." And as a matter of fact, they did rust shut. So I learned that sometimes, one did not have to be as good a pipe fitter as ultimately I did become, when the hard Ann Arbor water would seal less than perfect joints, mainly with rust.

I worked my master's year under Badger, who was one of the school of dogmatic, tough talking professors, bouncing down on every student seemingly at every chance. All of us were quite afraid of him. I remember him lecturing, smoking a pipe and pacing back and forth at the front of the room, or descending on you if you were at the blackboard and blowing smoke in your face. As to himself, he said he had learned his chemistry at the university (master's in physical chemistry) and his engineering in studying manufacturers' catalogues.

Badger never was particularly vicious with me but I remember the chap I was working with on my master's problem. This was Larry Phillips, who was two years further along than I, as he received his doctorate the next spring, when I had my master's. But, he was more of a philosopher than an engineer; and his sketch of the evaporator which Badger had told him to make on the board was somewhat less than a working drawing. All of us had been sent to the board for fifteen minutes with individual assignments. Badger then came and criticized our individual work. "Phillips, do you think that thing is going to hold together with haywire and glue?" Of course, Phillips was dismayed.

But Badger was not such a bad fellow after all. At the end of the semester, he called me in and said, "Othmer, I went through all of your work twice and I wanted to give you an <u>A</u> but I couldn't. I had to give you a <u>B</u>." I always appreciated this serious concern because sometimes I had been a little indignant at some of his comments; however, he was not quite as impatient and arrogant as he sometimes seemed. That was my first year of graduate work, and it netted me my master's degree.

After that, I sold vacuum cleaners during the summer of 1925. I was not a very good salesman. I was a better cleaner for the rugs of the ladies on whom I called.

But, I sold a few and I came back to Michigan, having received my master's degree but had no fellowship or scholarship. However, I wrote to the Board of Education in Omaha to the effect, "There was this scholarship which was meant for me to use for <u>four</u> years. I had it for <u>two</u> years at Armour. I didn't have it at Nebraska since it was not given that year. I have had it now <u>one</u> year at Michigan and in these four years that I have been in colleges, of which you have given me the fellowship for three years, I have received two degrees!—a bachelor's degree at the University of Nebraska and a master's degree at the University of Michigan. Now I would like to continue my doctoral program and I'm applying again for the fourth year of the scholarship."

After due process, the Board of Education agreed that since I had it for only three years and it was normally meant for four years, they would give it to me for a fourth year—my fifth collegiate year—which they did. And so, I received it that year. Then the same sort of appeal for the final year allowed me to get my doctoral degree in my sixth collegiate year—the three graduate years all being under Badger.

BOHNING: So you did your Ph.D. work under Badger?

OTHMER: Yes. I received my Ph.D. under Badger in 1927. Having worked under Badger, whose specialty was heat transfer and evaporation, I had expected I would go into some one of the industries which he had followed, and for which he was also a consultant—the chemical equipment industry, the salt and related chemicals industry, the sugar industry, or some other industry using many evaporators. However, in March 1927, there came from Eastman Kodak a recruiter, Earl Billings, and he interviewed me. I was really so very busy finishing my thesis that I didn't pay much attention to him or to the idea of getting a job with Eastman Kodak because I didn't think that was something for me.

However, in a week or so, I did receive a request to come to Rochester and spend the day with him and discuss the possibility of a job, which I did sometime in April. Billings was the chief recruiter for Eastman, but he happened to be at home sick the day of my appointment in Rochester. So, he couldn't be there as my host. I was very well taken care of, however, and enjoyed looking around the offices and meeting very many nice people whom I later got to know and enjoy working with.

[END OF TAPE, SIDE 1]

In the evening, probably six o'clock, because Eastman worked until five forty-five, I was in the office, not of the personnel department, but of the manager, Mr. [James H.] Haste. In the absence of Billings, he had the task of offering me a job because evidently, he had favorable reports from people with whom I had talked.

He asked me how I enjoyed the day. I said, "Well, I met many fine people that I would like to know better, and I saw many fine offices. But, I didn't see anything of your plant or any of your operations. And of course, I'm an engineer and that's what I would have enjoyed seeing."

"Oh, Mr. Othmer," he said, "Please, we had to spend all of the time examining you, but if you stay tomorrow, we'll show you everything." I laughed, because I already suspected what I found in the years later when I worked for Kodak. I learned well that I would see far from everything. Eastman was then and may still be the most secretive industrial organization in the world.

However, he then came to the point by saying that he would like me to come to work. He was authorized to offer me a salary of $2500 a year. That is a ridiculous amount these days, in the late 1980s for a salary for a man with a doctorate; but at that time, it was not far off. In fact, doctoral men starting in 1927 were getting up to $3000 and so I said, "I am complimented Mr. Haste, but I don't think that's quite enough." And he hemmed and hawed a bit, a little embarrassed because I was the first chap he had hired—in Billing's absence—although he was a vice president and general manager of Kodak. I finally said, "Mr. Haste, I'm a good pipe fitter, and if I came in the Kodak Park entrance, where blue collar people come to visit the personnel office there, I believe I could get a job as a journeyman pipe-fitter. I believe your people would pay at least one dollar an hour and you work a 48 hour week. So that would be $48 a week and if you multiply that by fifty-two, you would see that I would be getting just as much as a journeyman fitter as I would with a doctorate."

Well, Mr. Haste was a little embarrassed and maybe irritated at my frankness. He said, "Mr. Othmer, would you please step outside for awhile." And so I did. Meanwhile he telephoned Billings who was on a sickbed in his home, and Haste soon asked me to come in and he said, "I'm embarrassed. I was supposed to offer you $2800 or $2900, somewhere in between, but since I made the mistake, it's $3000 as you asked and if you please, don't go to our bidder for our mechanics."

He continued, "Your buddy from Ann Arbor is coming down tomorrow, Marvin Rogers, and he has a master's degree. And so what I was offering you was a mistake. It was what we may offer him tomorrow as a master's degree man." Well, Marv did come down the next day (our trains crossed in the night) but Marv didn't take the job. He took a summer job and then went on for his doctorate. So, I joined Eastman Kodak in early July of 1927.

An interesting note as to salary: in September of 1927, two months after I started to work for Kodak, the already great chemist Wallace H. Carothers (Ph.D., 1924, under Roger Adams at the University of Illinois), after three years as an instructor, was paid by Harvard $3,200 a year and du Pont offered him $5,000, where he developed neoprone and nylon. So I was not being underpaid, by reference to the times. Fact is, I'm impressed that I received almost as much as he, and he had a half dozen years of experience.

BOHNING: Before we talk about your experiences at Kodak, could we go back and add. I was curious about your summer job at Cudahy Packing Company. You said you used instrumentation rather than doing the typical analyses you had been used to. What kind of instrumentation did you work with?

OTHMER: As I told you, I don't like analytical chemistry so I've done my best to forget it. Instrumentation then is a long ways away from what we call instrumentation now. So, instead it was pH readings with a potentiometer which I became much more familiar with later because I used it in my doctoral research for quite different purposes; also the interesting Van Slyke apparatus, and the classic Kjeldahl flasks and distillations. These nitrogen determinations and others did not use 1990s instrumentations, but at least they weren't gravimetric.

Also, I did have one or two other chances at the packing plant. Just then there was an article in Industrial and Engineering Chemistry (2), telling of the use of sodium hypochlorite or a ready-made solution of hypochlorite made by passing chlorine into a solution of soda ash. A spray of this solution did an excellent job of killing odors and bacteria. So, I did have interest in making the material and then observing the spraying of the hide cellars with this. Its effect was wonderful. Those are stinking places, the hide cellars, and you have no idea how bad they can smell. The air was freshened up immediately by spraying with hypochlorite. Of course, that material or modifications has long since become standard for sanitizing water in swimming pools, etc.

BOHNING: When you missed the opportunity to go to Carnegie, why did you select Michigan?

OTHMER: I had applied to Michigan and I guess what had happened is that I had been given this $250 assistantship. So, that must have been the reason. I can't remember any other except that I had heard that it had an excellent graduate school of chemical engineering.

BOHNING: When you were finishing up your bachelor's work, did you apply to other places?

OTHMER: Oh, yes. During that summer I had applied to several places, including Carnegie, and none of them had come through except, as I remember, Michigan. That is the only reason I can think of right now, because I knew nobody who had taken chemical engineering at Michigan. As it turned out, Michigan was then and is now one of the top rated schools in chemical engineering.

BOHNING: Are there any other faculty that you interacted with at Michigan besides Badger?

OTHMER: Yes, of course. The entire faculty. I remember so well—top notch professors and engineers—all of them. There was Professor [Eugene H.] Leslie, probably the leading professor of petroleum engineering in the country at that time. He operated, privately off campus, a development laboratory for E. B. Badger and Sons, an early leader in building alcohol plants and later petroleum refineries. (And then, in the mid 1920s, it was only a half dozen years that petroleum was refined in continuous distillation columns. I remember a plant inspection trip in early 1925 to a petroleum refinery in Ohio that had only large still pots and condensers--no columns.)

There was some jealously between Leslie and Badger—and between their respective graduate students. Early on, I had a run in with Leslie (who had just finished what was then the best book on petroleum refining (3)). One of many water lines (only 3/4 inch pipe) ran near the ceiling of the lab of one of his graduate students. It had an unusual type of union, then unknown to me, with an internal gasket rather than a brass dome and seat. The break poured a torrent instead of a trickle when I tried to crack it open.

Probably the most colorful professor was George Granger Brown, who also had come in 1924 to Ann Arbor, he as a professor. I was in his course in thermodynamics, and it was the first time for both of us. He, like dozens of others, said there was no good book of thermodynamics, therefore he would have to write one. He never did! Meanwhile, we were to buy the, just out, massive Taylor treatise on physical chemistry, which did have a short thermodynamics chapter in its two volumes (4). They cost a horrific $25—compared to a usual text at a maximum of $5 in those days. But Brown referred to Taylor once or twice during the course and didn't suggest that he would rewrite the rest of the two volumes!

Ed [Edwin M.] Baker was one of my first professors of chemical engineering. He taught the course in graduate distillation and I had known him for the three years that I was at Michigan. He bumped into me in the hall shortly before I left, and after I had made up my mind as to where I would go to work. He asked, "Othmer, I understand you've got a job and it's not going to be, as Badger would recommend, with evaporators." I said, "No sir, it's not." He said, "Well, where is it?" I said, "Well, I'm going to Kodak's plant to design and handle some stills." I think those were my exact words, one of which he didn't understand. He scratched his head and looked at me, "Othmer, I know of this newly developed product, but how in the hell do they use stills in making Kotex?" Kotex, by the way, had only shortly come on the market at that time, in early 1927.

That reminds me of another item of more interest. In my first year at Ann Arbor, I took Baker's graduate course in distillation. In the class of a dozen or fifteen was Cliff [Clifford C.] Furnas. Cliff was considerably older than I; and he was due to get his doctorate that year and was extremely busy with his doctoral research. He was a brilliant theoretician and had had several years of industrial experience and additional chance to work on chemical engineering theory. Thus, he developed in our class, as part of a problem, a new method for the design of distillation columns. This was elegant compared with the tools that the rest of us had to use. The then standard Lewis theory and method of designing distilling columns was comparatively clumsy, tedious in its calculations, and much less exact.

Furnas developed a system with the assumption of the equality of the molecular latent heats of the components of two liquids being distilled. This follows from Trouton's Rule and he developed from this a much simplified system, compared to that of [Warren K.] Lewis for calculating the number of plates in a distilling column.

This same important design method was being developed at the same time by another Michigan man, Warren Lee McCabe, whom I hadn't met yet. McCabe had received his master's degree from Michigan in 1924 as I received my bachelor's degree from Nebraska. He then left Michigan, went to M.I.T. and in a graduate course there, developed with a classmate, [Ernest W.] Thiele, this exact system that Furnas developed at the same time that he was a student in the graduate class in which I was.

That system of designing distilling columns has now been standardized in all of the textbooks on chemical engineering that have been printed since, under the name of the McCabe-Thiele method of calculation for distilling columns. And it has had hundreds and hundreds of citations in the literature of the last sixty years. Furnas later became professor and head of the department of chemical engineering at Yale, and was there for years working with Barney [Barnett F.] Dodge. Later he went to the State University of New York at Buffalo and there is a building named after him there, the Furnas Building.

When Furnas was here at Poly for some celebration many years after being in the same class, I reminded him that he had developed this widely used system when we were solving the same homework problem, and it was to become one of the great tools for aiding chemical engineering calculations. (It has been used in modified form with many other chemical engineering calculations besides the design of distilling columns.)

This was some thirty years later; but Cliff still felt irritated and frustrated that he had not gone into print with this great system first. But he said, "Don, there was nothing I could do. I was so busy. I was married and had responsibilities that way, but particularly I was so busy finishing my doctoral thesis just at that time that I couldn't possibly have taken the time to write it up, even if Baker, our professor, had encouraged me." Baker did not recognize how important this was, but Furnas years later always felt that it was one of the golden rings that he had missed grabbing when he was on the merry-go-round of his doctoral program.

The same fall (1924) during which Furnas didn't get that great calculation method published under his name, I developed a very much less important theory in Professor Badger's class, which then was presented at a national meeting of the American Institute of Chemical Engineers and titled, "Studies in Evaporator Design—Optimum Cycle for Liquids Which Form Scale" (5). This is number one of my reprints which are bound in several volumes and, to date, number between 360 and 375. The last 100 or so will make Volume 4, when I am able to find time to organize them for binding.

BOHNING: I think we can move on to Kodak then. There are a number of things that you did in the years that you were there. First, you were asked to design distillation processes and equipment and you found insufficient data needed for liquid vapor equilibrium.

OTHMER: Yes. I was surprised when I found what I had been hired to do. As a matter of fact, in accepting the job formally, I wrote a letter and said, "I'm going to be here in Ann Arbor for another six weeks, maybe two months at the University; if you at Eastman can tell me on what I am to be working, I might use the facilities either at the library or of our faculty here to study it. Obviously, I'm not going to be working on what I had specialized on here because no where at Eastman is there any important evaporation problem."

So my new boss to be wrote back and said that the problem to be given to me was how to concentrate acetic acid from its dilute solutions. In its production, this is a solution of six or seven percent concentration which comes either by the fermenting of alcohol to vinegar, or by condensing the vapors which come when wood is destructively distilled to give charcoal. Either has to be concentrated to 99.7 or 99.8% to be used with acetic anhydride to make cellulose acetate, which Kodak was very interested just then in producing.

So, I knew before arriving at Kodak Park the problem which I would have to solve. But, needless to say, busy as I was finishing my dissertation, I didn't carry out the promise that I had made to come up with a method before I turned up for the job! I had much to learn about distilling and other engineering practices, particularly how to do this commercially with tremendous quantities. In making each pound of cellulose acetate, it is precipitated in washing out with water the three or four pounds of acetic acid in which it has been dissolved. By counter-current washing, this gives then 28 to 30% acid. To recover the acid for reuse, the 68 to 70% water must be separated. To do it by the usual distilling is possible; however, it would take impossibly large distilling columns and require much too much heat. So it was necessary to find some other way than the standard distillation.

I was assigned to a major R & D group, Experimental Work Order (EWO) 50-50, for development of cellulose acetate. One of the leaders of the group was Dr. Hans Thacher Clarke, a very great organic chemist who spent half his time operating and expanding the Synthetic Chemicals Department. He had started this ten years before when European supplies were cut off during World War I. By 1927 he was supplying laboratories with some thousands of pure research chemicals on a non-profit basis. He was a brilliant scientist and a wonderful, friendly man who helped me get organized and gave me to start with, a corner of his EWO 50-50 laboratory in a manufacturing building. His main operation, however, was the Synthetic Chemicals Department in Kodak Park West, a kilometer away, but he was a general consultant to many departments.

Dr. Clarke had prepared and refined many chemicals by distillation and had designed and published his clever design of a glass laboratory plate distilling column (6). But he never understood reflux and its necessity. Once he said of this return of products to the head of a distilling column, "What a good chemist has separated (the condensate) let no damn fool (a chemical engineer) run back together"—to the top of the column. So much for reflux!

At about 6'1" height, he was slightly shorter than I; and in a meaningless conversation one day, we discussed difficulty of getting shirts with long enough sleeves—then length of reach. Since mine is long, I took up a bet for a quarter. Back to back, we extended our arms. His span was one and one-half inches longer than mine; and he said as we turned to face each other again, "That reach is what enabled me to keep my college lightweight boxing championship for two years."

Dr. Clarke had been with Kodak many years, but he was invited (about 1930) to a professorship at Columbia University Medical School, and explained his leave taking: "The medical doctors are realizing that there is a lot of organic chemistry going on in the organs of their patients which they do not understand."

Well, as I said, the dehydration of acetic acid was given to me as my job; and the first thing I did was to look through the physical chemistry books and journals on chemical engineering methods. There seemed to be none of the data necessary for the design of distilling columns to separate most liquids from each other. So I developed a system—I called it the "boil-around-system,"—which solved by self-correcting several of the problems in the ordinary boiling of a sample of a liquid to obtain a sample of its vapor.

For example, if you boil a fermented beer at say eight percent alcohol, you just get vapors of about fifty percent alcohol; that is what makes whiskey or brandy or vodka, the usual distilled spirits. But, in doing that, if you start out with a glass full of distiller's beer, the first drop from the condenser comes over at, say, fifty percent. Thus a much higher concentration of alcohol comes out of the solution than is in the solution as distillation proceeds. This depletes the boiling solution which becomes more and more dilute. Therefore, to obtain an exact vapor sample in equilibrium with any given boiling liquid, it is impossible to distill simply and take the condensate as a sample because the composition in the liquid has varied during the distillation.

So the simple system which I devised was to condense the vapors and run the condensate through a small trap back to the boiler. When the thermometer assured that a steady state had been reached, there was an equilibrium reached between the vapors making up the condensate and the liquid in the pot. After the condensate had run through the trap for a long enough time the steady state or equilibrium had become fixed, and the temperature was also constant. Then, the sample was taken from the condensate trap to represent the continuously distilling vapors. A sample or fraction of the liquid that was in the pot was taken at the same time. Oddly enough, this very simple operation had not been done before; and I designed a glass apparatus to do this.

I am fairly good at many types of mechanics, but I never had any substantial glass blowing experience. Dr. Senter at high school had taught us how to bend tubing, but this is not glass blowing. That was merely heating and bending and a very simple operation. So I set up a blowtorch in my laboratory at Kodak Park and learned by experimenting how to blow glass. There was no in-house glass blower, but there was one a few miles away who made special pieces for us. I watched him and I learned a little from him and learned more by practicing. But, he did build this equilibrium still—as it was called—and one of the first papers I published described it, and my determination of the vapor-liquid equilibria of acetic acid and water, also of several other mixtures of liquids in which I was interested (7).

As to the equilibrium still, I continually had ideas for improvements and I would see possible errors in the determination which I wanted to correct. Over a period of years, I developed additional models based on improvements, while I was at Kodak.

Then when I came to Poly, there followed several models in succession, each one intended to reduce one or more of the possible errors in use. I guess the last paper in that line was about twenty years ago. But, dozens of other people have modified the system I had outlined for their special liquid systems or other reasons; and now what we see in the literature are modifications of modifications of modifications of my 1927 "boil around" unit. The genes are there but they are not identified in the generations of the equilibrium still which have been developed since then. And oddly enough, the basic system with some modification of the apparatus is used in many physical chemistry laboratory courses, as an experiment in teaching phase equilibria.

[END OF TAPE, SIDE 2]

BOHNING: I believe there's a picture of your equilibrium still, usually called the "Othmer Still," in an illustration for your Chemical Pioneer address before the American Institute of Chemists (8).

OTHMER: Oh, yes. That is a picture of one of the later models and this is a model of it here in my office. Take a look at it, it is gold plated! One of my doctoral students of years ago made it, had it gold plated, and gave it to me at a party a dozen friends had for me in 1975, I believe.

BOHNING: I'll have to take a picture of that later. It says on the gold-plated label, "The OTHMER STILL—a Cornerstone of Chemical Engineering Science—To Donald F. Othmer, a Beloved Leader in Chemical Engineering, From His Students and Friends."

OTHMER: I used this system and "boil around" still for years in study of many systems of liquids, particularly in the first months after I was with Eastman Kodak. Then I was modifying considerably a distillation system for concentrating acetic acid which had been used to make anhydrous alcohol twenty-five years before. However, the method had to be changed in the case of acetic acid because, of course, its properties are quite different from those of alcohol.

Simply, this system of azeotropic distillation depends on the fact that water and another water-insoluble liquid which is added develops at any temperature a summation of the individual vapor pressures of each of the two, and consequently a substantial reduction of the effective boiling point of water. However, if this other liquid is soluble in acetic acid, it does not exhibit this same additive partial pressure phenomenon. So essentially, what this means is that when there is added to a dilute solution of acetic acid and water some chosen liquid which is insoluble in the water, but is soluble in acetic acid, there is lowered by 10°C to 30°C the boiling point of water, normally 100°C, without lowering as much that of acetic acid which boils alone at 118°C.

Proper choice of the added liquid—usually called the "entrainer"—lowers the boiling point of the water down to 70°C or lower. That new lower boiling temperature of the water and the added liquid compares with the boiling point of acetic acid, 118°C, which give enough effective spread between the two effective boiling points to allow separation by distillation, since now the water comes over over at, say, 70°C.

However, the acetic acid in the lower part of the column is boiling at only a slightly different temperature with some of the added liquid than its usual one of 118°C. This new difference allows the separating of the two liquids in a distillation column, acetic at the bottom, water and added liquid at the top. All condensate goes to a decanter, the water insoluble liquid separates and is refluxed to the column, and the water layer, saturated with the other liquid, is stripped of this in a second small column.

This principle I tried with every liquid I could get which was soluble in acetic acid but only slightly soluble in water, and which would boil with water at a temperature which would allow its easy separation from acetic acid. The first one of course was benzene. Benzene was not known than as the dangerous carcinogen that it is known to be today, so I worked with it and also a dozen other materials. Each liquid I tried with a boiling point between about 70°C and 95°C showed promise. Benzene was not too good but some showed distinct success. And so, Kodak took out domestic and foreign patents—at least twenty five or thirty—on the work that I did in my first half dozen months at Kodak Park. Also, improvements in the processing method were being patented continuously.

Meanwhile, Mr. Perly Wilcox was encouraging me. He was a vice president of Eastman Kodak Company and also the general manger at Kingsport, Tennessee—which is where the cellulose acetate would be made. He realized the necessity of a simple, economical process because Eastman was interested at that time in developing cellulose acetate safety film, and about four to six pounds of water had to be removed from the acetic acid recycling in making one pound of cellulose acetate. The old and then standard film was cellulose nitrate—chemically the same as gun cotton and violently flammable or explosive. It had caused some very bad accidents. In one, an x-ray film storage vault in a hospital in Cleveland had flashed to cause a very bad fire, and X-ray film was cellulose nitrate.

The other danger and a much more frequent danger in using nitrate film was in its use in home movies. Kodak was trying very hard to develop home (16 millimeter) movies in the late 1920s. I myself, for example, played with home movies because the film, camera, and the projector were all available to me as an employee. But when projecting the film in a home, if the projector happened to go wrong, and the sprocket didn't wind, I would just let the film come out in a pile on the floor. I would rewind it later because I didn't want to interrupt my show. Well, that pile of loose film, if of combustible nitrate, obviously could be a very great hazard. If there was a pile of of loose 16 millimeter nitrate film on the floor and somebody dropped a cigarette in it, it would go up!

Kodak wanted to make and sell "safety" film. Cellulose acetate burns of course, but it burns no more rapidly than cardboard, so therefore it is one hundred times less dangerous than nitrate. Cellulose acetate was the only material then known to substitute for nitrate film.

The other thing was that Kodak had its eyes on down the road was getting into cellulose acetate fiber. Its competitor at that time was Celanese Company, which had developed acetate during World War I. I might come back later and tell you a couple of interesting things about the start of Celanese. The production of acetate for use in film, fiber, or later sheets or plastics required the handling of tremendous amounts of acetic acid because for every ton of cellulose acetate that is made, four or five tons of acetic as dilute acid (28% to 35%) must be recovered. This operation is very important.

Mr. Wilcox looked in on me in my laboratory in Rochester at one time in late 1927 and said that he was going to Europe for a couple of months to talk to people there who had developed a process for this dehydration of acetic acid, because Kodak was interested in exploring all possibilities. Meanwhile, I worked hard and developed my azeotropic process with the glass stills that I had devised, later to be expanded into much larger units.

By the time that I saw him next, probably about the first of February in 1928, when he again called on me in my laboratory, he said that he had discussed this matter with a French company who were willing to license their process to Kodak. The down payment would be one million dollars, and their royalty one cent per pound for every pound of acetic acid that was concentrated to glacial, 99.5+% (much more money in 1928 than today!).

Well, I told him that I thought I could concentrate the acetic acid for a total cost of less than one cent per pound, including plant amortization, with the cost of steam and so forth as it was in those days. He agreed to put his chips on the process which I continued then to develop, using as entrainer for the water, ethylene dichloride. This was one of the first petrochemicals to come from Union Carbide's plant in Charleston, West Virginia, and was first available only a year or so before. It boils at about 84°C and thus the excess of that in the lower part of the column after taking the water over the top is separated readily from the acetic acid at 118°C.

It may be interesting to discuss cellulose acetate as a material and how it happened to be commercialized, first actually as a replacement for cellulose nitrate in quite another form. Also because of the flammability of the nitrate, the Celanese Company had a ten-year start on Kodak and had scores of patents on everything related to its manufacture and use, including the acetic acid recovery.

BOHNING: You might as well go into that.

OTHMER: I'll spend a few words on that history. I never worked for Celanese until many, many years later, when I had an interesting consulting assignment. This was in the late 1960s when I visited the plant in Cumberland, Maryland, where they first made cellulose acetate; and some of the original equipment was being considered for retirement.

The acetate process had been developed about 1915 by a Swiss chemist by the name of [Camille] Dreyfus. He and his brother [Henry] came to Washington during the early years of the first World War, just at the time when airplanes were first being used as fighters. Airplane wings then were made of canvas that was stretched over a light wood structure. In order to make them windproof to give lift, the cloth was coated with a lacquer made of cellulose nitrate which was the only then known material suitable and light enough that could be used for this purpose. Of course, the pictures which we all remember of early World War I airplane dogfights always showed an awful cloud of black smoke going downwardly. That was the cellulose nitrate coating burning with the canvas and wood frame of the fuselage and also, of course, the pilot.

So, it was necessary to eliminate the cellulose nitrate and use something less flammable. Hence Dreyfus, a Swiss chemist, had developed a method of making cellulose acetate. The brothers came and talked to our War Department who said, "You must come here to make this immediately. You must make cellulose acetate to coat our planes. We'll do everything possible to expedite."

So, the Dreyfus brothers came here and the first consideration was where to put the plant. The plant must be a long distance from the coast. Why? Because at that time of World War I there had been flights which made our military people think the German Zeppelin dirigibles would be bombing our eastern coast! So the plant must be some hundreds of miles inland. A prime requisite is water in large amounts for any acetate production, not for fiber now, but for lacquer. Large amounts of water are used in washing and other operations. The choice turned out to be Cumberland, Maryland, some hundred or more miles inland, west of Washington. That was where the first acetate plant in America was built, and then, the largest one anywhere.

Celanese then supplied the Allies from that plant with the lacquer. Later, after the war, Celanese turned to acetate fibers, still a major product of acetate.

BOHNING: At Kodak you were working on the stills for dehydrating acetic acid and I recall a statement you made that it was a very large tonnage of copper in one place. I guess the stills became larger and larger.

OTHMER: Yes, that was the case after several years and more stages. But my research and development problem had several angles. Fortunately, they didn't all come at once. The first one was, of course, the theory and the process itself which I was able to develop with little glass stills.

I acquired some fair skill at glass blowing. I wanted to make glass distilling columns with bubble cap plates. I talked to our German glass blower across town. He said, "Dr. Othmer, no. What your sketch shows me is impossible to make." So I said, "O.K., I'll develop some other type."

Mostly what I used were glass columns packed with 1/4 inch glass Rashig rings. I standardized on a two-inch diameter Pyrex glass column in five foot sections. These were drawn down to an inch diameter to fit in to a rubber stopper in the next lower section of the 22-liter flask pot, which was heated electrically by an internal coil 3 inches in diameter. I wound the silver-plated coil so as just to go into the neck of the flask. The column sections were insulated with standard steam pipe insulation and assembled together with rubber connections, either hose or rubber stoppers.

I built fifteen- or twenty-foot heights of these sections. I also at that time developed the glass condensers with two or more inner coils, which have been used throughout the world ever since in laboratories. In those condensers, the biggest ones, we installed large pilot plants for condensing very pure acetic acid. Otherwise it had to be condensed in pure silver condensers (stainless steels, as then known, could not be used). Also, we designed and used glass decanters, automatic level controllers, temperature controllers operated by streams of hot and/or cold water, and other accessories—all described in various articles in the 1928 to 1932 years of Industrial and Engineering Chemistry (9).

However, the glass distilling column I wanted was a more intricate design because it had bubble cap plates and downcomers, the same as a large distilling column. But the glass blower had refused to try to make it. However, I usually took an hour or a little more for lunch and instead of playing cards—as most of the fellows did after eating a quick lunch in the three or four restaurants in Kodak Park—I went back to the laboratory and spent a half an hour or so practicing glass blowing, and became fairly proficient. I worked only with pyrex glass and then laboratories used both pyrex as well as "soft" glass. Nowadays there is only pyrex or other borosilicate glass because it's so superior, although of course, it must be worked with a gas-oxygen flame giving a much higher temperature.

However, I then made one of the bubble caps with the downcomer assembly in a couple of noon hours. I did not seal it into the two inch column—which finally was to have seven or eight plates on a six-inch spacing. Unfortunately, I didn't have any oven to anneal my work, and the single bubble cap cracked. The next time I saw the German glass blower, I showed him what I had made and he said, "Well, this is fine but it has a crack. If you could make it with a crack, I can make it without a crack."

He was encouraged, and he then made this particular distilling column which of course was published in the <u>Industrial and Engineering Chemistry</u> in about 1931 or 1932 (9). Anyway, we made those up to a three inch diameter, each plate having a single bubble cap and downcomer. We made one for a special project to be used under one hundred and fifty pounds pressure, but it was only one inch in diameter.

(Years later at Poly, I used this column with seven plates as a visual demonstration of distillation and rectification. Red bromine and a water-white solvent of a higher boiling point were separated. The bromine refluxed from the condenser with a red vapor; and the descending liquid and rising vapors became less and less red, plate by plate, to give entirely clear liquid in the pot where the solvent was boiling much higher than the bromine at the top.)

We then jumped from those columns to an eighteen-inch copper column for a pilot plant. This was ordered sometime in the winter of 1927-28 from Paul Lummus in Boston. He was then an old man, I thought, compared to a youngster like I was, twenty-three or four. I called on him in his drafting and engineering offices in a loft off of Milk Street in Boston. His company (now the Lummus Company) did build that first column and it worked and did just as we thought it would.

We were using an added liquid, called the entrainer, because it entrained or withdrew water over the top of the column by lowering its effective boiling point without lowering to the same extent the boiling point of acetic acid. That first successful entrainer was ethylene dichloride, and it boiled the water over the top at about 72°C while the anhydrous acetic acid at the bottom of the column boiled at 118°C. Therefore, there was enough spread between the effective boiling points of water at the top, and acetic acid at the bottom, to allow the separation to be accomplished easily.

Then came my four-foot diameter column from Lummus. That had some special attention, and I decided that the still pot would have a forced circulation heater because I expected trouble from the sulfate salt which would be present. Sulfuric acid in a small amount was usually used to catalyze the acetylation of the cotton with the acetic anhydride and acetic acid. The sulfuric was neutralized with alkali and I expected the salt would come down the distilling column and cause trouble in scaling the heat transfer surface of the pot.

So we persuaded the Duriron Company to build the biggest Duriron centrifugal pump that they ever made—and this record may still hold. It had a twelve inch suction and a ten inch discharge to pump the liquid up and through the vertical tubes of the heater for this evaporator or still pot.

However, the unit didn't work just the way I had planned. What I should have foreseen, but didn't, was that the dilute acetic acid (28% to 32%) was losing its water in going down the column, and the sulfate salt became insoluble as the liquid reflux became more concentrated. Acetic acid is an organic solvent, and strong solutions are not good solvents for most inorganic salts.

That means that at some point in the column most of the sulfate salt would precipitate out as a scale. So that is just what happened the first time we operated the four-foot distilling column, of which I had been so proud during its erection. We had built a multi-story annex to the building. I regarded this as a rather expensive project as the column itself was also very expensive. It was made of very heavy copper and aluminum bronze, as were all of the accessories, except the pure silver condenser to condense the acetic acid. And what happened? It only ran a couple of hours until the pressure at the base showed that there was something wrong, and the column was plugged. We had to tear the column all to pieces, which was somewhat of a job, and take out the scale.

The second time we operated, the same thing happened. And then I saw that what was necessary was to evaporate the raw acetic acid liquid (28% to 32%) away from the salt. Then there would be no salts in the column which would precipitate out as the acetic acid became concentrated.

Fortunately, I could take the pot and base heater that I had in my eighteen-inch column. This was in the building, one hundred yards away, and in a couple of days we jerked that out and piped it in, alongside of the four-foot column. This small distilling pot, which had been built with a special heating unit, was thus able to evaporate the water and acetic acid and to leave the salts behind in that evaporator, the same operation as in any salting evaporator.

The vapors that came overhead would not be condensed; but they were passed directly into a middle plate of the distilling column. Thus the pre-evaporation process cost nothing as to heat for that additional separating step, and the heat supplied to give the overhead vapors was simply supplied by two heaters, the normal base heater and the pre-evaporater.

That worked beautifully, and we ran that four-foot column then for some years as part of the pilot plant for recovering substantial amounts of acetic acid from the pilot plant production of cellulose acetate. Meanwhile, we were getting ready and planning for utilizing this same process in Kingsport, Tennessee, where the manufacturing operations were to be conducted. This must have been, by now, sometime late in 1928. The planning for the plant in Kingsport was done in Rochester and I worked with the engineering department and designed these columns, which would all be built in Kingsport.

Everything that Kodak did always was very conservative with the most rigorous attention to details of every kind, and the use of very heavy equipment with multiple corrosion allowances.

So, the first production units, as I remember, were six feet in diameter and we planned and built four or five of them in Kingsport in specially built shops. We designed the building in Rochester and the equipment and all that was then used and could go onstream within a year or so. The distilling towers were made of very heavy copper. All of the accessories, like bubble caps, were made of bronze. I won't go into the details of the tons and tons of copper that went into these first units; there were also many tons of aluminum bronze castings. Also, many tons of bolts were used because every single plate was individually flanged, which is rarely done. The construction was three-eighths- or five-sixteenths-inch thick copper. Everything was six to ten times as heavy in copper weight as would be used in an alcohol column. This sort of over-design was used throughout. The first condensers, like the one for our four-foot column, were made of silver and, of course, I designed them to minimize the weight of silver. Even so, the one for the four-foot column had five thousand ounces of silver, as I remember.

[END OF TAPE, SIDE 3]

BOHNING: What was the largest column you used?

OTHMER: Those first ones were six foot in diameter, but then we went to eight-foot and then ten-foot—always the same very heavyweight specifications. We built these first in one building and then in a second building. Unlike modern columns, and since these were small but expensive because of the heavy copper construction as compared to columns of steel which are used for petroleum, we followed the old practice of alcohol distilleries. We built them in multistory buildings rather than building them out in the open. Actually, it may be remembered that it was only about a dozen years before that petroleum was distilled in columns, before, always before, simply from pots to condensers.

There were also a few extra pieces of equipment that could well have been put outside, but were not. We built a beautiful building, probably seventy feet high, for these first four columns and then space for two additional ones, as I remember, eight-foot columns. Then later, ten-foot columns were built in a second building.

I have often thought that there was and never has been any other place in industry, no single place in the world probably, where there would be as many tons of copper as in these tremendously heavy distillation units which may still be operating. They should be, for they were made so heavy, and copper is not corroded by acetic acid if air is kept out and there are no impurities. Air is absent if the still can be kept operating continuously and we used every precaution to do this.

Of course, there was a first program and then a second and later programs. Once Eastman got into cellulose acetate for their safety film, they meanwhile had been developing experimentally a program to make fiber. So fibers came. Then came sheeting to compete with cellophane, but has disadvantages as to static and cost. This was not my business. But always there was expansion of acetate production and with it the concomitant acid recovery. The one thing to remember was that suitable stainless steel was not yet available and could not be fabricated dependably. Also, aluminum was comparatively much more expensive then than now, also quite unsuitable for most uses except tankage.

One little incident, or maybe a couple. Kodak had developed a great skill in fabricating—and were among the first to weld—copper. They also did various other fabrications. I was concerned with very big heat exchangers that were being built there in Kodak Park for heat exchange of the gas-vapor mixture which came off the coating machines that made the film, in condensing out the solvents. Those were very large and were made in the shops in Rochester. I don't think there was anything novel except their enormous size. Instead of measuring the heat transfer surface in square feet, those actually had <u>acres</u> of fluid films on the two sides of heat transfer surfaces.

But, getting back, the copper work for our stills was all done in the shops in Kingsport. For clamping the flanges of the plates together, one-inch studs were used, and there were dozens of tons of these, long enough to hold the very heavy backing flanges, with gaskets on either side of the plate facing the turned over flanges of the plate section itself. But the conservatism of an Eastman design always impressed me. Of course, Kodak has been in business and is in business to stay a long time; and if those columns are still running, that heavy construction was worthwhile.

The second one of these distilling buildings, I remember so well. I had changed and modified the process a little and we didn't have quite the condenser arrangement at the top of the columns. The first brick building, seventy feet or so high, had on the roof, probably for one-third of its area, a glass penthouse, built with windows that could be tilted as usual for ventilation. And, in the new building, which was built adjacent, we didn't need that superstructure or "doghouse," as we called it, on the roof, in spacing the condensers.

Mr. James White was the operating manager of the plant. He had come into the business from the wood distilling days up in Michigan and was a tough but fair man to work with. He looked over our blueprints of the new building. We had drawn the other building alongside the perspective to see what it would look like taken together. He said, "Hey boys, you don't have a doghouse on the roof." "No, we changed the process a little and don't need it. We figured we would save some thousands of dollars by not putting it on. Is that important? Maybe five percent of the cost". "Well," he said, "Put it on for symmetry. You know even though they may not be necessary, God still puts tits on boars." Well, that was the way it was built.

That doghouse reminds of another incident that happened when I wasn't there, as I spent about half of my time in Kingsport and the other half in Rochester. But, there was an upset, something went wrong. I forget now what the incident was, but large amounts of boiling acid escaped. The still house was maybe seventy feet high and fifty feet by one hundred or so feet long. This was a tower which gave quite a draft or stack action to the air inside—now acid-filled—because of the heat lost by distilling towers. So, under normal conditions, these windows on the doghouse were tilted open and there was quite an updraft.

Something went wrong one time and a large amount of boiling acetic acid ran out the still on the ground floor. The stack action carried upwards these excruciating, irritating vapors of almost 100% acetic acid—an awful affront to ones eyes, nose, and throat. The operators tried to escape upwards and fled to the roof by the stairways, for there was no elevator. A couple of the operators fell down exhausted on the roof, under the open windows with hot air and acid vapors pouring over them. This one chap was lying there prone, crying out in pain, and another one came up and saw what was happening. He dropped the window down so that the fumes didn't come out over the one lying there, and the wind took them away. The prone chap rolled over, his eyes still closed from their smarting, and said, "Thanks, God."

As an indication of the preciseness, of the nicety of many things at Eastman, we had green lawns all around this growing group of buildings across the highway from the old constructions of the original charcoal and wood chemicals plant, which were typical of that type of operation. (The old wood distilling plant had originally started out in the early 1920s to supply the acetone and the methanol used as solvents for the film base.)

We had another bad spill on the ground floor of the still house. I happened to be in Kingsport, although this happened about two in the morning and I wasn't on the job. A three inch stainless steel pipe was carrying 28% raw acetic acid. This was in the days when we were learning how to weld stainless steel. Stainless steel now is adjusted in its metal content so that after being welded it cools without the crystal formation which makes it brittle. But, this was yet to be learned. Stainless steel, at that time, should have been annealed but this could not be done with pipes that are welded in place in the plant.

This three-inch pipe broke and poured out a thousand gallons or more of the 28% acid. This ran out the twelve-foot wide doorway along with all of the wash-down water outside. Fortunately, nobody was injured and repairs were made quickly with pipe flanges. The next morning when I arrived, men were already digging up the sod and within two days there was new sod, rolled down to eliminate any sign of the accident.

This was in a factory building and this was a beautiful complex of new buildings to make cellulose acetate. Everything had to be spic and span. However, this care was nothing like that in rooms in Kodak Park for making Kodak film. There, everything must be super dust-free and clean. However, the acetate plant was very nice. On the other side of the road was the wood distillation plant, which was tar and dirt, cinders, smoke, and charcoal. An entirely different thing, but that illustrates so well the different types of operations. There are many different standards or degrees of cleanliness, of tidiness, of housekeeping and so on, depending on the operation. Obviously, a cement plant can't be as dust-free as a packing house although the smells are not as bad.

Referring to film making and its packaging—these rooms were among the first factory rooms anywhere to be air conditioned, very nice, but very dark, only a very dim red light. A new employee—barely passed by the personnel department—resented this dimness on her first day, so the next day she brought a flashlight!

I might divert. This is partly a history of chemistry and chemical industry. After the first World War, the expansion of Kodak operations was considerable and in the making of Kodak film, the old nitrate film and then the acetate film, solvents are required. The principal solvents were wood alcohol, i.e., methanol, and acetone. And those two things then came entirely from the destructive distillation of wood. When wood is carbonized by baking in an oven, vapors come off with the smoke.

When this vaporous mixture is passed through a condenser, pyroligneous acid is obtained, mainly water with smaller amounts of acetic acid (6% to 8%), wood alcohol, some acetone, and much tar and tar-forming materials. (Chemists have identified about one hundred and eighty chemical materials in this condensate).

The acetic acid was converted to calcium acetate, dried and sold. Other companies "sprung" the acetic acid with sulfuric acid or heated the calcium acetate to decompose and give acetone. Thus calcium acetate during World War I had a great market because acetone was necessary to dissolve cellulose nitrate for use in heavy ammunition.

During World War I, fifteen or more small wood chemical plants sprung up to supply calcium acetates, thence acetone, in both New York and Pennsylvania near their border, also many in other parts of the country, including Kingsport! However, synthetic methanol and acetic acid were being made in the late 1920s and also there was the loss of much of the market for charcoal, the fuel of "cold water flats" in New York and other big cities. Thus the wood chemical business was in a bad way to enter the Depression starting in 1929.

However, back in 1920, which was just after the height of the boom business of World War I, Mr. Eastman and his directors decided they had to have their own source of supply of wood alcohol. Some of the wood distilleries which have sprung up to make these two materials necessary to produce smokeless powder or cordite were available in southern New York State and northern Pennsylvania.

The man delegated by Mr. Eastman to investigate this was one I mentioned before, Perly Wilcox, who looked over the entire country. One plant I will mention later, was just one hundred miles south of Rochester in northern Pennsylvania—the Gray Chemical Company. It had many advantages—much wood and water, natural gas and a proximity to headquarters in Rochester. This last, however, according to gossip, was not to the liking of Wilcox, who didn't want to be so close to Rochester. He wanted to set up his own empire not so close to home and this may have turned out better also for Kodak. So the place he decided to settle was five times as far away—much more by rail or mail. Mr. Eastman went down with him and they negotiated to buy this ex-World War I plant in Kingsport, Tennessee, which was almost three times the size of the one in northern Pennsylvania. It had, of course, much lower labor and wood cost. When Kodak took over, plant labor worked twelve hour shifts, eighteen hours when shifts changed. Christmas and July fourth were half holidays.

BOHNING: I live in northern Pennsylvania. Where was the Gray Chemical Company in northern Pennsylvania?

OTHMER: It was in a little town called Roulette, which is on Route 6. It is eight miles west of Coudersport, and probably thirty-five miles east of Bradford. Well I could spend hours telling you about that, but I'll come to it gradually. So Wilcox rebuilt this plant in Kingsport and built a new system to carbonize wood to charcoal. This turned out to be a failure so they went back to the old oven system.

Cellulose acetate came into the picture. It may be that Eastman first planned to make it in the early 1920s, and then Kodak actually did get into the business by 1930. One of the important steps, as I said, was the recovery of the acetic acid.

In the history of the chemical industry in the late 1920s, here is an interesting commentary. Kodak needed acetic acid for very large pilot plant operations in Rochester, for the making of cellulose acetate. It is used in a very pure form, 99.5%. This is called glacial because it freezes just below room temperature—16°C to 16.7°C for glacial. Incidentially, it is analyzed readily by determining the freezing point. This goes down noticeably for every fraction of a percent of water. In a test tube of the sample is placed a precise thermometer, the test tube is placed in a beaker of ice and water and stirred. As the acid is cooled it suddenly freezes at a temperature which indicates the concentration, and this heat of freezing is removed by the ice bath.

Kodak long had sold acetic acid for photographic developing use, to acidify some photo finishing baths. That was made in the 1920s in a wood distillery at Marquette in northern Michigan's upper peninsula. This was shipped in carboys of glass. Aluminum was then coming in slowly for shipping glacial acetic acid in drums and tank cars but aluminum wasn't used until the early 1930s for that purpose. So, glass carboys of acetic acid, hundreds of them in freight cars, came to Kodak Park in the summertime so that they wouldn't freeze. A very large number was stored, packed in their wooden crates with excelsior around them.

Kodak was buying acetic acid from Marquette, Michigan—a distance maybe of 1,000 miles or so by rail—to bring to Rochester. In the large pilot plant for cellulose acetate, 28 - 30% acid was being made by the ton. But, when I came to Kodak in 1927, there was no means to recover this 28% acid. This was shipped to Dow Chemical in Midland, Michigan, which was another 600 or so miles by rail. There, Dow converted it to acetic anhydride and shipped it back to Rochester, where the acetic anhydride was used to make cellulose acetate.

Meanwhile, Kingsport, down there in Tennessee, probably 800 or 900 miles by rail, south of Rochester, went merrily on its way turning out daily some ten tons of acetic acid in the pyroligneous acid. This was neutralized to calcium acetate, dried and railroaded to Everett, Massachusetts, just outside of Boston. Here the New England Chemical Company, which later became part of Monsanto, in turn was making from this salt acetic acid of 28% or 56% strength, for sale to the local textile trade.

One can see why in 1927—when Kodak hired me—they had this problem of, "How are we going to cut all of these freight bills?" Of course, what ultimately happened was that everything was done in Kingsport. Anhydride production was introduced, as well as other steps (including the recovery of acetic acid and so on). Also, Kingsport was close to the supply of cotton linters, then used as the source of cellulose.

BOHNING: That's fascinating.

OTHMER: Oh, let me say one other thing. We were talking about my education. I had occasion to write to Professor Badger in the fall of 1927. He had written me about something and asked me how I liked my job. I told him I liked it very much. There were so many new things to think about and I said, "I learned much more in these three months here than I did in the three years that I was working with you." He wrote back, "Well, damn it all, I expected you would. If I hadn't trained you so well, you wouldn't have." (This is the obverse of the story of the professor who compliments a student by saying, "You are learning this faster than I did at your age." The student replies, "Yes, but you didn't have as good a professor!")

BOHNING: The other thing that I have here that you did at Kodak was extractive distillation, which I guess is what you've been talking about. I have a date here of 1929—separating acetone and methanol.

OTHMER: Oh, yes. I was working supposedly 100% on acetic acid, but sometimes I was called in by some other departments for help. The Kodak distilling department was huge, with tremendous distilling equipment, so large because of the great amount of solvents, also because the operations were extremely inefficient. The man who ran it had been hired by Mr. Eastman in about 1899. By now, after doing this job for thirty years, he was aging, but perfectly satisfied bossing a crew of men who knew how to turn, or to pack, a valve, but nothing about the processing they were controlling. In all these years he didn't even understand what vapor pressure was, much less anything about the theory or practice of separating liquids by distillation and/or rectification.

So from time to time, I was called in. One time it was to to develop a system by using my azeotropic technique that I was using successfully with acetic acid. This was for separating vast amounts of butanol from water. Butanol, that is the C_4 alcohol, was used to dry the cellulose nitrate made for film.

First, the mixed acid is washed off the crumbs of cellulose nitrate after cellulose is nitrated, then the water is centrifuged off. In order now to dry the nitrate, it obviously can't be put it in a toaster to eliminate the water. So it is washed with butanol, which dissolves the water and the butanol is dried for reuse by distillation.

Anyway, there were very large amounts of this butanol saturated with water, which then was distilled to eliminate the water. The simple thing about the azeotropic distillation technique that I had developed was that with a liquid like butanol, with only partial solubility for water, the two boil together at a temperature lower than either would when alone. When distilled over the head of a column at the low boiling point, the vapor mixture condenses.

Then, since water and the butanol are only partly soluble, the two liquid layers separate easily; each liquid layer is, of course, saturated with the other. The water layer goes off to a second column and the butanol layer goes back to pick up more water and repeat the process. The dry butanol drains out the first column, the reverse procedure brings the butanol, which was dissolved in vapors with the water to the top of the second column. These liquids separate in the same decanter, and water flows out the bottom of the second column free of butanol. In the case of the butanol, this had never been done. This was an unknown technique with butanol, as with acetic acid, which was not so simple in that a second liquid, the entrainer, must be added.

Previously the wet butanol was given an expensive simple distillation, then allowed to stand and settle, sometimes for weeks, because the minute droplets of water condensed from such a simple distillation settle very slowly. There was a very large tankage of butanol; and even so, the butanol still had a minimum about 10% water held in it. The continuous process used very much less heat than had been used for the simple distillation and released tens of thousands of gallons of butanol from this storage-settling process.

Another separation, however, that I did for the Distilling Department was the separation of acetone and methanol. Acetone and methanol form a constant boiling mixture which is impossible to separate in a usual distillation process. The two liquids are both completely soluble in water. However, methanol is much more like water and thus has much more affinity for water than does acetone.

Hence, if water is poured into the top of an efficient distilling column, which is distilling the two, it is possible to allow the acetone to go over the top, and to dissolve the methanol in the higher boiling water and take it out the bottom.

[END OF TAPE, SIDE 4]

OTHMER: This works, but better if, instead of water, calcium chloride brine is fed into the top of the column. Calcium chloride, as everybody knows, has a very great affinity for water and greatly reduces its vapor pressure, considerably. It has similar but much lesser effect with methanol, not with acetone. Hence in the distilling column, acetone is distilled very easily from methanol by running a stream of calcium chloride brine as a sort of reflux down the column. This takes all of the methanol down in the pot. This can be done in a batch way and that's the way we did it at first. When I did this first in my laboratory two-inch column, the product acetone was analyzed by the Department of Synthetic Chemicals, who said it was at least as pure as acetone they synthesized and sold for dollars per gram.

We then expanded this to some thousands of gallons of the acetone-methanol mixture. This was charged into the pot and—with the acetone discharged overhead—the calcium chloride brine with the methanol dissolved remained in the pot with the methanol vapor pressure greatly lowered, and that of the acetone relatively unaffected. So the acetone was distilled entirely overhead.

At that point additional heat raised the temperature of the pot and the methanol distilled away from the calcium chloride brine, much before the water. This was later made standard practice in a continuous distillation using two distilling columns.

BOHNING: Where did the acetone methanol mixture come from?

OTHMER: This came in many thousands of gallons per day from the Roll Coating Department, where the film was made on tremendous rolls—about twelve feet in diameter and three or four feet wide. These had a surface which was spotless. Each square inch was burnished by hand under a lens, and the outer surface was silver-plated. It was so smooth that as it rotated it was coated with a thin lacquer of the "dope," as its called. This film formed as the roll dropped into a bottom reservoir of the cellulose solution in an acetone-methanol mixture. A stream of air would evaporate off the solvent so that the film could be separated after completing most of the rotation. Different mixtures of solvents and solutes gave different thicknesses and other properties to the film.

I never had any expertise in this art and science of roll coating the film base and then the emulsion for photography. The mixture of methanol and acetone which condensed out in the solvent recovery was distilled and a part would end up as this constant boiling mixture, which is about 90% acetone. It became an embarrassment as to how it should be disposed of.

This exhaustive distillation was effective. I don't know whether it is still used or not. Other materials may be used instead of the calcium chloride brine as the extracting solvent. These are certainly no disclosure of secrets of Kodak's practice because I haven't been in Kodak Park in over fifty years and I haven't been in Kingsport in over forty years.

The system of handling solvents as well as many other operations in Kodak Park in the 1920s had been developed by Mr. Eastman, and much was retained that long, as if from a divine act. The tradition was that there was one set of formulations for different films, particularly for the coating of films with specific emulsion coatings. This, of course, was very sensitive knowledge for different types of film and papers. But the system of putting these things together was extremely—well, the term I use is "Boy Scoutish"—so overly careful so as to be unreasonable. So one man would have the formulation of what was used to make some coating for some purpose on some film or some paper. Now, as to the emulsion coating, suppose the emulsion to be used had ten ingredients. Names of chemicals were not used; they went by numbers instead. I don't remember the numbers. Butanol, for example, may have been 10, or it may have been 8. But each material or mixture had different numbers. The men who handled these were entirely uneducated in chemistry and they talked and specified materials only by numbers.

So, if ten different ingredients were wanted in an emulsion, their assembly was complicated. The 1920s were before stainless steel. For containers, monel metal cans were used, probably between about twenty and forty gallons. A truck would be given an order to take a specific can first to building number 8, for example, and then to building number 45, and so on, maybe to get at each place one or more of the ten chemicals that were to make up a particular batch. The truck might take the can to five or six different buildings, collecting in each numbered can the numbered chemicals, like going to a half dozen markets to buy the numbered ingredients for each different stew in its numbered can. At each stop it was prearranged that the order for each of these number-coded cans would receive so many gallons or pounds of this numbered material and so many gallons of that numbered material. Then the cans would be put on either one truck or several trucks. There might be five different trucks going to five different buildings for a single order. Each of these cans was then brought to one of several central spots and with the labels removed, double blind. At that point the materials would be mixed. This secrecy of formulations had been perpetrated, at least up until the time I was there.

One of the side jobs that I did was to develop an evaporator for processing silver nitrate. At that time, about three tons a day of silver was being dissolved in nitric acid on the top floor of a six or seven story building housing their operations. This was the Silver Nitrate Department. Right in the center of Kodak Park, this could have utmost security. Of course, there was the high value of silver on which the entire photographic industry was based. It was dissolved by nitric acid and flowed down to a lower level as a solution of silver nitrate and went into big, standard porcelain evaporating dishes of about twenty-two inches in diameter. There were hundreds of these evaporating dishes placed in hundreds of holes of the right diameter in steam tables, just like steam tables in a restaurant. The holes were large enough so that three-quarters of the bottom of the dish would be exposed to steam, and a minimum of steam would escape. However, this very large room was filled with steam which did escape.

Operators were continually transferring dishes from place to place and they were entirely clothed with black clothes, caps, and masks with goggles. They wore gauntlet gloves and black shoes with heavy wooden soles. The very large room was poorly lighted because of the steam, and the entirely black-covered men looked like devils in a smoky inferno. It was a hot and uncomfortable place to work, in a steamy atmosphere and every bit of the body covered to protect from the liquid. All clothes, shoes, gloves, etc. were ultimately burned to recover the silver, all floor drains were also processed. Yearly loss of the department was a very small fraction of percentage of silver charged.

As water and any excess nitric acid evaporated, these vapors and the steam escaping from the steam table went out ventilators and crystals formed in the dishes. There was a peculiar system of washing. The mother liquor was drained, reevaporated, and subsequently less pure crystals washed and rewashed. Ultimately, recrystallizing gave almost entirely pure silver nitrate. Of course, the silver that came in was pure as could be made metallurgically. It may have been four or five nine silver, that is, 99.999% silver.

Anyway, these solutions, as they came along the steps of crystallization and washing, would become bluer and bluer because the solution contained more and more copper as the principal impurity. Then those were all worked up and the process repeated almost ad infinitum.

In the evaporator laboratory in which I had worked in Ann Arbor under Badger, we had—and I used once—an all glass evaporator. The glass evaporator was made of tubes of pyrex about six or eight feet long. They went through tube sheets with packing glands on either end of a steam chest and steam was on the outside of the tubes. The tubes were connected on the outside with "U" bends. The liquid was circulated through the pyrex tubes, which were about one and one-half inch in diameter.

Badger had made the first orange juice concentrate. This was in the early 1920s. He was technically successful but the whole project was more than making a good concentrate. There was no market developed, because then there were no refrigerators that we now have in every home. Everybody had an icebox, and refrigerators came about 1928-1932. So Badger's concentrate, good as it may have been, never got off the ground. But that is the sort of system where trace impurities would give a bad taste to the orange juice and only the pyrex tubes could be used. This was long before stainless steel.

So I had a steam chest made with the packing glands for six or eight tubes. Our glass blower then came and welded the "U" turns into place and we had an entirely tight job, with the concentrated solution separating from the vapors in a specially fitted 22-liter flask complete with trap.

I was working there one time, of course, with a mask and goggles on. Nobody could go into this room where the silver nitrate was being processed without a mask, because of the danger of nitrate in your eyes. Somebody dropped a tool and broke one of the "U" bends. Silver nitrate solution was sprayed all over me. We all wore clothes which were burned to recover the silver that was in them. Management banned glass tubes thereafter, and I agreed!

At a later time I rebuilt the evaporator and used tantalum lining for the tubes and flask chamber. This worked well. Some years later, after I left Kodak, the patent of a later Kodak engineer was issued. He patented a very clever modification of this evaporator, which I had never thought of in my use of corrosion proof materials—pyrex glass and tantalum.

Why should the tubes be made of very expensive metals that, however, are slightly corrodible by the very slight amount of nitric acid which may be present? Why not simply use silver tubes, and account for any silver corroded off in the product? So extra-heavy, very pure, silver tubes were installed in the steam chest and used for a year or so. The silver lost from the tubes went into solution. It would be a simple matter to replace tubes when their weight or inside diameter indicated they were getting thin. The tubes could then go to the dissolving tanks along with the virgin bullion.

The reason I got off into this interesting episode in Kodak technology was because George Eastman, now a hundred years ago when he first made silver nitrate, developed a standard for crystal size. I was told that he weighed out an amount—and this amount I don't remember. Let's say he would take one pound of crystals in a glass funnel of a certain size. He would then pour, say, one quart of water at a particular temperature. When the last of the water had gone through the crystals, the last of the silver nitrate should have dissolved, and the rate of this dissolution would depend on the size of the crystals giving a particular amount of surface for dissolution. When I was there, probably thirty years after he developed this standard, it was still being used.

Mr. Eastman committed suicide, I think in November of 1927. I had been introduced to him at some reception. But I think I only saw him that once. He felt he was old—not as old as I am now. But, he had done everything in life that he felt could be done. He enjoyed life and was incurably and painfully sick. So he felt he had done enough, and shot himself.

It is fun to reminiscence about some of these George Eastman traditions, which were thirty years old when I learned them, and now, almost sixty years later. Kodak is probably using some of the techniques which he developed in the wide range of technology which he had to work with in his early years. Mostly, the operations have taken different forms then when I was at Kodak—and it would be interesting to see what forms have survived. I have lost my friends of that time. They have died or disappeared—and all long since retired. And, they probably wouldn't want to discuss such old trade practices and no one probably knows a wide range of those used now—as then.

Let me tell you one other incident. You asked about extractive distillation and I mentioned that acetone could distill overhead while draining methyl alcohol out the bottom of the distilling column in solution with a calcium chloride brine.

In the fall of 1927, I was looking for a better way than my azeotropic distillation to recover acetic acid. I thought glacial acid could be produced with less heat cost after using a preliminary liquid-liquid extraction. The ethylene chloride system made everyone happy and it was so much cheaper than any purchasing of patent rights to European processes that there was never any criticism; or if so, I never heard any.

However, I wanted to improve it and I could see that I could do that if I inserted an extraction process wherein acetic acid would be extracted away from water by the same solvent used as entrainer. I didn't know anything about liquid-liquid extractions. We had talked about it at Michigan without any examples. I suggested it to Ed Farrow, my boss, and also to Mr. Haste, who was vice president and general manager of the Kodak Park works. They were all chemical engineers—and they and others had all come from MIT. I said that I would like to develop a simple extraction in the laboratory. I merely took a lot of separatory funnels and put them in a wooden rack with holes. Then I would shake the funnels, then separate layers in each and countercurrently pass the lower layer to the left one and pass the upper layer to the right. Thus I could obtain a six or eight stage extraction. It took a lot of shaking. I never did enough of this to make it practical, or to develop an agitation for all the stages, although several have been described since in the literature.

But I wanted to do something about large scale extraction.

So Ed Farrow, the boss of the Chemical Plant Department, said, "Well, Don, why don't you go down to that fountainhead of all chemical engineering learning, MIT, and talk to that great wizard there whom I studied under, W. K. Lewis." And I said, "Gee, that's a good idea."

So, I got in my car and drove East to Cambridge, Mass. On my way East I stopped in Troy, New York, and saw a liquid-liquid extractor, the first commercial one that I had seen. They were using this there in 1927—a long time ago. Benzene was used to extract cresols and phenols from gas-house liquors—the waste liquors coming in making city gas from coal distillation or coke making. The liquors couldn't be allowed to go into the Hudson River without this clean-up. And I don't remember now how long they had been doing this, since they had developed this system of extracting the phenols and cresols from waste water. It was very interesting. And I said, "Well, I ought to be able to do the same sort of thing if I get the solvents and equipment." Their equipment was extremely crude.

On this trip to Cambridge I carried a letter to the president of MIT from Mr. Haste, the vice-president of Eastman and the general manager of Kodak Park Works. He knew I was coming. He had a tremendous office. It must have been twenty-five feet by thirty feet, with a magnificent oriental rug on the floor. And over in the corner he was sitting—a little man behind a big desk. I came in a door diagonally located from his desk, and as I saw him the thought occurred to me, "Gee, I can see why this long walk. It is to impress recalcitrant students when called up on the carpet. They are made to walk this long diagonal, being properly impressed by Prexy, before they get to him to get raked over the coals!"

Well, as I walked across I was looking at the man, but out of the corner of my eyes there were portraits around, and one big one near his desk caught my eye. "Gee, that's Mr. Eastman." Suddenly, I realized—"Well, here I am coming representing the man who made MIT possible by his big gift only seven or eight years before!"—which gave it the critical mass which has allowed it to develop into what it is today!" He knew I was coming and was most cordial. He said, "Yes, indeed. The man you want to see is Dr. Lewis."

So he called Professor Lewis who came over and took me in tow, and I spent the rest of the day with him. He was just as dogmatic and positive as I had been told he was in class. "Oh, extraction, Othmer, it's very simple—just like distilling. You have two countercurrent streams of fluid—as in a distilling column—a bubblecap column. If you don't have an extractor you can make one." Lewis was very much like Badger in his always so definite expressions. "So you feed your one liquid, the denser one in at the top, the less dense one in at the bottom and they work their respective ways to the other ends. If you don't have the bubblecap column, just take long strips of channel iron and put them across the plate and put pipes up as risers underneath to let the lighter liquid up, and other pipes as downcomers for the heavier liquid." I said, "Yes, sir." Anyway, I thanked him, got back in my car and drove back to Rochester.

Of course, he didn't know even as much about extraction as I did because it was something that most chemical engineers had not yet worked with. I had already combed the literature as to what there was, and there were practically no reports. Some clumsy systems were in the French patents; however, extraction does not have the driving energy of vapor expansion—from high pressure at bottom of a distilling tower to low pressure at top to cause the bubble interaction on the plates of a distilling column.

Of many interesting problems and incidents in Rochester and Kingsport, one other is regarding a chemical whose synthesis just now (mid-1986) is growing in production and use probably faster than almost any other chemical. This is methyl tertiary-butyl ether, MTBE, an octane booster for gasoline, as lead is being phased out.

About 1930, Philips Petroleum had completed a laboratory program in the synthesis of various petrochemicals using waste olefins as feedstocks. This was at the start of the Depression, and no markets could be developed. However, Philips had a few barrels of mixtures of products left from pilot plant work, and it seemed to be a shame to discard them after the considerable effort in their making. This material was offered to Kodak and Dr. Hans Thacher Clarke for his Synthetic Chemicals Department. Dr. Clarke accepted and asked me if I would use my eighteen-inch column and still to separate what could be refined—particularly methyl tertiary-butyl ether, MTBE, a large part of the mixture.

I had a very fine, capable, and loyal assistant, Frank Vetter, a long time Kodak employee. He was from Alsace-Lorraine and so fluent in French and German that he translated everything I was interested in which was written in these languages. Thus, I readily forgot what little I had to learn to pass the two language reading exams everyone had to take for a doctorate in the 1920s.

Frank was running the still and I was busy at my desk nearby on the third floor which had the still's controls and was below the condenser. Then I answered the phone saying that someone from the Research Laboratories was bringing in a master's degree physical chemist who was a candidate for a position. "Would I visit with him?"

A bit later something went wrong with the still, which by now was running at the boiling point of pure MTBE, and one of the few "upsets" or mis-operations we ever had with that unit. The column "primed" and at least five gallons of MTBE overflowed on the floor outside the curb around the hole through which the column rose.

Just as this MTBE was cascading down all around like Niagara Falls, in the door walked the chap from the Research Laboratory and the new master of chemistry. As Frank and I got the operation back to normal and hosed the overflow down the drain, our new M.Sc. sat down in my chair at the desk with his head in his hands and almost wept at the sight, and the smell which he recognized. When we could relax, he told us that a year before he personally had bought for $40 from Kodak's Department of Synthetic Chemicals about a cupful of MTBE, synthesized in the laboratory. And $40 was a lot of money to a college student in the depths of the Depression. He spent a year determining the physical properties, etc., of MTBE for his master's thesis, always having to refine tediously by a micro-distillation column to use it again. He finally had given what was left, about a third of his original, carefully hoarded material to a colleague when he received his degree. And now he comes into a distillation laboratory at Kodak and sees us washing down the floors with precious MTBE!

There was another amusing footnote to chemical history from my distillation laboratory, which was in a corner of the building of the Powders and Solutions Department where all sorts of odd materials were made, packed and shipped out to the photographic trade. One of these materials we called dragon's blood—I've forgotten the trade name. It was a plate resist used in some photo-lithographic process, a brilliant orange-red powder made from some gum exuded from a South Pacific plant. Sales and production were expanding greatly, and a new ball mill was installed to grind it with standardizing materials. It was just next to my lab and office, and the other side of a very thin partition and door. The impalpably fine orange-red powder sifted into everything, my books, instruments, and clothes. It was a real nuisance. Sales continued to expand, far beyond the world's photographic needs, and much going to the Orient, especially Japan. Kodak sales people there discovered finally that Buddist monks found it a most convenient and standardized form of what had long been used in a crude form as incense in their ceremonies. I don't know the photo-lithographic business, but with the many years and World War II since then, I feel sure that a synthetic resin must now be doing the job of "Dragon's Blood."

BOHNING: You left Kodak in 1931. What were the circumstances that led to that?

OTHMER: I was frustrated. I had filed, by signing applications, for probably forty or so patents in this country and abroad. Always I was given ten dollars, which is more than some companies gave as a reward in those days, where the Assignment would read, "For one dollar in hand, receipt of which is hereby acknowledged, etc." Most companies now are much more generous.

Anyway, I am somewhat motivated and a little bit of an entrepreneur, and I thought to myself, "Here are millions and millions of dollars going into these plants with processes that I invented and designed; and millions more dollars were being saved in the processes." So I was thinking about getting out to be on my own.

I was in Kingsport one time with Perly Wilcox, who later became president of Eastman Kodak—at that time he was vice president and manager of Tennessee Eastman, which was a subsidiary. I was in Kingsport much of the time in the years 1927-1930. We were chatting on a walk around the distilling building late in 1930, and he said, "Don, you know we're not going to have all of these dollars for these toys we have been giving you to play with." I said, "You mean my pilot plant and laboratory in Rochester?" "Yes," he said. "Now we have almost accomplished our present program." I knew that in Kingsport we had come to some production plateau. "Therefore, we don't see the need for more development work and all of these expensive toys you've been having and playing with."

That was the term he used and it irritated me a bit because they had been quite useful toys to his program and its economy. He, of course, was only joking because he very well satisfied with my work. He said, "With regards to you, we would like you to get into management. We would like you to be the head of our distilling department in Rochester."

Well, the distilling department was quite a department, and I think it literally had millions of gallons of solvents going through there every week. And there were so many problems because, as I have said, there was absolutely no theoretical or technical skill at that time in that department.

I said, "Oh." And he said, "Yes. How would you like that?" I said, "I don't know. I have to think about it."

The more I thought about it, the less I liked it because I wanted to continue to play with "my toys" and my theories and my process designs and my calculations. I didn't want to be worried about how the foremen got their employees divided up for their shift work, or how to get some necessary men on the payroll. Nor did I want my life to be filled with worries about time cards and that sort of personnel thing, as well as outages and losses and spills of product, and other inventory problems.

[END OF TAPE, SIDE 5]

OTHMER: I wanted to be concerned more with the type of development work which I had been doing because I had been very well satisfied with my programs and work. The net result was that I made up my mind, ultimately, to leave. This, of course, I realized was in the middle of the Depression. I later asked for somewhat of a raise. As a matter of fact, I guess nobody got a raise that year.

BOHNING: You started at three thousand dollars. Where were you after four years?

OTHMER: Oh, I hadn't gotten very much higher. Maybe thirty-five hundred. I don't remember. Anyway, I asked for ten percent more or something like that. This was the 1930s and many people were being let out. So I thought, "Well, I've made so many improvements that I ought to be able to think of something new." I was a bachelor and had no responsibilities as far as family, and I thought that I could do better for myself. Rather than signing over inventions and patents at ten dollars apiece, I'll develop a market on my own.

Well, it so happened—and I don't remember whether this was before I actually left Eastman or not—that I met and made an arrangement with Sam J. Cohen. He operated a very little specialty shop, American Chemical Products Company, in Rochester, and did this in a very economical, cheap, inefficient way. He had two or three specialities. He was a graduate chemist, but not an engineer. He made things in glass in kilogram lots. I don't remember now just how I met him, but we made the deal that I would work one day for him as a consultant and whatever else in his shop that I could do for him. On the other days of the week, I could have his facilities, such as they were, not chemicals or equipment, but space, light, heat, power, etc.

Here I could put up my glass still up from the basement through a hole I cut in the first floor of his old, very dilapidated residence building, in the worst possible state of repair and housekeeping.

Then I built new stills very similar—two inch columns packed with glass Rashig Rings, the same as those I had used with Kodak. I bought from my friend the glass blower, the columns and the necessary accesories. So I set up shop, again distilling acetic acid and again, trying improvements on my azeotropic distillation with a new theory which I developed. I continued then the use of azeotropic distillation of aqueous acetic with a liquid insoluble in acetic acid.

As I tried new liquids, the idea came that the optimum entrainer is not only something which is insoluble in water and mutually soluble entirely with acetic acid, but it should be a liquid in which acetic acid is preferentially soluble compared with water. And I could see from thermodynamic activities that such a liquid would be better, if available. I could go into the principle but this is too much theory.

Within some months, I developed a new azeotropic material from a product just then being introduced by Du Pont. My successful entrainer for which Eastman had put so much money into plants to use was ethylene dichloride and in 1927, when I had started to work with Eastman, this solvent had just then been made commercially by Union Carbide.

The second good—much better—entrainer which I now developed was propyl acetate. I esterified it from propyl alcohol, which had come on the market industrially as a minor by-product of the first synthesis of methanol by Du Pont. Here then was a new process for improving the recovery of acetic.

Theory showed it to be very good—wonderful, in fact, as compared to ethylene dichloride. The practice of it meant that a given distilling column would handle 40% more feed or take that much more water out of dilute acetic acid. Thus, because the ratio of solvent distilled was so much lower than with ethylene dichloride, 40% more dilute acid could be handled in an existing azeotropic column. Because of their expansive construction, this was a great immediate capital gain, i.e., gathering the production of two columns more for every five on line.

Another advantage was that it would take less plates in the distilling column because, being a good solvent for acetic acid, propyl acetate would extract and wash the acetic acid down the column much better than ethylene dichloride, which is not a good solvent for acetic acid. This was so, even though the amount distilled overhead per pound of water (and thus refluxed) was much less with propyl acetate.

Propyl acetate did work very well in my little two-inch column. I found a lawyer and filed a patent application, which I received in a couple of years (10).

Well, that was 1931, in the depth of Depression. I tried every company that I could think of, and nobody was interested. Nobody was building new plants. I had saved money from my previous salary. I wasn't hurting financially, but on the other hand, I finally realized by the coming of 1932 that it would take some time to find any one interested in my new invention.

But then I had two employment offers. The Head of the Department of Chemical Engineering at the University of Michigan, Alfred H. White, was a good friend of the man who was the Head of the Department at Poly, John C. Olsen. Professor White of Michigan knew that I was available, so he suggested that I come here to Poly.

Also, there came at the same time another offer, and it refers to a very interesting incident in the history of the chemical industry. In early 1932, the Standard Oil Company of New Jersey, now EXXON, had just made a deal with the I.G. Farben—the Big German dye trust—to produce synthetic rubber. The two companies were to go into its production, as I remember I was told, as equal partners.

The vice president of Standard of New Jersey was a well-known chemical engineer whose name I knew well but I had never met—E. V. [Eger Vaughan] Murphree. His name had long been associated with the design and efficiencies of plates in a distillation column.

Murphee had found out that I was available, after seeing some of the articles or patents that I had, and talking with Professor White, head of chemical engineering at Michigan. Anyway, he suggested that I come to New York and we met. He had this interesting offer. I would represent Jersey in the direction of this new project; I. G. Farben would send a man over from Germany and the two of us would establish the development of synthetic rubber in Baton Rouge.

Looking back at his description, the chemical division of Standard Oil was a separate corporation, En Jay Chemicals, I think, or some such name. En Jay had another subsidiary, which in turn had still another corporate subsidiary, the names of which slip my mind. Anyway, there were a line of four subsidiaries down to the company JASCO that actually was to do the development of which I would have been a part. And I said to Murphree, "I remember from Greek mythology that the chaos from which came our Earth, there was a tortoise standing on the back of a whale which was swimming in an infinite ocean. Atlas was standing on the tortoise. On the shoulders of Atlas was a skin and on the skin was our Earth. Atlas persuaded Hercules to take the load, while he adjusted the skin, or something, and then Hercules bore the Earth for awhile." Anyway, I asked Murphree why the multiplication of supporters, "Why your hierarchy—five corporations—on top of each other to support in tandem this new idea?" He said, "The obvious answer; if one slips and the earth falls there is no blame to the important one, which gets the profits."

Well, for some reason or another, if it had been a really attractive offer financially, I might have taken it. And it was a better salary than I had from Eastman and much better than I could get in teaching. "But still," I thought, "I am an entrepreneur and if I go to work for Standard Oil of New Jersey I still won't be able to peddle my own inventions." And while I am certainly a capitalist, I don't want again to be a slave to the capitalistic system."

So I didn't become a polymer chemist and I prefered academe, where I would have not only academic freedom but also industrial freedom.

I never kept in touch with Esso's or JASCO's development; but looking back, Murphree's discussion with me must have been top secret then, since progress of this program was not reported.

The Buna type rubbers must have been the background, but the Esso name didn't come to my attention associated with Buna or other elastomers.

Much later, an article in Industrial and Engineering Chemistry in 1940 announced butyl rubber, a hydrocarbon product, as an ESSO development, under the well known names of R. M. Thomas, I. E. Lightbrown, W. J. Sparks, P. K. Frolich, and E. V. Murphree (11). This review article on elastomers (butyl rubber) gives the date of discovery as 1937, and patents to the above listed chemists and many others were assigned to JASCO, Esso Chemical, Esso Research & Engineering, Standard Oil Development, and others.

However, in 1932, I came to Poly where I would have not only academic freedom but also industrial freedom. To tell you how deep the Depression was, and this was now springtime in 1932, I was invited to come down and see Dr. Olsen at Brooklyn Poly. His proposal was that if he offered me a job and I didn't accept it, I would pay my cost of the trip. If he didn't offer me a job, he was to pay. If he offered and I accepted we each paid half. Anyway, the net result was that he offered me a job (this deal shows how much the market favored employers in 1932).

Dr. Olsen became a good friend and of the same age as McCormack, who was Head of the Department, whom I had known at Armour, Illinois Institute as it is now. Olsen was an analytical chemist, who had his doctorate at Johns Hopkins at about the turn of the century. His name was known for two or three books in analytical chemistry (12). He also had organized an original reference book of data. This was Olsen's Chemical Annual and Von Nostrand published it (13). It was modeled after the Chemical Rubber Handbook(14), which came later than Olsen's first edition.

I'm talking now as of 1932. There had been six editions of Olsen's Handbook before then and he was working on the seventh. He was very much an analytical chemist, and the seventh edition was being assembled with very little help when I came down and met him in 1932. Until about that time the two handbooks were very competitive, but much more money and personnel went into the Chemical Rubber Handbook and that seventh edition was the last of Olsen's Chemical Annual.

Dr. Olsen, to me, was then an old man of sixty years. I was twenty-eight. Age is a very relative fact or concept! I could see that he had only seven or eight years before he would retire. And, that wasn't far wrong.

BOHNING: This was the Chemical Engineering Department?

OTHMER: Yes. Olsen had been the head of the combined Departments of Chemistry and Chemical Engineering until the year before. In 1931 Ray [Raymond E.] Kirk had come and became Head of Chemistry, and it was then separated from Chemical Engineering.

BOHNING: And you came in just at that time?

OTHMER: I came in September, 1932, and I could see that in some years Olsen would be retiring and presumably I would be stepping into his shoes, which is what happened. He retired then in 1937 to become Professor Emeritus. He had been one of the founding fathers of the American Institute of Chemical Engineers and from almost the founding of the organization, in 1908, he had been the Secretary of the Institute and was at the time I came aboard. At about the time he retired from that position, or a year or two later, he became the President of the American Institute of Chemical Engineers. I came when we were at 85 Livingston Street, less than a half mile away from our present location. The building housing chemical engineering on the fourth and fifth floors was at 85 Livingston, while the connecting building, 99 Livingston, was the official address of the school. The Livingston Street buildings were just south and adjacent to the Borough Hall complex of buildings, the transportation, civic, political, judicial and governing center of Brooklyn.

BOHNING: I have just one thing I would like to clarify before we get into the Brooklyn years. I'm not clear when you left Kodak.

OTHMER: The first of 1931.

BOHNING: And then you went to work with this friend of yours?

OTHMER: Yes, in January of 1931 that began. I was quite independent. Our agreement was that I would work one day of the week for him. That never worked out quite that way. I was there almost all off the time and, as needs arose, I would help him do anything.

BOHNING: What was the name of the company and what did you do for it?

OTHMER: The American Chemical Products Company. Among other things, I developed one process for Sam Cohen, the owner. One of his specialties was acetamide, a very versatile material, which for some reason has never lived up to its possibilities in several fields. I developed a continuous process for manufacturing it, again depending on azeotropic distillation.

Equal moles of acetic acid and ammonia were fed separately into my distilling column and the water formed was taken off in the azeotropic distillation overhead. Two moles of water come over from making the ammonium acetate and one mole from the chemical breakdown of the ammonium acetate to give the amide. There should be an excess of acetic acid which collects with the amide in the distilling pot wherein its residence allows any necessary time to complete the conversion to the amide. From this distilling pot a stream of amide-acid mixture flows continuously to another column, and there is a dehydration of any ammonium acetate remaining. Acetamide has a boiling point, the highest of any component, 222°C. It is boiling in the second pot and is removed from there substantially pure.

I developed this continuous process, but Cohen was never able to build up much of a market for the acetamide. So that particular work really wasn't very important, unfortunately. But acetamide is a very interesting material. It is a wonderful solvent, but it is a solid at ordinary temperatures. However, this is an interesting example of azeotropic distillation. It removes the water formed and thus speeds up two chemical reactions, first that formed in making the salt and then that formed in its dehydration to make the amide.

But to answer your question, from January of 1931 to the fall of 1932, I was independent, and a consultant to American Chemical Products Company, a very little enterprise which Sam Cohen sold a few years later to Harshaw Chemical. Also, I consulted with Duffy-Mott in developing processes and equipment for apple products, juice, vinegar, sauce, etc.

Early in 1932, I had been approached by a builder of distillation equipment in Cincinnati, the Vulcan Copper & Supply Company. Hugo Wentworth had founded the company at the turn of the century. He was a first-class coppersmith and a successful businessman whose company supplied stills to the bourbon trade across the Ohio River in Kentucky, and during Prohibition expanded to Caribbean operations. His son, Ted Wentworth, studied chemical engineering at MIT—class of about 1927 or 1928—and was the first trained chemical engineer to work with his father's company. The two of them had came to Rochester to meet me in early 1932. I knew the company because it had supplied large amounts of copper and bronze for the acetic acid stills in Kingsport. They wanted me to work with them in building acetic acid plants from wood distillation to save the neutralization to calcium acetate, drying, then springing with sulfuric acid. We were hoping to sell several different plants who badly needed a "direct acetic" process, but it was the Depression and no one was buying anything.

[END OF TAPE, SIDE 6]

OTHMER: One of the best prospects was that of Cleveland Cliffs at Marquette, Michigan, from which Kodak bought acid. Later this was taken over by Dow, but a modern plant was never built there.

BOHNING: And the Chemical Engineering Department really had just then been formed at what was then the Polytechnic Institute of Brooklyn?

OTHMER: Yes, the year before, 1931, when it was separated from the joint department, Chemistry, which Ray Kirk headed ably.

BOHNING: How many faculty were there in the department then?

OTHMER: There was a Dr. [Willet F.] Whitmore, who spent part time in chemistry; he was a good organic chemist. And at about that time came Dr. [William H.] Gardner, who was primarily what we would now call a polymer chemist. He was concerned with polymer chemistry of natural resins rather than synthetic polymers. That was about it. Since all classes were taught also in the evenings, there were some adjunct professors from industry.

BOHNING: So you were the first person to come with a more traditional chemical engineering background?

OTHMER: Oh, yes. Dr. Olsen, the next year did edit a book with Chemical Engineering in the title; but it never was successful (15). It really didn't have the feeling of chemical engineering.

BOHNING: How many students did you have when you first started?

OTHMER: Our classes—all undergraduate—then were probably about thirty. That was the Depression. The tuition was very low.

I came to work in September, 1932, for $2600 a year, and the Depression deepened that year. Spring of 1933 was a low-point; even professors and heads of departments were leaving involuntarily. I was given a $100 a year cut, but to show it was not meant, I was raised in rank from Instructor to Assistant Professor!

Always, I had great hope for the sale of my patent. Kodak got in touch with me in the summer of 1932, and almost immediately when I came to Poly I started to work again with Tennessee Eastman, now as a consultant. I had written Jim White, the manager in Kingsport, about propyl acetate, and he responded, "Come on down and talk to us."

So I went down to Kingsport and, immediately on the street, bumped into one of my former friends, Cleo Cox, who was a coppersmith who had worked closely with me in the years before in the development program and building the stills. Such wonderful workmen in Kingsport. They were almost all of Scotch-Irish decent. They came from the mountains in eastern Tennessee and immediately adjacent Kentucky and West Virginia—hillbillies and moonshiners. This was prohibition time, but many had hardly seen a railroad before they left their home valleys. The company attracted them to Kingsport, taught them trades and they were the most loyal people ever, anywhere—excellent workmen, diligent, capable, honest.

Cleo Cox was a master coppersmith and he had worked with me, particularly on several emergencies. I bumped into him and he said, "Gee Doc, we're so glad to see you; we need you." When one of the executives had picked me up at the closest railroad station at Bristol, which was thirty miles away, that wasn't what they told me. He said everything was running fine. But Cleo said, "I can't keep these stills in repair and they can't shut them down for repairs because we have the acid running out of our ears. We are <u>overflooded</u>!"

Well, propyl acetate would allow the stills each to handle 40% more dilute acid; and per pound of acid produced, it used about 40% less steam. Also, only about one-tenth of one percent of the piping would be the only parts changed. Of course, Jim White and others never told me how badly they needed this additional acid handling capacity; evidently, other parts of the plant were giving more than designed production. I filed Cleo's comments away in my mind. We made a deal. It was the first deal that I had ever made and I was pretty green. Of course, they intimidated me. "Oh, you have a patent coming along but it isn't any good. You've already have so many patents in this field. The patent office won't allow it." (It turned out that the patent did allow some later, even broader claims than I first expected.) "And if the office allows, we may argue with you in the courts." They didn't say it in those words but that's what they meant.

So we settled and they gave me $150 a month or something like that to consult only on the process of the patent—nothing more—for this one school year and took title to my patent. Then they treated me so abominably when I offered to work with them on some ideas I thought good, on making acetic anhydride, which was entirely outside our contract. They turned me down on this and then intimidated me and another company against developing this anhydride process for the other company. Anyway, I continued relations for that one school year while I was teaching here at Poly, and then we had an argument.

I had agreed to consult exclusively with Eastman only on this acetic acid process, which I turned over to them. That was definite. I had an idea as how to make acetic anhydride which is a very close relative. They said they weren't interested, and thus I talked to Commercial Solvents Company, which was interested for reasons that would take much time to outline.

In the Chemist's Club, which I joined in 1933, I had met Charles McKenna, a vice president. He asked me to go to Peoria, their headquarters, to talk to their people. After agreeing, I received a letter from Jim White in Kingsport saying that I couldn't talk about acetic anhydride because that is really a part of acetic acid—although the new process I was working on never involved the acid. This was after White had said that Eastman was not interested in anything I did with anhydride. I told Charlie McKenna. He said, "You have your tickets to go to Peoria?" And I said, "Yes." He said, "Well, go out and tell the people there." He didn't want to take responsibility.

So I went out there and of course I told them the very first thing, that Kodak threatened that it would be in the way. One looked at the other and said, "Well, I guess the next train back is in a couple of hours." So I turned around and came back, embarrassed and mad as hell. The next month Kodak canceled my consulting contract. This was immediately after blowing my chances of working with Commercial Solvents on something quite outside our contract and after increasing the capacity of their multi-million dollar acetic acid plant by 40%, using my new patent which they now owned.

By chance, Hercules Powder Co. contacted me soon after this Commercial Solvents-Tennessee Eastman fiasco and invited me to Parlin, New Jersey, where they had a pilot plant to make cellulose acetate from the acetate linters which they produced, and sold in very substantial amounts to Eastman. The Hercules program was about as far along as was the Kodak one when I arrived in 1927. I told my acetic acid history to A. B. Nixon, the general manager of the large Parlin plant—a major operation—as well as the cellulose acetate pilot plant. (Nixon was a fine person, later president of Hercules).

He said he would contact Jim White in Kingsport, and I realized that the Hercules multi-million sales of cotton linters to Kingsport must have been in his mind. I went over in about a week. Nixon showed me White's answer: "All that Othmer knows about acetic acid has come from Eastman Kodak and belongs to us". This irritated Nixon, but he had waited to see me. He called in his secretary and in my presence, dictated, "We are sure Dr. Othmer knows what is his and what is Eastman's and we are retaining him now, as a consultant." This work with these very fine people lasted a couple of years, when I was told that Hercules was stopping it because their attorneys could not see an open way to produce cellulose acetate through the maze of patents to others on all aspects.

Often I have thought that the 1920s was the decade of viscose rayon fibers, the 1930s of acetate, the 1940s of nylons, with the acrylics and polyesters later in their turn.

A similar decision by a major chemical company came many years later. Fred Olsen, of Olin Industries of East Alton, Illinois (now Olin Corporation) was determined to get into the wrapping film business—something competitive with Du Pont's cellophane.

Even at that late date in the history of the acetate industry, legal opinions told Olsen that he could not by-pass all of the patents in cellulose acetate production, to be used to make such a film. Some variation of the cupra-ammonia process for making ordinary rayon was developed, and I was asked to help in the recovery of the resulting solutions containing copper.

Some considerable efforts, time, and money were spent, as this would have to be a good and economic material to compete with the established viscose cellophane. But one morning the <u>New York Times</u> noted that the Supreme Court had decided that Du Pont's monopoly of the cellophane industry must be broken. Du Pont <u>must help</u> establish a competitor. I called Fred Olsen, who had not yet seen the news. Our work on cupra-ammonium film stopped; Du Pont helped Olin go into cellophane.

Now, going back many years, Gray Chemical in Roulette, Pennsylvania, eight miles west of Coudersport, got in touch with me early in 1934. This was a wood distillation plant, about one-hundred miles due south of Rochester, New York. It was one that Wilcox had looked at about 1920 and had many advantages, but I have always thought that, as its boss, Wilcox felt he would be more independent if farther from Rochester.

Gray's people had seen some article or patent that I had written, and they were considering making acetic acid instead of calcium acetate. This was deep in the Depression, and I was glad to know that Gray had the necessary capital, from a sale of gas rights under some of its 30,000 acres of forest land.

After much demonstration work, Ted Wentworth of Vulcan and I did get the job. I persuaded Gray to use a new process. Meanwhile at Poly, I had developed still another patentable azeotropic process. The first process used ethylene chloride and, from the distilling column standpoint, was based on the idea that there would be an excess of the entrainer in the lower plates of the distilling column because the entrainer used would be one with a low boiling point. Therefore, it was easy to be separated from the acetic acid. Thus the upper part was removing the water and the lower part of the column was taking the high-boiling acetic acid to the bottom of the column. The entrainer separated upwardly by ordinary rectification and went to take the water out in its low-boiling mixture.

Propyl acetate was used in my second type of process and what I found in that development was that I should control carefully the amount of propyl acetate in the column. Propyl acetate itself boils at about the same temperature as water, thus is not easily separated from acetic acid if an excess is present over that in the azeotropic mixture with water in the lower part of the column. The azeotropic boiling point with water is around 82 °C, which is far enough from acetic acid to allow easy separation. However, since propyl acetate boils at about the same temperature of water, this is close to acetic acid. If an excess gets down into the acetic acid, it can't be separated readily and it may go out the bottom with the acetic acid.

Therefore, what I found desirable, possible and patentable was to add <u>just the right amount</u> of propyl acetate to form the azeotropic mixture only in the upper part of the column and not enough for it to descent to a lower level, where there would be only acetic acid. My patent on that, which I obtained but sold to Kodak for a pittance (16), claimed the use of propyl acetate to equal the amount in its azeotrope with water.

However, Shell Chemical in California, in the extension research of the oil companies to develop petrochemicals in the early 1930s, was trying to find markets for ethyl tertiary-amyl ether. I found that a small sample worked, much as does propyl acetate—<u>but outside my patent</u>—which Eastman had chisled me out of for a nine month, $150 per month, retainer fee. (This new ether was what I would have used with Hercules if Hercules had been able to get into acetate production.)

Since it worked in my laboratory here at Poly, we persuaded Gray to operate a small pilot plant, which we built in Roulette in the old primary house of the wood distilling operations, a vast barn covered only with corrugated iron sheets. We used as entrainer, ethyl tertiary-amyl ether. This new product, which Shell Chemical had just made, was only available on special order. It worked, and it had a boiling point close enough to propyl acetate so as to use this just the right amount of entrainer process; and it was free from the patents which I had licensed to Kodak.

I enjoyed working with the people in Roulette and made so many friends. They were such wonderful people that I wanted to be with them in that magnificent mountain forest country. Ultimately, I did spend more time there, and did build a very complete home in Coudersport, the nearest town, about eight miles away. That's another story. I won't get into that. But let me tell you one incident while it comes to mind.

First, we had built this pilot plant in Roulette. It was used equipment that Vulcan had in Cincinnati, and cost very little to put together as it came on a free loan. The column, about fifteen inches in diameter with condenser and decanter, went up some sixty feet from the ground and was surrounded by a rough scaffold with platforms about every eight feet and ladders between the platforms. Next came the column, some fifteen inches in diameter. With condenser and decanter, the structure went up some sixty feet from the ground—literally the ground in this building.

There were ten or twelve wood distilling plants within 150 miles and all were invited to send people to come and study. We were trying to sell everybody on this idea and process, not only Gray Chemical, which was the best financed of all of the operations, and the best managed. These people from the various other plants came to look, but it was hard to explain what we were doing since they had no understanding of any of the technical background.

They looked up and asked, "Up there you're taking the water off and you say you are boiling the water off at a temperature lower than the boiling point of water." I said, "Yes, that is the principle but, of course, the boiling point is lower here in this mountain valley because of the altitude of about nineteen hundred feet. Thus it is different than at sea level." "Well, how in the hell can you boil it way up there at the top of that column at a lower temperature than at sea level way down at the level of the ocean?"

Anyway, let me tell you one other little story of this operation. We did this during Poly's winter vacation, the temperature day and night averaged below zero. I had brought with me two of my students. One was Bill [W. Fred] Schurig, who was my graduate assistant at Poly, and the other was Jesse Bellet, the top student of the senior class. We drove up (some 300 miles) and stayed at a farmhouse which was a half-mile away from the plant.

We had to operate our pilot plant continuously. Bill was the older of the two, so he took the night shift and Jesse the day shift. I divided time between the two. This was in February and it was bitter cold. The northern tier of counties of Pennsylvania are on the Allegheny Plateau where they say, "We have two seasons, Fourth of July and Winter." And this wasn't Independence Day. I've seen and felt 40 degrees Farenheit below zero there.

One night I left Bill, as everything was going fine, drove to the farm house, and went to bed around midnight. At about four o'clock, the phone rang in this farm house. The two students shared a room and I had a room. Both rooms were on the first floor, just off the living room, where the phone was, an old-fashioned one with an oak cabinet hanging on the wall. When the phone rang, I knew it was for me. I woke up suddenly and ran over and answered it. One of the regular operators at the plant responded, "Bill is dead!"

"My God, I'll be right over." My car was parked just outside the front porch. Fortunately, cold as it was, the car started immediately.

I had requested that stairs be built between the platforms around the column. Instead, ladders had been built. Bill was checking temperatures, up probably 20 feet. Something went wrong, too much steam for some reason or another, possibly the boiler's steam pressure jumped. The boiler controls were not good. Anyway, for some reason or another the column "puked", as we say. Liquid went over the top and out the vent. Too much vapor load and liquid poured out the vents for pressure, and this came down in a tremendous shower.

We were using the new entrainer, ethyl tertiary-amyl ether. It had worked well. Shell supplied a couple of drums for this use. So this was what came over, some water, but mainly entrainer. It poured down and showered Bill, who was climbing half-way up. Fortunately, there was no fire. But he half climbed down, and half fell from these several landings about eight feet apart, with crude ladders between.

Safety in the old wood chemical plants was hardly considered. A working area never would have been like that at Eastman. But he tumbled down, passed out entirely from the ether he had breathed, but none was swallowed.

When I arrived, I was so relieved to find him sitting up against one of what they called the primary stills, in this part of the operation of the wood chemical plant. I said, "Well, let's get back to bed." He was still three-quarters unconscious. I opened the large sheet iron door big enough for passing a big still. These plants always were merely big barns of corrugated steel.

Outside, was this wonderful, beautiful full moon, but it was so cold, no snow. I half carried him, and all he had on was a laboratory coat over his shirt. The temperature was something below zero.

The shock of the cold air knocked him out, and he dropped to the ground, just slipping out of my arms. And a shock to me! All I could think of was Bill, my responsibility to him and to his father. His mother was dead. I knew his family—his father and his sister. What to do? If I had mentioned ethyl tertiary-amyl ether to any doctor, he would know nothing. It had never been used. Well, the only thing I could think was artificial resuscitation and I went to work on him. I rolled him over, with his back on the frozen rough ground. I remember the frozen ridges from wagon tracks under his shoulders. I had to pull his tongue up a couple of times, and I pumped his ribs, hardly covered with flesh because he was skinny.

Finally, he started breathing this ether out at me with its characteristic odor in the wide outdoors there, and no breeze.

It was just absolutely still, but cold. Finally breathing came in deep gulps. I picked him up and drove back to the farm house. By that time he was able to walk a bit, and I helped him into his bed alongside Jesse's.

I couldn't sleep in my room, next to theirs. Every once in awhile, I would hear his breathing; it was awful. I would get up and look into his room and could smell this ether from his lungs, slightly sharper than the sweet odor of usual ether.

The next morning, Jesse and I went over to the plant. We let Bill sleep, which was causing him no pain or worry. Along about eleven o'clock or so, he came over and asked what had happened. I told him as nearly as I could. I asked, "How's your insides?" He said, "Oh, I had a good breakfast." He was a very hearty eater. Mrs. Nichols had fed him well, as us always, a real country-style breakfast.

Then he said, "Doc," (to my students until they graduate, I've always been Doc. And after they graduate, I'm Don. There's always this slight change of one letter from Doc to Don.) "Doc, I don't understand it. I know it was all in my lungs but why are my <u>ribs</u> and <u>back</u> so sore." My vigor and the frozen ground had their bad effects closer to his surface, as well as the better one on his lungs.

We proved the process. Ted Wentworth designed and built the plant for my process and all worked well (I enjoyed designing the building). My good friend Bob Lyman, Gray's president, telephoned me here at my office after the plant had been running for some months and said, "Don, we had a spill." I said, "Anything serious?" He said "Yes. All of the ether we had was in the system and someone turned the wrong valve and it was lost down the drain."

"Gee, that's too bad." He said, "Yes, and I called Shell Chemical out in California and they said it wasn't now in stock and they wouldn't get around to making anymore for several months, and then it might be many months. What do we do?" I said "Gee, Bob, that's wonderful." He said, "What do you mean, wonderful?"

I said, "You know I had been developing something better and cheaper and readily available." He said, "For what?" I said, "To use instead of this special ether." He said, "Oh, can we get some of that?" I said, "I am sure you can." "What is it?" "Butyl acetate." He was a forester, a wonderful chap, this man who was the president. He said, "Yes, I think I've heard of that. They use it in automobile lacquers." I said, "Yes." He said, "Wonderful. How much do we put in?" I said, "Put in about the same amount, get about the same level in the decanter and it should work very well, only a little difference in the temperatures, not more than a few degrees, and I will tell you what to do to control temperatures with the new entrainer."

So he called Buffalo, somebody in the chemical business, about 150 miles northwest; some drums of butanol were trucked down that day. With the anhydrous acetic acid in the bottom of the column only butanol is fed into the system and the acetate is made concurrently.

That was the end of the special ether and after that we had no need for Shell Chemical. It was not until a year later or so that a poorly prepared patent on the use of ether in this process was issued to Shell Chemical (17). One of the inventors given on the patent was the technical salesman I had told just what I would do with the liter sample he supplied for my first work in my laboratory at Poly. Even with all my writings and printed patents on azeotropic distillation, neither he nor the scientist he put to work in the laboratory could understand what made it work, as his patent shows.

I had received my patent in spite of his efforts (18). He never sold more of the ether to Gray or anyone else for azeotropic separation of water from acetic acid. Both Gray and I were lucky that the loss of their entire inventory of the ether made Gray change abruptly to the readily available liquid, butyl acetate. Looking back, I have sometimes questioned whether the disinterest in selling "for many months" more of the ether may have been a purposeful delay while their patent application matured.

Butyl acetate, like propyl acetate, is a good extracting solvent for acetic acid—not quite as good in some ways, but its boiling point is considerable above that of acetic, of course much higher than the ethyl tertiary-amyl ether. Therefore it is considerably more efficient in water entraining ability—and acts in the mode of what I have called, "insufficient entrainer." The separation by distillation of the last of the water from acetic acid is not so difficult, while the separation of the last of the acetic acid from water is very difficult. Hence, if there is a small percent of water below the level of the entrainer's descent in the column, the reflux and a few plates will allow the production of anhydrous acid at the bottom.

Because of this <u>insufficient amount of entrainer</u> to form the azeotrope with all of the water, going downwardly in the column, the last of the entrainer disappears at an acid concentration of some 80% to 95% (20% to 5% water). This operation is readily indicated—and controlled—by temperatures shown by thermometers placed in several plates near the bottom. These show the absence of entrainer and the relatively easy rectification of water (as more dilute acid, which rises in the column, where it is separated) from glacial acid in the reboiler, which is possible as shown by the vapor-liquid relations, only at high concentrations of acetic acid.

Looking back, I feel that I should have been able to have gone from ethylene chloride to butyl acetate in less time than from the years 1927 to 1935 although there were many practical and engineering aspects to be learned in between. These would not be of interest here. Theoretically, the ratio of through-put for a column of a given diameter was on the basis that ethylene dichloride gives 100, propyl acetate gives 145, and butyl acetate gives 220. Considering the high cost of plant construction to withstand corrosion, this is a very great difference in dollar investment, when millions or billions of pounds of water must be distilled each year. On heat consumption per pound of feed, the theoretical ratios are: 100 for ethylene chloride, 74 for propyl acetate and 51 for butyl acetate.

Most of the acetic concentration plants I designed after 1935 used butyl acetate. This included those in this country, Japan, Sweden, England, Central and South America. Also, there were plants of very substantial size used for recovering the acetic acid used as a solvent in making the nitramines, RDX and RMX, high explosives of World War II. For different reasons other entrainers were used in five plants producing a World War II chemical, still classified; also, in a plant in Burma for removing aromatics from kerosene to stop smoking of kerosene during its burning.

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INTERVIEWEE: Donald F. Othmer

INTERVIEWER: James J. Bohning

LOCATION: Polytechnic University

DATE: 11 June 1986

BOHNING: Dr. Othmer, the last time we concluded while discussing your work with the Gray Chemical Company of Roulette, Pennsylvania. Can you tell me anything more about what happened to that plant?

OTHMER: We started planning for this plant of the Gray Chemical Company in early 1935. It was for the production of methanol (wood alcohol) and acetic acid from the pyroligneous acid coming from what might, over simply, be called the condensation of the smoke from wood distillation. It was to be the last word at that time in technology in the wood chemical industry, i.e., the carbonization of wood. The methanol was first distilled out and then refined, then the acetic acid. Always, there were tars, and materials which combined to form tars.

This industry had been extremely important during World War I because the acetic acid was neutralized with lime to give calcium acetate. After drying, this then was roasted or pyrolized at a relatively high temperature to give acetone and lime. During World War I, this acetone was very much in demand as it was used to dissolve cellulose nitrate (gun cotton) for use as an explosive. Later calcium acetate was not used to make acetone because fermentation gave acetone along with with butanol and methanol in the so-called "Weizmann" process, developed only after after World War I.

The Gray plant worked successfully, although we had some problems. At first many were due to the ever-present tars which formed from the cresols and aldehydes present in the pyroligneous. Often products were non-viscous liquids, sometimes they were like the hard, brittle solids which Leo Baekeland had synthesized from pure cresols and aldehydes. During early start-up problems, we developed a method, general also for other systems of processing pyroligneous, which entirely eliminated this ever-present tar problem.

One other problem had a very interesting chemical solution. The acetic acid must be made anhydrous, that is above 99.5%, and it was shipped as such. However, we were confused and irritated in that the acetic acid, instead of being water-white, had a very faint yellow color. After much work, we discovered this was due to a yellow liquid called diacetyl which stayed all through the processing and boiled over with the acid. We then found that by the addition of hydrogen peroxide, one molecule of diacetyl would form two molecules of acetic acid, our very product!

A very simple purification process thus involved merely the suspending of a two-gallon bottle of concentrated hydrogen peroxide above the final product meter, and letting H_2O_2 drop, drop by drop, into the acetic acid as it was metered following the final purification still and condensation in the silver condenser. Either in the pipeline going to the final aluminum storage tank or in that tank, the diacetyl reacted with the acetic acid which was well mixed with the hydrogen peroxide; by the time the material was shipped, it was water-white. This was a neat purification process—instead of separating one molecule of colored impurity from thousands of molecules of the product, a single molecule of a common reagent H_2O_2 produced without fuss or further processing two molecules of the product, acetic acid.

Those years before World War II were the time of great development of the petrochemical industry in this country. However, acetic acid already had been made from calcium carbide via acetylene and acetaldehyde. This had been done in Canada at the Shawinigan Falls and also at Niagara Falls, both having cheap hydroelectric power for making calcium carbide. While these chemicals did not come from petroleum, they are still called petrochemicals because later, acetic acid did come directly from petroleum. However, the competition of this acetic acid with that from wood distillation, and the fact that the charcoal had only a limited market and could not carry the cost of the industry as a whole, was killing the wood chemical industry.

Only the expansion of all industry during World War II kept the wood chemical industry alive until sometime after the war. By that time, Gray became nonprofitable, and operations ceased for the sixty-year-old company.

Because of inflation in the price of copper, it was possible to sell the acetic acid plant for almost what it cost, because it was made of such heavy copper. It was sold to another wood distiller, which was better situated in another part of the country as to charcoal markets, and was used for its designed operation of dehydrating acetic acid.

It might be worth a couple of minutes to recount the history of what was once a very important part of the chemical industry, that of charcoal and wood chemicals. The oldest plant, which I have designed units for in sixty years of watching the slow decline of the wood chemicals industry, is still operating, I believe—the Shirley Aldred Company in Worksop, England. Almost two hundred years ago, in 1792, the first of many Aldreds started condensing chemicals, including at first the tars and tar oils coming from the incomplete burning of the oak wood of Robin Hood's Sherwood Forest to give charcoal. These materials had use with wooden sailing ships, caulking wooden joints, waterproofing of sails and ropes, etc.

During the last century and even before its middle, when [Sir William Henry] Perkin used the tars from coal carbonization as a feedstock for producing chemicals, pyroligneous acid from wood carbonization yielded many other materials, and, of course, their combinations yields many more. A total of some 180 compounds as such have been identified in it. Some of these had great commercial importance by themselves, others as starting materials for further synthesis.

Wood alcohol, methanol, was the best known chemical from wood—although wood tars found many uses. In 1900 in this country, methanol had almost a monopoly of most nonpotable alcohol uses, because all ethanol had to pay the same high tax as that in for beverages—spirits as whiskey, gin and brandy, wines, beers, etc.

Just after the century turned, Congress passed the law allowing alcohol for industry to be denatured—i.e., made unfit to drink—and to be used and sold without tax. The wood distillers cried, "Foul. We lose this monopoly and our business." To appease them, Congress said, "All denatured alcohol must contain five percent impure methanol." This impure denaturing grade carried many of the nasty allyl and other volatile compounds which made it quite undrinkable. However, all industries were growing at such a rate in the early 1900s that in a few years, more methanol was sold as denaturing grade than in total before. Ford and other early automobiles used methanol or ethanol as fuel. They were standard liquids, while gasoline was a nonspecific liquid, the discard from refining kerosene, the burning oil which was petroleum's important product.

World War I required calcium acetate to give acetone for dissolving gun cotton. A score or more little plants sprung up in the mountains of northern Pennsylvania and southern New York, also Michigan, Tennessee, and elsewhere. The calcium acetate was usually shipped to a central plant, where acetone came by pyrolysis and condensation. The Weizmann fermentation broke this monopoly of wood distillation by the early 1920s. The acetate of its co-product, butanol, was produced in large quantities for automobile lacquers. By the early 1930s, the excess of acetone so co-produced could not be sold in amounts corresponding to the butanol produced, and the Commercial Solvents Company was storing millions of pounds at an inventory valuation of one cent per pound.

Pure methanol was made from coal via carbon monoxide and hydrogen in the middle 1920s, and so this monopoly was lost by the wood distillers; with, however, the paradox of the very crude denaturing grade selling for several times or more a gallon as did a 99.5% pure grade.

Acetic acid was "sprung" from calcium acetate with sulfuric acid to give about seventy percent crude acid. This was distilled in columns having heavy copper shells, perforated porcelain plates with downcomers fastened in with silver wire. The condenser was a silver coil discharging to ceramic pipes and tanks. (All of this was, of course, before stainless steels and aluminum.)

In the late 1920s calcium carbide, from coke, lime, and electric power gave, with water, acetylene. Further hydration gives acetaldehyde, which is oxidized to acetic acid. Synthetic acetic acid was coming to market slowly when I came into the industry, but because these markets were expanding greatly, the slow loss of this acetic acid monopoly was not complete until after World War II.

Charcoal, the main product of the industry, had lost its monopoly in the blast furnace for making iron long ago when England's forest, inadequate for booming steel requirements, had to yield to its coal mines, and thus production of coke. In the late 1930s, I did try to help produce acetic acid in a make-shift charcoal plant in upper Michigan adjacent to the only charcoal blast furnace I have ever seen. ("Charcoal iron" sold at a few dollars per ton premium as being purer than "coke iron" and thus had some better properties.) And I did consult in the design of an acetic plant for a charcoal iron plant in a forest east of Perth on the west coast of Australia. We were asked to go down there, years later, for a rebuilding program in the early 1950s, from one of our numerous sojourns in Rangoon, Burma. But we found that Australia is a "long way from anywhere" including Southeast Asia and we could not afford the time to make the visit.

In looking at the fundamentals of the charcoal industry, originally it was made in "beehive kilns" in the forest with very little capital cost, no recovery of chemicals, and a low tonnage cost for use in the iron industry. Always it was a low value product, and through the 1930s, the biggest market for charcoal from Gray was as a domestic fuel in the "cold-water" flats of New York City—the same use today for charcoal as in many developing countries. This use had led to the deforestation of many countries of the world. Through the 1940s, and up until now, charcoal has become a luxury fuel for backyard barbecues. The pattern has been from a monopoly in many things, from the largest use of charcoal in smelting iron, methanol lost its position as a nonpotable alcohol, acetone, acetic acid, their derivatives and other chemicals, so the industry has been a long time in dying as the successive monopolies were lost.

BOHNING: Your first-hand history of wood chemicals is interesting. Do you remember what year the Gray Chemical Company went out of business?

OTHMER: That must have been in the late 1940s. Meanwhile, I had become so closely associated with the company and my friends there that I wanted to spend time in that delightful forest land. I had built a rather fine home in Coudersport which was some ten miles east of Roulette and is the county seat of Potter county. In 1936, I was able to buy a beautiful seven-acre tract, with a two-acre pond fed by a spring that delivers over two hundred gallons of pure delicious cold water per minute, even in the driest weather. The half-mile northern boundary of the land was the South Branch of the Allegheny River, over which a private bridge—50 feet long—ended at the junction of two of the nicer residential streets of the town.

Developing and landscaping the surroundings and building the house was indeed a great activity and source of pleasure to me, and I spent more time designing and building that house than for any chemical plant that I had ever worked with.

I always bought—and cheaply—bordering property as it became available, until there were about four hundred acres on my south side of the river, mostly up a rather steep hillside of timber land. During World War II, everyone was thinking and trying to buy everything except timber land. I bought cheaply several tracts scattered throughout the county, totaling almost ten thousand acres of woodland covered with trees of fifty years growth, usually for a few dollars per acre. From most of this acreage, I allowed the Gray Chemical Company to cut selectively the timber during the war. When the war was over, I felt that I didn't want to wait fifty years for the land to produce another crop which it would do without any reseeding under the conservative system of woodland operation which we used. So I sold the land to West Virginia Pulp & Paper Company through my friend, Bob Lyman, its local forester. Unfortunately, while I did profit, I would have made several times more profit if I had kept that land from the early 1950s, when I did sell it, to the present time. That land around the house, and within the limits of Coudersport borough was sold more recently, the last thirty-five acres not until 1986.

Ultimately, the property, which was almost an even three hundred miles from Brooklyn, became somewhat of a snare and a delusion. When I built it I thought that, as a professor, I would take three months off each summer. It seemed a good plan to build a summer home far enough away so I would not be running back and forth between the Institute and Coudersport. Instead, I expected to spend the whole summer in the lovely country of the Allegheny plateau and enjoy the friends that I had made there, and the development of the beautiful property into a lovely estate.

What happened instead was World War II, and I, too old for ordinary Army service, became involved with several federal departments as consultant—the Army, the Navy, and particularly the State Department—for several missions to Central and South America. For chemical warfare, there were six different plants using my processes in various parts of the country—then built by Wentworth—for chemicals for protection against poison gas. Some of these are mentioned elsewhere. Of much greater size and output was the very large plant using my processing for acetic acid which Tennessee Eastman built that made most of cyclonite (or RDX), the high explosive developed during World War II. The net result was that I had too little time to vacation.

But back to Coudersport. In 1950, I married and we did go up; but everything had become so busy that we never were able to get up there for longer than a weekend at a time. That was quite impractical because it meant three hundred miles of driving each way, partly on mountain logging roads, for two or three nights there. While the house was as good as I could design (with a very competent architect) and make it with the finest materials and best craftsmen from within one hundred and fifty miles, there were always things to do to keep the house in good repair and service. It has two miles of electric wire (No. 14 and heavier) between over two hundred outlets, four or five bedrooms and as many baths, and two tons of copper on the roof. It has its own water and sewage disposal systems, and a private bridge over the headwaters of the South Branch of the Allegheny River, the headwaters spring of which is just ten miles east of that bridge. But unfortunately, Coudersport is not on a rail line and the nearest airport serving planes to New York is ninety miles away.

Ultimately, and that was only in 1983, we gave this rather fine home to the local hospital for use as a nurses home, along with the pond and the seven acres of beautifully landscaped grounds and gardens which we had labored in so as to beautify the lovely spot. It was a nice place but we couldn't use it enough to justify keeping it. We have been delighted we did give it to the hospital to be used for a nurses home in a community which had no facilities for such residence and neither rail, bus, nor plane connections. Nurses are a major problem of getting and keeping, especially in Coudersport, with no amenities except rental rooms in homes or over stores. The very fine local hospital also has plans to use it as a center for conferences for attracting seminars on medical problems.

BOHNING: Let's come back to Brooklyn then. The last time we talked about your position here and how you came to Brooklyn and the fact that there were just a few people in the chemical engineering department which had just been split from the chemistry department the year before, in 1931. What was your first teaching assignment and responsibilities as a new faculty member?

OTHMER: At Polytechnic, all students were men, only in the very last class I taught was there one woman. For many years, there were no undergraduate women. They were allowed in graduate classes only if that class was not taught elsewhere in New York city. Now all is changed. At that time, Polytechnic had a large undergraduate evening division as well as the day division. Graduate courses also have always been taught in the evening, so men who are employed within a large commuting area could come.

My first class was on the use of fuels, also the related chemical engineering heat and material balances. This was an evening class, and I shall always remember walking into the class at six in the evening, just on time. There were some forty men who averaged more than my age. They instantly became quiet and looked at me expectantly. I recognized the ball was in my court; I stepped up and broke out with a loud voice to begin a lecture which had been well prepared in advance. I know that all of my lectures after that were as well prepared, but at least I did maintain the attention and definite order in the class.

I was not a strict disciplinarian and I do remember telling that class, and other classes, that I didn't care whether a man showed up or not. I never took attendance. They paid their fees which each could calculate to be a fair number of dollars per lecture; if a man had something to do that was worth more than that to him, he should do that instead. However, I said, their presence was appreciated only because in the large classroom, some clothes and bodies would absorb the sound of my voice and prevent echoes.

By and large that was the start of a long and pleasant relationship with many students over many years. Our students are serious. Never did I have any disciplinary problems.

I took the job of lecturing in stride, although I never had the very real joy in this that many professors have expressed. A first graduate class, Fluid Operations, was however a source of satisfaction. I started it in 1932 or 1933. Since slots for times and rooms were almost filled in evenings, I selected Saturdays afternoons—lecture from 1:00 to 2:30, laboratory from 2:30 to 5:30. No quizzes, but a few short and four long problems were for individual solution in calculation and design, of systems or plants in data processing, hydraulics of fluid other than water, evaporation, and distillation. The students did not complain, although this homework was probably excessive, and when asked, students told me that the semester's total was 75 to 125 hours of homework and more. However, the problems led to solutions which seemed to satisfy the students by their accomplishment—and me by the papers turned in—and there were no nervous strains of cramming for an unknown exam.

I shall never forget one incident regarding this first class of about twenty graduate students, almost all from industry, many older than their teacher. During that semester, one of my Rochester friends, an executive of Kodak, was in town and invited me over to Manhattan for lunch. I should note that Kodak always had an intense rivalry and jealousy against Du Pont. So I told him that I had two bright young men coming the forty miles from the Parlin plant of Du Pont. "Don," he said, "do you mean you are actually teaching Du Pont men how to build chemical plants?" "Yes," I responded. "Don, teach them wrong!"

The second year, this first graduate class doubled, more than the laboratory could accommodate, so with assistants, we had half of the students in lab in the morning, half in the afternoon, and all in the midday lecture. By this time many men were not working Saturday morning as hitherto had always been the usual practice. Enrollment increased, and so those who had what was regarded as adequate laboratory experience in their undergraduate school or on their job took only the lecture course. After the war, the large attendance required the Saturday stint, a second lecture class, and one or two laboratories in the evening. The class total was 123 students, which I have been told is the largest graduate class registration in the school's history.

Students in general had a feeling of accomplishment in solving the problems, and I enjoyed working with them. After the final lecture, in two different years, at the final "Good-bye" they gave me a hearty hand-clapping and cheers, the most heart-warming occasions in my many years of teaching.

Every evening student and day student was required to present a research thesis for an undergraduate degree as well as for a graduate degree, and I did enjoy the work in advising individuals on their problems. These were not merely laboratory assignments. Each student had to come up with a bound copy of a dissertation at the end of the year and this taught the young men—all of the classes in those days had only men—the theory and practice of research, the carrying out of the research, and then putting results into words and script, and finally into a bound volume. This was quite a task and quite a challenge and often there were surprisingly good results.

Our graduate classes were also in the evening, so many of the research projects were done part-time by men who were employed. I do remember in one month's issue of the old Industrial and Engineering Chemistry, we had five papers, each carrying the name of one of my students, as well as mine (19). All five reported were bachelor's and master's theses which had been given previously as papers in one of the symposia on unit processes which were organized year after year for the Industrial Division of the ACS by my long time friend, Professor R. Norris Shreve of Purdue University, during the late 1930s and early 1940s. This is mentioned to indicate that the work that these undergraduates did was rather worthwhile.

While commenting about the old Industrial and Engineering Chemistry, which ran from 1906 to the late 1960s, one of my students went through the tables of contents of its entire publication life and told me that my name appeared on over 120 papers, more than any author in its history and averaging about three per year during the thirty-odd years of my representation.

During the 1930s, the years of the Great Depression, so many competent and motivated men could not go to college. Since they had top minds with top ability, they were able to get jobs and simultaneously to pursue the academic work as a graduate student in our evening courses. That was extremely important to them and allowed us to expand greatly our graduate programs.

When it came to discussion of topics for thesis research, I always have told every single candidate for a thesis that I wanted him to work on something in which he was interested because he was going to spend much time and effort on it, and it might as well be in a field of interest and benefit to him. In most cases, the men working in individual plants often were most interested in the work that they were doing. That meant they had to get written clearance from their superior to be able to work on a problem with regard to the making of some chemical in which their company was interested, or some other chemical engineering problem which might be associated generally with such manufacture.

I remember in one case a doctoral student told me that his superior said, "No. You can't work with Dr. Othmer because he will then become privy to our secrets of manufacture and our production of this particular chemical which, however, we would like to have studied as to this phase of its manufacture." I settled that point so that the student could work on a process design problem of his particular interest. I signed over any contribution I might make, and pledged secrecy in a document making me a consultant in that field to that company. For this, I acknowledged that I had received—although I never did—a dollar a year, during the two years that this man was working for his doctoral thesis.

We have always boasted that Polytechnic is a school of opportunity. I took every chance to make whatever adjustments were necessary for students to do their work and to do their theses under difficulties pertaining to their particular lives. Only highly motivated and capable men survived; these would be the men who were doing top work in their regular employment.

On various occasions we had students coming from as far as the other side of Philadelphia for our evening classes; also, from the other side of New Haven, Connecticut. Consider that these men, if they stayed in class until 10:00 p.m., then would have two or more hours of transportation to get home—very few drove cars to Polytechnic. In any case it was a very rough schedule for the men involved, and I tried to make it as easy as possible.

In some cases, we allowed the student to do his thesis research in his company laboratory. Thus, in several cases when they were no longer taking classes, they did their work at their job sites, some distance from the Polytechnic. They would come in only two or three times during the year to meet a committee, or to discuss their progress with me as their advisor. In some cases, they worked with very large expensive equipment, pilot plant equipment, in companies as far away as Cincinnati or in one case, northern Michigan.

In the case of one student operating a small chemical plant in Michigan, one of my prize students of all time, Dr. Raphael Katzen, actually had to take one course in Chemical Plant Design which he had not completed while he was in residence for his master's degree. I gave that course by correspondence, and possibly it has been the only graduate course ever given by mail by the Polytechnic.

He then persuaded the small company he was working for to establish at Polytechnic a research program for four men on his new system for pulping wood, and then came back there to direct it for the two years further to get his doctorate. He was a very capable student and has been very successful ever since. He now operates two or three companies in the consulting engineering and plant design business and over the years has been responsible for the design of many hundreds of millions of dollars worth of chemical plants. He was in the class of 1936, which had several other outstanding men. I have always felt 1936 was at least one of the best classes of any in chemical engineering at the Polytechnic.

Now we are fifty years later. Last June, Katzen and others were here to help celebrate the fiftieth anniversary of that Depression year class. (Katzen was actually in the class of 1935, but he had to stay out one year to earn funds to continue.) Some of these outstanding men of 1936 showed up as heads of their own companies and all have come very far along in the practice of chemical engineering. It was a class of which everyone at Polytechnic is proud, particularly myself, as it was the first class I saw for the full four years.

Obviously I cannot salute all of my brilliant and successful academic sons, but as to thesis research, I remember that Don [Donald Q.] Kern did his at an installation of Foster Wheeler thirty miles over in New Jersey, which I visited from time to time. Kern's Process Heat Transfer, which McGraw-Hill has kept in print for generations of chemical engineers, has been one of the great and classic books of our profession (20). Unhappily, it has long outlived Don.

BOHNING: Were there certain companies that many of the students were working for, or were they quite scattered?

OTHMER: In the 1930s and 1940s, and to a degree which keeps lessening ever since, New York City has been the center of three major industries of our interest. First, there is the chemical's manufacturing industry. So many of the country's major chemical producers had their executive offices, engineering offices, and in many cases their laboratories, either in Manhattan or in the other boroughs of the City of New York or within a few miles of the city.

The second major industry was that of architect/engineering, with companies like Kellogg, Lummus, Foster-Wheeler, and similar companies all having their offices before World War II mainly in the financial district of downtown New York. At a later time many of these companies moved up to the 42nd Street area and later to the Radio City area.

The third main industry was the petroleum industry, with Exxon, Mobil, Texaco, and others first downtown, then uptown, and now in Houston, Texas. (Incidentally, the large oak double desk—about six square feet—which I inherited from Dr. Olsen when I became head of the department, and later discarded, had been John D. Rockefeller's in the old Standard Oil Building on lower Broadway.)

Between petroleum and chemicals came the petrochemical industry, and Union Carbide had for many years its offices at 42nd Street and Madison Avenue. Thus, within twenty or thirty minutes subway time we had the junior engineers working at those companies, men with bachelor's degrees who were wanting to carry their engineering-academic work farther. We offered a very wide range of courses for them here at the Polytechnic. Attendance was simplified by the fact that Borough Hall, Brooklyn, the area where we are still located, is the center of the City's subway facilities. It has always been probably the best location for subway transport of any place in the five boroughs. The time by subway from downtown Manhattan has always been five to eight minutes; from Times Square, Penn Station, or Grand Central, twenty to twenty-five minutes. As a matter of fact, across the street from our present building, for thirty years has been the City's Transportation Building. This is practically the crossroad for all of the trains underground. Special trains collect subway tokens every night after midnight, using only a comparatively few feet of special track, and unload them in the basement of the Transportation Building.

Our evening undergraduate work expanded greatly during the years of the Depression and particularly during my time, 1932 to World War II. Our daytime undergraduate work, however, was falling off—people just didn't have the money to go to school full-time—but still we had this advantage for our evening work, including our graduate work. Those motivated, competent men who with funds would otherwise have been in school full-time, were able to get jobs because they were "tops", and did have the determination and stamina to work days—usually giving top performance—go to school nights, and study weekends.

Then came the war in 1939. Before America had entered the war we were for years building up for it in our chemical and engineering industries, and the several years of expansion continued. At the end of the war, the GI Bill of Rights brought a tremendous influx of students, both graduate and undergraduate.

[END OF TAPE, SIDE 1]

BOHNING: When you had your graduate students doing their work out in the field in these companies, did you have contact with their supervisors within the company?

OTHMER: Yes indeed, particularly when the companies were remote from New York City, and when I couldn't conveniently visit the students. Always, there were companies with which I either consulted, or in which I had good friends and associates, or former students. I could keep good track of what was going on in the graduate research programs.

BOHNING: What companies were they?

OTHMER: One was Northwood Chemical Company. That was where my good student, Dr. Katzen, worked. Another was the Vulcan Engineering Company in Cincinnati, where several top students worked in succession under my long time friend Ted Wentworth. Without checking, I can remember two men who did their doctoral research there and two others who did their master's research. Now they are chief executive officers in chemical or engineering corporations. One is Joe [Joseph J.] Jacobs, who did his bachelor's, master's, and doctoral theses under me and runs Jacobs Engineering, long listed on the American Stock Exchange. Another company was in Cleveland, Diamond Alkali Company, which has since become Diamond Shamrock. American Cyanamid was another one, with their laboratories not so far away in Connecticut.

BOHNING: You said that your undergraduates were doing work that was publishable. What were your laboratory facilities like from 1932 on?

OTHMER: They were deplorably poor. We did not have the instrumentation and other facilities that are available now. On the other hand, research didn't seem to be as expensive then as it has become now. The frontiers of science and research move outwardly, and require more and more complicated instrumentation. We really operated with a very low budget and very little equipment. We improvised, but fortunately we always had one or two excellent mechanics in the laboratory. Thus we could build much of the equipment that was needed.

Instrumentation was in other cases woefully inadequate. But we made do and on some occasions we borrowed instruments from laboratories of companies where students were working. I remember at least several good papers from excellent researches on adsorption done with a McBain balance, which we improved with a cathetometer that we borrowed for several years from the American Cyanamid laboratories.

Looking back over the years and the scores of peer-reviewed papers in the journals of this and other countries, I am astounded at what little money was spent. We did "make do" and I confess this was partly because I recognized the scarcity of funds. I was not aggressive enough in demanding my department's share of Polytechnic's very limited funds. I shall never forget what I regard as one of the greatest compliments I ever received—and that in a letter to Alec Jordan from my long time friend Sid [Sidney D.] Kirkpatrick. Sid had been a fellow editor with Alec—first of Chemical & Metallurgical Engineering, later of Chemical Engineering—at McGraw-Hill. Alec was the founding editor of the company's long-time success Chemical Week. Alec quoted Sid's letter in nominating me for the Perkin Medal. This, after making some complimentary remarks about the success of the department I had developed and of the published research conducted with numerous of my students said, "All this was accomplished with a logistic support of space, equipment, and budget that was an order of magnitude lower than that of many schools of less prestige." And in those years, the American Institute of Chemical Engineers ranked Poly's department of chemical engineering as one of the top ten of the country.

BOHNING: There was a five-year period from 1932 to 1937 before you became chairman. How did the department change in that five year period?

OTHMER: We changed the curriculum entirely during that time. Prior to that it had been a very qualitative and descriptive exposition of chemical manufacturing processes as they existed then. This was instead of the science and profession of chemical engineering devoted to the determination of the quantitative factors for designing and controlling their operation—heat transfer, mass transfer, diffusion, etc., as well as thermodynamics and reaction velocities.

We were able, however, to do some rather good research with rather simple equipment. One that I remember was the determination of reaction rates for the esterification of some volatile alcohols and acids in distilling columns. Here we were able to design and operate distilling columns with the determination of temperature and each of the reactants and products of both the vapor and the liquid on each plate. Then, e.g. in the esterification of butanol and acetic acid, we had already determined the kinetic factors of the reaction. Also, we could determine the rate of removal of one or more of the components in the distillation system by the vapor and liquid equilibria which we had determined already. While we had major disadvantages of evening work and low funding, we did have the major advantage of very motivated, competent men, thoroughly experienced with years of professional work. For example, in my first graduate class, the average age of the men was more than my twenty-eight years, so they had the same number of years experience—many in the same type of work they chose for a thesis.

With regard to the curriculum itself, we did quantify and put in more theory and much less of the qualitative description of manufacturing operations. We were fortunate also in those years to be able to obtain the services of top-notch engineers in the design companies like Lummus, Foster-Wheeler, and others. They came in as adjunct professors to teach our graduate students in the evening courses specialized phases of plant design, heat transfer, fluidization, and other aspects of chemical engineering. Examples were Don Kern, as he was writing his classic book on heat transfer (20), and Fred [Frederick A.] Zenz as he was writing with some little help from me, the classic Fluidization and Fluid Particle Systems (21). There were many others also.

BOHNING: What were the circumstances around your becoming department chairman in 1937?

OTHMER: I came to Polytechnic in 1932. I saw in Who's Who that Dr. Olsen was then sixty years old. So all I had to do was look at the calendar and think that with any reasonable progress, I would follow along in his footsteps and become chairman within about a half a dozen years. Of course, he aged at the normal rate! So did I, but I learned very much in those years. I learned some of the problems of our education, and also, much of the best current and future practice of chemical engineering. I continued my consulting work with Gray Chemical, to whom I licensed the process that they used. I also continued with Eastman Kodak, to whom I licensed the processes which they used for refining billions of pounds of acetic acid in removing twice as many pounds of water, as well as to Vulcan in Cincinnati and various others.

Also in those years, I worked with Hercules Powder Company and with several other companies that made solvents and other chemicals. I also had one or two consulting jobs with Exxon, then Standard Oil of New Jersey. So I had an opportunity of learning many aspects of the practical or industrial application of chemical engineering principles. In this regard, while I was head of the department—and ever since—I never felt a new man should be added to the staff until he had some five years of responsible work in chemical engineering practice.

BOHNING: What was your agenda as the new department chairman?

OTHMER: By that time, with sometimes halting agreement from Olsen, I had already changed the curriculum about as much as I wanted, except that I continually added graduate courses for our expanding number of graduate students. I don't remember any abrupt changes that were made in the curriculum or in the activities, but the curriculum was professionalized and quantified.

It so happened that that same year, 1937, we had a disastrous fire at the Polytechnic. It burned the whole top floor of our building at 85 Livingston Street. This was in early spring.

My office was on the fifth floor. At about 9:20 in the morning the fire alarm sounded. I looked out my window and saw smoke rising from another wing of the building. I had a fire escape nearby and was not terrified. As a matter of fact, I started to take the drawers out of my file of papers and correspondences and carry them down the back fire escape which represented five floors of very difficult maneuvering. I didn't get very far and soon realized that the fire would not get to my particular office. However, the whole roof burned off of this building, which may have had 15,000 square feet area, except over the small wing with my office. Later, a temporary roof had to be built below what had been the original roof. For two years, while they were building this and a new permanent roof, we were most uncomfortable.

We had a faculty meeting the afternoon of the fire and President [Harry S.] Rogers discussed the matter. By taking inventory we found that there had been considerable damage to equipment, laboratory facilities, some instrumentation, and the entire destruction of the roof. My own laboratory—general chemical engineering—had a thirty foot ceiling as this was on the top floor, a pitched roof and then above the ceiling, an attic where I had stored various materials. It also had a water tank which supplied equipment below at constant pressure.

President Rogers was a great leader and executive in my estimation, and he told the faculty that no valuable papers had been lost, i.e. no manuscripts or anything of that nature that would be impossible to replace. Only two other professors had offices on that top floor and none of the other floors had anything more than water damage.

Dr. Rogers read a telegram from the president of Rensselaer Polytechnic offering condolences and sympathy and saying if there was anything that Rensselaer could do, they would be very glad to do so. President Rogers, who had come to Polytechnic a year after I did and was therefore not an old-timer, said that he had checked why this telegram came from this particular school. He found that in the late 1890s, the main and single building of Rensselaer had burned to the ground and at that time, there was a discussion of uniting Polytechnic with Rensselaer because Rensselaer had lost almost their entire facility. This, however, had not been taken advantage of by Rensselaer because they wanted to stay where they were. So this was only a tit for a tat some forty years later.

The interesting thing of this fire was that it had started in the attic over a chemistry laboratory. Of course, everyone first blamed the chemistry laboratory for its noxious use of chemicals. However, this was a student analytical laboratory, and there were no organic chemicals. Also, it was a Monday morning and there had been no classes since the preceding Friday. So the chemistry department got out of blame rather easily. Then came the electrical engineering department for blame because that laboratory had been rewired the previous summer—mainly in the attic—by one of the electrical engineering department, Professor [Clyde C.] Whipple. (Keep in mind this was still during the Depression and everybody was doing anything to earn a nickel, and he was doing it as a summer job.) Inspections showed that seemingly nothing could have occurred from the very point where it started.

Then since chemical engineering had the adjoining laboratory, we were blamed. However, our attic was entirely safe. We never stored anything inflammable up there besides dust. So we got off the hook.

Nobody ever announced publicly what had been the cause, but it was ultimately decided that the fire itself had started due to the president's occupancy. He had a fireplace in his old office. The building with the fire was 85 Livingston (built in 1893). It was adjacent to and had a common wall with 99 Livingston (built in 1854). The fireplace chimney was an old flue with no ceramic tile lining that was in the wall of the 1854 building and then continued on up through the common wall of the 1893 building.

That chilly morning, the superintendent of buildings—a Mr. Lindholm—who had a little office next to the president's, came in and fired up the president's fireplace before he arrived to give a cheerful light. He always used cannel coal which has considerable volatile matter, and smokes. What had happened was that the fire evidently ignited the accumulation of seventy-five years or more of soot in the chimney. In turn, this heated and burned out the old mortar. This chimney had been built before the days of fire clay liners. So this chimney fire started on or in a beam evidently which rested too close to the flue in the masonry of the old chimney.

We never had a public chance to joke with the president in his search for what had happened, but this became general knowledge. Our new roof of 16-ounce copper weighed many tons less than the old tile one and lasted well for the remaining twenty-year life of the building.

A word about Dr. Rogers, who had come in 1933 from being Dean of Engineering at the University of Oregon. He was a good organizer, steered the school through difficult years, and arranged for the future move to the present much enlarged quarters—but died suddenly before the move. A civil engineer by background, he could see some faults in his peers, and once said, "If you want to hide knowledge from a civil engineer, write it in a book and put it in a library." He was a big, hearty man, a college football player, with at least one season on a professional team. He was the third of the nine presidents or temporary presidents I have served under in my fifty-five years here.

We were talking about what Polytechnic was like in the early 1930s. Some of this is well described in notes and remarks which I had given at a luncheon celebrating the golden anniversary of doctorates in chemistry at Polytechnic which I gave on November 23, 1985. I will edit a type script copy of this that I have for your use (22).

BOHNING: Is there anything in there you care to elaborate on?

OTHMER: It in general was also a light-hearted discussion of the situation at Polytechnic in the early days when I came here and for the next years.

As an addendum to these notes, this was primarily a celebration of the chemistry department as to when their first doctorate was given in 1935 to a man by the name of [Samuel A.] Goldberg, who unfortunately had a very bad accident in his thesis research experiments. One of his associates, who was working for his master's degree, was also injured in that accident.

In chemical engineering we gave our first doctorate degree three years later to a man who had studied microphotographs of cross-sections of catalysts that were used for different lengths of time in sulfuric acid manufacture. He did this work under Dr. Olsen in analyzing the changes and mechanism of the catalyst's function. This second doctorate, our first, was Joe [Joseph J.] Vetter. I knew him better than the first chemistry doctorate—who, like all chemistry doctorates for many years, were required to take a minor in chemical engineering, including my class in Fluid Operations.

I remember Vetter as typical of our graduate students and of the work load that they were carrying. I looked up his record when he was coming up for his doctorate degree. This was in 1940. In looking in the registrar's office, I found that he had come to Polytechnic in the evening school in 1918. He was working in a chemical plant in New Jersey and had to commute after work. When he first registered, he had not completed the requirements for entrance, so he had to take some courses equivalent to high school math. He then took, over a period of eight or nine years, the courses for his bachelor's degree in the evening. In the late 1920s, he was finishing off his work and I think he earned his bachelor's degree about the year that I came to Polytechnic. He then worked year after year on his master's and then his doctor's thesis, ultimately getting it in 1940, totaling some twenty-two successive years of our night school.

Meanwhile, he had become the general manager of the Mutual Chemical Company across the Hudson from Manhattan and lived in a suburban town twenty miles inland. (He helped sell Mutual to one of the big chemical companies and continued to manage it.) In his spare time, he was an amateur astronomer. He had a very practical, mechanical mind, and he ground an eleven or twelve inch glass blank into a mirror for a reflecting telescope which he constructed. Then he gave the telescope, and some money to install it, to one of the institutions in New Jersey. Joe Vetter was a very hard worker and he did all of his work with a minimum of visual ability. He always had to wear very heavy glasses as his eyes were far from being 20-20.

[END OF TAPE, SIDE 2]

BOHNING: Your first doctorate was awarded in 1937. How many doctorates had been awarded up to the time of World War II?

OTHMER: I will have to look up the number. We did have very large classes with our evening students right after World War II, due to the GI Bill of Rights. I remember one of the years, we gave twelve doctorates from chemical engineering as well as twenty-five or more master's degrees, which numbers have never been equaled since. The tendency of graduate education has changed quite considerably. In those days, our growing population of graduate students was almost entirely part-time. However, with the near disappearance now from the New York area of the executive, engineering, and scientific offices and laboratories of the chemical, engineering, and petroleum companies—all of which were expanding greatly during the war—, we do not have nearly as large a population of part-time students. Nowadays, our students, as in most schools, are full-time, and they expect and receive various salaries and aids for conducting their class and research work.

BOHNING: When World War II broke out, what impact did that have on your department?

OTHMER: As in as many institutions, we had to do more, with less. We were very cramped for space as our evening graduate and undergraduate programs were expanding. We also then took in a very large number of men for the Army. These were mainly enlisted men and, however, some officers. We had the nuisances everybody does who works with any branch of the government of having to go along with excessive governmental regulations and red tape. (I remember we had to have all drinking fountains throughout the buildings changed to be safe for the GIs—then changed again three months later to make them more safe.)

Somebody had told the Department of Defense or the Army as it was then called, that college education should be on a three semester basis rather than on a two semester basis. That means that within the months of the ordinary two semesters, one has to organize all curricula into three semesters. So we had to be teaching the same courses to our civilian students on our regular basis of two semesters and to the Army—the same courses, the same professors, different students, different clothing of course, on a three semester basis. This was indeed a nuisance. Also, one of those years, probably 1943, we had three commencements. Due to the mix-up of these semesters and classes, we had to have a commencement two times for the Army and one time for the civilian students. That was the only time in the history of the Polytechnic that we ever had more than one commencement. The normal practice has been that if a student finishes any time after a commencement—he may finish his thesis at the end of the summer—he still doesn't get his degree until the following June. But the Army boys were going off to the war directly from here—many of them in combat duty, and we had to give them their degree when they finished at these odd times of the year.

BOHNING: Had you expanded your faculty any? You said you had a lot of adjunct people.

OTHMER: We were fortunate to get topnotch people. Sometimes they had been our own people who had shown extraordinary ability and were inclined academically. For example, one was Donald Q. Kern, who was working for his doctorate. He worked for Foster Wheeler and did his heat transfer research in the company's Mechanical Testing Facility in New Jersey. He was exceptionally able. He took thermodynamics, also from the mechanical engineering department where thermodynamics then was often taught as a course in steam engines—as when I took it in an M.E. department. Don, however, had already taken our course and was so much more concerned with the fundamental aspects of thermodynamics. He asked so many questions on theory that the head of mechanical engineering, who taught the course and knew steam engines inside and out, and had written the textbook, told me never to send another of my graduate students to take his thermodynamics course because Kern had been an inquiring nuisance about thermodynamics, and not about steam engines.

Meanwhile, while he was still doing his thesis, he was also an adjunct professor and he was writing the classic book which is still in print after thirty years and still referred to constantly in heat transfer work. It is certainly the best-selling single book in heat transfer that has ever been written, and was published by McGraw-Hill (20).

We had other topnotch people as well. One other one I remember was Carl Gamer, who had received the prize for the best undergraduate thesis of the whole school in the class of 1934. He got his master's and most of his doctorate work finished while he was working for the Lummus Company, of which he ultimately became vice-president. But as to his 1934 bachelor's thesis, I said to him at about Christmas time of his senior year, "Carl, where have you been and why aren't you working on your thesis?" "Well," he said, "Doc, I have it all done." And I said, "Well, I haven't seen you in the lab." Carl said, "Oh, no. I set up a laboratory in my basement and I've got it all done. I'll show it to you." Of course, he didn't have it all done. By the time he did finish it then, he had by far the best thesis and won what we call the Raymond Prize for the best undergraduate thesis of his year.

After graduate work, years with Lummus and the war, he became one of our adjunct professors in Petroleum Refining, and Petroleum Equipment Design. Meanwhile, he had been working with the Lummus Company in their building of plants and other petroleum equipment and petrochemical equipment throughout the world. One of his jobs was organizing, designing, and building a refinery in Finland. He received a gold medal representing the highest order of honor of the Finnish government for this demonstration of the ability of Finland to manufacture its own petroleum products.

While I should like to mention a dozen such successful students, then part-time teachers, then professional chemical engineers, there isn't time—and I can only say in passing that Ed [Edward] G. Scheibel contributed as much to our department and the chemical engineering profession and literature as did anyone else. Also there was Robert B. Benenati and W. Fred Schurig. They did come into the department as full-time professors after many years of full-time professional work.

Schurig retired a dozen years ago and continued his long-time professional work of designing and building educational equipment for chemical engineering laboratories throughout the world, as he had been for twenty-five years in charge of

Polytechnic's. In 1985, he called it enough after building equipment or complete teaching laboratories in fifty universities in this country and a half dozen others, as far away as Iran.

I have mentioned Benenati, who later was head of the department. Fred Zenz was another, particularly Ed Scheibel, who later was head of the department at Cooper Union. He developed with Otto York the York-Scheibel extractor. I helped Otto organize his company for wire mesh packings. He was a graduate student, not an instructor.

These few men that I've mentioned were only examples of the outstanding young men that I was able to recruit for teaching specialized graduate courses from my own doctorates and their associates in the New York based engineering companies. This did not expand our full-time faculty during the boom just after the war and it represented a considerable savings in cash amounts, because these men were paid on an <u>à la carte</u> basis for each lecture they gave, which was substantially less than if they had been on the full-time faculty, carrying a dozen or fewer hours a week of class work.

BOHNING: How many full-time faculty did you have before the war started?

OTHMER: Not so many. Five or six was all. As a matter of fact there was some criticism as to that and a later time we did adjust and have more full-time people. But it was the only way it would have been economical to handle the load after the war years. We then lost these people by attrition as they acquired more responsibility in their regular positions—they were always young men. Instead of recruiting part-time men, I put on more full-time people each year.

BOHNING: At the time that you had five or six full-time faculty, how many adjunct faculty did you have?

OTHMER: At that time we had maybe eight or nine. Each one taught only one particular graduate course. We did have a few adjunct faculty teaching undergraduate courses. In the late 1940s we developed courses in Biochemical Engineering and in Chemical Engineering Economics on both the graduate and undergraduate level, and some other specialized courses. After the war, we also developed both a graduate and undergraduate course in Nuclear Engineering, which was another new course that was very popular for a while but then gradually fell off. At that time, I had the opportunity of obtaining, practically without cost, a very small nuclear reactor. We never could find the place in our crowded confines of central Brooklyn to do anything about installing it. It probably was just as well because it might have become troublesome.

BOHNING: During the war, your processes were used in making RDX cyclonites. Were you personally involved with that?

OTHMER: Only at the start. I was invited to Canada to give a series of lectures by the Canadian Chemical Society. The invitation came in a rather odd way and I was a little surprised, but I accepted it and did go and give the lectures. During the time I was in Toronto, I was invited to discuss some matters and got into the technology of making RDX with some chemists whose background I didn't know. A few months later, I ran into these same people in Kingsport, Tennessee at the Tennessee Eastman Company where I had been asked to come.

As I mentioned, my consulting contract had been broken in 1934 when Eastman was irritated that I had talked with Commercial Solvents about acetic anhydride. Again in 1935, I offered them a new process on which I had a patent application which again would increase the capacity of their multi-million dollar plant by an additional forty percent and reduce steam costs about the same amount.

As part of this new arrangement on acetic acid (my third with Eastman) I had become again a consultant to Eastman. I think I mentioned the difficulties of working with them because of their intense secrecy. But in this discussion in which were present the people from Canada, we discussed ordinary problems that were concerned with the expansion of Tennessee Eastman's acetic acid recovery.

I had very minor consulting work to do for Eastman. Gradually this contact died and Eastman had the right to cancel this consulting arrangement. Suddenly they did this, in about 1943. They paid me a fair fee for these later patent rights. After all, consulting with Tennessee Eastman was difficult because they didn't want to let the left hand know what the right hand was doing. They didn't even want the right hand to know the shape of the fingernails it was supposed to file on the left hand. So it was rather difficult, but I was sorry to lose the relation.

Some years after the war, I found that my later process which Tennessee Eastman had the right to use for recovering the acetic acid from cellulose acetate manufacture was an important step for the recovery of all of the acetic acid used in the making of RDX by this Canadian process. This was done by a separate corporation which Tennessee Eastman had set up called the Holston Ordnance Company, which built the plant near the main plant of Tennessee Eastman.

Oddly enough, many years later, the Department of the Army asked me to review the plans of the Holston Ordnance works using my process because it was desirable to be able to optimize processes in case it ever became necessary to make RDX again.

During the war it was necessary to have a minimum of people knowing a minimum of facts. Since by this time, Tennessee Eastman knew well enough how to build acetic acid recovery processes after the Othmer patents, there was no reason why I should continue to be involved, and I wasn't. But it was surprising to find this out indirectly ten years later and then actually to have to review the plans many more years later.

BOHNING: You weren't aware of the RDX aspect until after the war?

OTHMER: No. I had run into this concept first in Canada where I was invited and I think the main reason was to pump me to get my ideas and guesses as to costs of recovery of acetic acid—an important part of the process. This was a preliminary period in the decision as to whether and how to make RDX. Many years later the RDX process was described in an article in the Journal of the American Chemical Society which I never happened to see (23). Then it wasn't until long later that I knew that most of the RDX made during the war was made made by the Holston Ordnance Company, and the large amounts of acetic acid involved were recovered by the Othmer process.

BOHNING: You said the Canadian invitation came in an odd way?

OTHMER: I don't remember why they should have been asking me. I was a little surprised but I went.

BOHNING: You were trained be the governemtn to be an evaluator for the German chemical industry during World War II.

OTHMER: You refer to a very interesting and odd incident, the Spy School. The group I attended with was No. 58 of the Army Intelligence. During the war (it may have been sometime early in 1943) an Army colonel called from Washington and invited me to have lunch with him when he arrived at the Pennsylvania Station. There was a good restaurant there then. We met. He was sounding me out as to my availability, and various aspects of my background and familiarity with the chemical industry. He was from military intelligence. There were one or two other meetings. I had agreed that I was available, having no family responsibilities, and obviously I would be available from the school for anything that would be worthwhile in the war effort.

A telegram came one day in early June which said, "Enter the northwest entrance of such and such a public school building at such and such a location in the outskirts of Washington at eight minutes after four in the afternoon on such and such day a week or so later." It was signed by my contact. So I did show up at eight minutes after four, thinking this was all very "Boy Scoutish." Immediately on entering, I was passed to a room, asked to strip completely, and was given a GI issue of all clothes except shoes and socks. I had been advised that I would be incommunicado for about eight or ten days. I was then told not to take anything with me that had in any way my initials or other identification. I couldn't take my billfold and anything that would have my monogram on it but I was given a receipt.

There was a total of some seventeen men in this group and I saw the reason for specifying the exact minute of entrance. We were given three minute headways for entrance so that we would not see each other in street clothes. I immediately recognized another chemical engineer who was a professor at Yale, [R.] Harding Bliss, since deceased. Everybody was given a pseudonym with the instructions that we were not to divulge anything about ourselves to anybody. Each of us, however, was instructed to find out by hook or crook anything we could find out about each of our fellows.

We were the fifty-eighth group that had been on this same spy school, not necessarily all in the same place. Our group, all in GI fatigues, was loaded onto an Army truck and went through the streets of Washington, across the bridge, and down into Fairfax County, Virginia.

[END OF TAPE, SIDE 3]

OTHMER: We ended up at a delightful large country estate with a large residence which could take care not only of us but an equivalent number of staff. I think there must have been five or six pstchologists there, one or two medicals, and other Army officers. I'm not sure that the whole staff lived in this house which was a large rambling place, and some may have lived nearby. We only slowly found out what was going on. this was later written upp by one of those involved.

For the next week, we were given every sort of mental, physical, and emotional test that could be imagined and could at the same time be safe and decent. I think I was just then over some age limit that didn't allow me to take one of the tests, i.e. to climb up a tree and then run out on a limb and jump to a nearby building. We had many physical and mental tests. Most of them were great fun. there were others that were entirely impossible, at least for me.

We arose and had breakfast at 7:00, and at our particular place at the breakfast table there would be a schedule for the day for a half-dozen appointments for tests, at different points in the many buildings on the grounds. In between time we were told to do several things like prepare some letter to saboteurs in some small French town occupied by the Germans, on how to do some job. I, as an engineer, was expected to find out all I could about the place we were inhabiting and then make its map. The grounds were about one hundred and sixty acres of a family estate. I never did know the entire background. I did gather it had been the home of the Willard family—the long-time owners of the classic grand old hotel in Washington, recently rebuilt and reopened. I am always interested in horticulture, particularly trees, and I remember that about five acres of one corner of this estate was a nursery for boxwood.

Here was half a million dollars worth of boxwood bushes up to about eight feet high that had been carefully trimmed. There were tennis courts, everything for a gentleman's estate. Evidently the oldest part of the house itself was about one hundred years old. Our quarters were excellent and we had good food. There were at least a dozen buildings for various original uses.

We had an overhanging worry knowing that each was to be subjected to a third degree sooner or later. That ultimately did happen for each, and all of us were more or less nervous, each one individually, to get through with this third degree of questions with the spotlight in your eyes like a police line-up. The interrogators were behind the spotlight, asking you all sorts of questions.

At no time was one to give his identity. My cover story was that I was a mechanical engineer, which I am pretty close to, and lived in Rochester because I knew rochester well, having lived there, and I was engaged in a mechanical manufacturing company. I could be interrogated at any time by anybody of the staff as to this alibi.

As I said, it was a wonderful game. They did it in such way as to be intentionally nerve-racking. Then they would divide us up in the evening after dinner into groups and each person was at everybody else's throat to break down their cover story. The surprising thing was the amount of knowledge these men had. Somebody talked about the streets of Marseilles. And somebody immediately argued with him about the streets near the docks of Marseilles. Several men were very fluent in various languages.

One little incident was most interesting. We, individually, would be instructed by our morning program to go to different places in this tremendous estate, and there be put through a different test. One of them was in a courtyard with about a ten foot wall, and each man had two or three different tests in that particular court. I was told to be there at 9:00 one morning, and found two men there. One was an Army sergeant and the other was a psychologist with a pad of paper. The psychologist said, "Now, Othi (that was my pseudonym) you are an engineer and I want you to assemble this group of sticks." these were different lengths, say two, three, four, and five feet long about the size of broom handles. and then there were wooden balls like croquet balls with holes the size of the wooden sticks.

"Here is a small model of the assembly that I want you to make with these fifty or more sticks and these balls to hold them together in the pattern of this shape, like a tinkertoy. All of the pieces are here. I want to see how much you can do, in assembling them according to this small model. you have seven minutes. The sergeant is here to assist you, because I realize some of these things may be done better by two people than by one." There were maybe fifty sticks of these different lengths involved.

I said, "Yes, sir," and started to work. Then I said, "Sergeant, kindly put all of the three foot sticks together in one pile, and all of the four foot sticks in this one." He looked at me and said, "You're not an Army man." And I said, "No I'm not, but kindly do this." He said, "You're wearing colored socks. I don't like those under fatigues." I said, "Well, I'm sorry, but we'll talk abbout that later."' At first he was just decent in the same tone. But then as we got along, he became almost insulting in his observations, and then hhe would put the wrong sticks together. By the end of the time, we had one tenth of the whole assembly. It was impossible. Meanwhile the psychologist was making all of his notes.

I'm not so smart in psychology, but it was several minutes before I realized that it was not a mechanical test. If so, it would have been something that I would have been good at, and which I thought was my sort of skill. Instead, it was a test of keeping one's temper and finding out that the supposed helper was a contrived hindrance.

Anyway, one of our seventeen classmates, I found out indirectly, was a major in the Army. One morning, he went through that same test and this nasty sergeant snarled around with him so hard that this major, not used to being insulted by drill sergeants, let loose with his fist and knocked him out.

The purpose of my being examined was this. I'm a chemical engineer. I knew more then about chemical plants than I know now, although I have more plants to remember but less intimate experience. The idea of the military was that when D-day came in northern France, it was realized there was going to be a tough, long, hard job. The Army, in its usual and necessarily pessimistic attitude, instead of thinking the war would be over in one year, was planning for two or throo years of attrition of the Germans in driving them out of Belgium and France, then finally to Berlin. We never discussed this. This was never mentioned to me before, during, or after this spy school. It was top secret, until long after the war. The point was that the Army wanted some chemical engineers, possibly including me, to go into French chemical plants that had been operated by the Germans. This would be after they would be recaptured. We would try to extrapolate from what we could learn of the techniques and the capacities in these French and Belgian plants as to the German chemical military production capability in Germany. So we chemical engineers were chosen to be the men who would go behind the Army and to look into the chemical and other military production facilities in France.

What did happen was that the war went so much faster than the military had anticipated, or was being prepared for, that there never was any real need for these studies. By a week or two after D-day the Army strategists could see that it wasn't necessary for any of us prepared for this job to be called.

But that was one of the more interesting weeks in my life—The Evaluation of Men. Of course, the other men in our Number 58 school who were not chemical engineers—and I recognized only the one other professor, Bliss—were there for individual evaluation for many other assignments for which their individual backgrounds fitted them. Some may have been asked (or ordered if military) to go immediately behind or in front of other than existing war zones.

BOHNING: Did you have any further contact with any of those people?

OTHMER: No. I did meet casually one chap in Nebraska just in passing, who had been in another school. He also was a chemical engineer who was the man who gave me this book, of which I have a copy at home.

BOHNING: Do you remember his name?

OTHMER: No, but I have often wondered at how many men of the 58 schools up to mine—and more later—did perform, and survive, in accomplishing the particular missions for which each individually was being evaluated.

BOHNING: I would like to look at some more of your technical work. Perhaps the easiest way to do it would be in order of topics as opposed to a chronological order. Maybe we should begin with the application of some of your extraction techniques which we've already discussed. How many of those were developed? In terms of application of extraction, I have several things. One is separation of aromatics from petroleum fractions. Another is the processing of liquors from wood pulping operations.

OTHMER: In my first days at Kodak I was looking for the best way to concentrate acetic acid. One was by extraction of the acid by a solvent. That is especially good for handling quite dilute solutions, such as one to five percent acetic acid in water. In stronger solutions and depending on amounts to be handled and particularly the other materials present, possibly even those of six to ten percent, in many cases the more economic system is azeotropic distillation. But I did develop extraction processes. First, I had to develop a good extractor. One of the types I developed in 1927 or 1928 was patented in this country and various other countries by Eastman (24). It was the forerunner of other vertical types of extractors which are now used. Two or three of them were developed by former students of mine at much later dates.

This type of multistage vertical extractor violently agitates the solvent with the liquid to be extracted in one cell or stage, passing to alternate quieting cells above and below. These calming cells separate the two liquids, and each is passed countercurrently to successive agitation cells. There is a vertical series of many such alternating cells. There may be fifteen, twenty, or more agitating cells. The York-Scheibel and the Karr vertical extractors are of this type. They are well described in the <u>Handbook of Solvent Extraction</u> developed and co-edited by T. C. Lo several years ago, and soon to have a second edition (25). All of these are names of graduate students of mine.

We used this extractor of mine and others with many materials in various places. We then developed various processes which have been used throughout the world for such separations of materials which normally cannot be separated as easily by distillation, which is the classic method. Extraction is considered when the boiling points are too close and when the two liquids have different solubility characteristics in other liquids.

But you asked about the separation of aromatics and naphthenics from paraffins, the straight chain hydrocarbons in petroleum distillates. This was a most interesting development which had many side effects, including the opportunity of knowing and working with the most renowned chemist in India of the second quarter of this century.

By 1936, I had spent much time on methods for concentrating acetic acid. People interested in acetic had seen my descriptive patents and journal articles. So in early 1936, there was an inquiry on acetic from a company, Steel Brothers Limited, in London. Steel Brothers was a big trading company. Most of the business both inside and overseas, in and out of Burma, passed through Steel's hands. Burma has a small amount of oil, and

Steel Brothers owned part of that. The main product was kerosene for a burning oil, as there were few cars to use gasoline. It was high in aromatics (including naphthenes) and gave smoke, if a flame in a lantern or stove was over one-quarter inch high. Desirably, in order to produce adequate light or heat having a high flame, it should be three or four times this high before smoking. This can be obtained if the smoke-forming (less hydrogen) aromatics are removed.

Two chemists, the outstanding one of then British India and Burma, Dr. Shanti S. Bhatnagar (a great physical chemist who later was knighted to become Sir Shanti) and a Scotchman, P. J. Ward, had discovered that the troublesome aromatics are preferentially soluble, as compared to paraffins, in anhydrous acetic acid. This relative solubility varies markedly with the addition of very small amounts of water. Necessary was the design of process and equipment for a plant for the separation of aromatics which this discovery suggested.

I had expected to go to England in June of 1936 for a joint meeting of the AIChE with the British Institution of Chemical Engineers. It was a great trip over, with two hundred of the AIChE practically filling the passenger list of the Samaria, the small ship which was one of the first victims of the U-boats of World War II. Steel Brothers invited me in to discuss the matter with Dr. Bhatnagar, who also came to London. As a result I was asked, with my friends in Vulcan, to develop the optimum process and to operate a pilot plant in Cincinnati for obtaining the data necessary for design of the plant. Vulcan then contracted for and erected the plant in Burma.

The process is interesting because of the changing solvent characteristics of acetic acid in relation to the paraffins and aromatics when it is diluted successively with very small amounts of water. Thus, both the product (raffinate) stream and the solvent stream were extracted with slightly more dilute acetic acid solutions coming from successive extractions of resulting streams.

There was a total of five extraction steps; the ultimate dilution of the acid was through the addition of only the equivalent of 10% water. The 90% acid was then concentrated in an azeotropic column back to glacial grade for re-use. The maximum height of the flame established before starting to smoke was approximately one inch, and approximately 90% of the charge stock of kerosene passed this test as product. The 10% containing the aromatics was used as a boiler fuel.

Vulcan built the equipment and erected the plant in Burma. All of this had taken years, and indeed after being completed and paid for, the plant operated for only a few months before the Japanese took over Burma in their drive for India. The plant was blown up to prevent it from being used by the Japanese. Unfortunately, Steel Brothers was not important in oil, and this good process was never used again, as far as I know.

Sir Shanti Bhatnagar was a great chemist, motivator, and leader. After the war, he became Chief Scientific Officer in India, and as one of many activities, director of a program which would send two hundred students abroad each year for education in science and engineering. In about 1947, he wrote and asked me how many of these we could take at Polytechnic. I responded that in my department, chemical engineering, we could take four and probably I could get places for four more in the rest of Polytechnic.

But I emphasized that each would have to have the credentials and capabilities to carry our programs. When he wrote to thank me, he said these two hundred for the first year would be tops. They would be selected as the best of twenty-five thousand applicants!

I have never imagined a worse job than that selection process. How mentally tired I have been on each of several occasions when as one of a committee at the National Academy of Sciences, I have evaluated on each of three days fifty or so applications for National Science Foundation Fellowships. And much of the drudgery for us had already been done by a computer!

That series of Indian students coming from Bhatnagar's program melted into our large postwar enrollment and was the first wave of many hundreds of Indian students we have had since, in all departments, particularly in the Graduate School.

During the early 1950s, Bhatnagar and the Indian government saw the necessity of what has become Indian Institutes of Technology and built, I think, five or six in Bombay, Delhi, and other cities. These have become very creditable schools indeed and I have visited and lectured in those in Bombay and Delhi.

BOHNING: Did you ever have reason to work again with Sir Shanti Bhatnagar?

OTHMER: Although I never was able to see Sir Shanti during any of the several times we were in India before he died in 1955, we corresponded on several matters. I had a sprightly, possibly irreverent, secretary who in speaking to me referred to him as Sir Hut for Sir Shanti. He helped the Indian government arrange what worked up to be a billion dollar deal with American oil companies to develop the so-called "Bombay High" oil and gas field and to refine its products for India. He came to New York in completing the deal and turned up at my office unexpectedly. My secretary was somewhat flustered when she looked in my office to tell me that here he was, Dr. Bhatnagar. Then she brought him into my office and stammered, "Here is Sir Hut Bhatnagar," which was evidently the way she identified him in her mind. Suddenly, her rather white cheeks then flushed as red as her hair and she fled.

BOHNING: Did you ever have more to do with Steel Brothers or Burma?

OTHMER: By far the biggest job I ever did was in Burma.

BOHNING: But if you did not get to Burma to see the start-up of the process for desmoking kerosene as a burning oil, what else did you do there?

OTHMER: At various times over the years I had consulted with the large international engineering firm TAMS, the M in this acronym standing for Gerald McCarthy, whom I had known for years. TAMS had no chemical engineers on their staff and my first relation in the early 1940s was in the design of a large alcohol plant in Columbia, South America.

In the early 1950s, TAMS had a major contract to <u>examine</u> the basic needs of Burma, having then some twenty-three million people, and to <u>engineer</u> the needed facilities, with designs, costs and economics. The civil engineering firm of TAMS had about fifty people in Rangoon planning bridges, roads, railroads, airports, dams, and water supply, also hydroelectric power, etc.—in fact, everything that was needed after British occupation and the ravages of the Japanese conquest during World War II. This was to be at least a five-year program. Local costs were paid by Burma, while foreign costs, including salaries, were paid by our country—then anxious to establish a position to prevent Communist takeover, which had then, and has since progressed very far.

TAMS came to me and asked if I would <u>examine</u>, <u>plan</u>, <u>design</u>, and <u>cost</u> in a preliminary way what should be done in this whole country as to building and operating plants for chemical and other process industries. Particularly at the start it was desired to use indigenous raw materials so as to stop paying the high costs of imports. (Burma exported only a few items—rice, hardwoods such as teak, and some gems, especially sapphires, being the important ones.) Would I come, with my wife, and spend three years with practically no restrictions on the breadth and depth of the work I decided was necessary, and I would be given whatever assistance I needed. We would be given a mansion, left from the British, on beautiful Lake Victoria, and eight servants to care for us. I said this was quite impossible as I was very busy as a professor and head of the department with many thesis students, and other responsibilities. Also, by that time I was consulting to four or six Japanese companies designing plants, writing reports, etc.

On the other hand, I thought that with some of my former students, who had by now become experts in various basic and important fields in chemical industry, we could do a creditable job of technical and economic analysis with recommendations. I suggested that I could do a better job at less cost of time and money by having a contact in Rangoon to supply needed data by mail and working in my own office here in Brooklyn with all of the facilities and expertise which I would have available in New York.

We would go to Rangoon two or possibly three times per year and obtain necessary background data, specifications, and feeling for the job as a whole from the TAMS people there. Well, the answer was, "Yes, here is carte blanche, do the job as you see it, send us your bills, including all expenses and costs for assistance of experts you need and select, but give us your best work—preliminary plant designs, economic analyses, and recommendations." Rangoon is just halfway around the world by air stops, so each time we continued on for a circumnavigation.

For the first trip we had a few stops in Europe and India at Thanksgiving time and we were ticketed Pan Am, which became always "our mother away from home." We were flying across India, after a fine dinner aboard, to Calcutta, where about midnight we were to disembark at Dum Dum airport, go into town, and return to the airport about 7 a.m. for an Air India flight of five or six hours to Rangoon. This was the nasty part of our schedule. I was trying to sleep and Mid awakened me with a start. She was reading a new Pan Am timetable from the plane—with changes from the one of a couple of weeks before when we bought our tickets.

"Wasn't this December first," when the new timetable noted that Flight 2, the famous eastbound Pan Am flight around the world, would stop for the first time in Rangoon? Certainly we were on it, but the Rangoon stop had not been announced to the Pan Am clerk who sold us tickets in New York. The captain called ahead to Calcutta to persuade Air India to transfer our tickets over to Pan Am. Response was delayed until the Air India agent could be roused from bed. His agreement was available on our arrival at Dum Dum. (This small town is where the first Dum Dum bullets were made by encasing lead ones in a thin steel shell.)

So we immediately reboarded Flight 2. As we drove in from the Rangoon airport on December 1, day was breaking with a few drops of rain on our windshield, the last drops from the last summer's monsoon—and <u>the only drops until the next June</u>. We saw no further drops during the whole month we would spend at the Strand Hotel, on the banks of the muddy Irrawaddy river. Always, we stayed at the Strand and always we enjoyed the fine British service there. It was a fascinating experience in this friendly country—so it became from the many friends we made, particularly among the TAMS personnel.

I could talk for hours of the many good and interesting times, but I will only mention one or two which indicate so well a point I have often noticed in people assigned for long periods in a foreign land. They try much harder than those, always resident in their native homes, to relive and maintain the everyday traditions of their homeland.

The Rangoon office of Steel Brothers knew of our coming from usual contacts with TAMS. Waiting for us at our arrival at the Strand was Steel's manager with an invitation for dinner Monday night at his home. The dreadful formality of the British had been eased immeasurably in Britain during the war, as I had discovered on several trips to England in the late 1940s. Here, however, in this distant outpost, there was no letup. My new TAMS friends told me, "Black tie for men, long dresses for ladies." (This was de rigueur I was told, even out in the boondocks, if one English couple invited to dinner another English couple, even though these four might be the only non-natives around.) Mid, of course, had something along in which to look attractive; but I was told I must wear a black or white tuxedo jacket with contrasting (white or black) trousers.

I had a pair of substantially black trousers and TAMS said, "No problem. The jacket can be tailored Monday during regular business hours—try on at noon." So someone from TAMS took Mid and me at 9 a.m. Monday to the big department store. (Like all business, large stores, and professions in Rangoon, this was Indian. The Burmese have little motivation for business or other work, and like to talk, rather than work.)

We selected a length of beautiful white English sharkskin cloth, and I was measured. I went to TAMS headquarters where I was introduced around and given an office. The operating officer, John Alexander, had been a buddy at Oregon State University with Harry Rogers, then President of Polytechnic, and this was the start of a long friendship with him and Mrs. Alexander, an equally remarkable person.

At noon on the way to the hotel for lunch, I came back for a fitting. I was astonished to find four or five Indian tailors at work squatting on the tile floor of a room about ten by ten feet and sewing away on parts of my white jacket. Parts were being tossed back and forth between them, sliding on the tile floor which obviously was immaculately clean although it didn't look so. Parts were pinned together on me, as the clothes horse, and these were beautifully sewn together that afternoon.

On the way back to the hotel at about 5:30, I picked up a very fine garment. The total cost, if I remember, was $12, and that included its first washing after I wore it, since there was no time to dry it before I wore it.

The cocktail hour before dinner had no mixed drinks, uniformly Scotch whiskey until pink gin came in small glasses all around. This announced dinner and presumably started the digestive process. One gracious act of the host was to get up from his chair of the dozen drinking in the circle and crossed over to another. "Pardon me, old chap, will you exchange seats?" Once or twice the hostess also rotated her seat with a lady guest. Thus, each person was able to talk with every other. A gracious alternative to our stand-up cocktail parties.

Burma was very pleasant. There is one more incident I will tell of. It was the summer of 1953, the last time we were there, and I was overworking in the heat and humidity of the monsoon. One inch of rain falls regularly between from about one and two each afternoon, then again the sun, almost overhead, brings the humid earth temperature up to the high 90s. I didn't know the reason why my own vitality was slipping, and I found it must be something more than the awful climate.

The work was intriguing as I tried to understand the needs of the country and the ways to satisfy them, also the complete lack of appreciation of what these needs were, much less how they could be met. My library of facts consisted of two books I had carried: the third edition of Perry's Chemical Engineer's Handbook (26), (I was on its editorial board) and the first edition of Shreve's Chemical Process Industries (27). As I said in the foreword I wrote for Shreve's later editions, this was always in my travel bag, because it had almost all I needed to know of the many facets of the chemical industry.

Much knowledge was needed, not only of technology of manufacture of chemicals, but of human needs, markets, sales, and economics. The top technical man I discussed such matters with as a representative of the government was an Indian physician with a thirteen letter Indian name I never could remember. As to the country's alcohol needs—neither he, an Indian, or the Burmese drank alcoholic beverages—he suggested that for the pharmaceutical and other needs of Burma and its 23 million people probably we should plan production for 100 to 125 gallons per day. Many other indications showed the complete lack of understanding of what a program should be to accommodate the needs of this developing country.

There was a great need for ammonia for fertilizer, and I roughed out designs and costs for, among many others, two ammonia plants, small by our standards, from two sets of raw materials. These productions would be expensive because of very limited indigenous fuel. I am a capitalist, and was warned at a cocktail party at the American embassy not to repeat openly that comment including that word, by now a dirty one to the increasingly socialistic Burmese. But my economics and then my report recommended that the government make ammonia fertilizer and then give it freely to all farmers who definitely would use it.

This socialistic suggestion was good economics! With a much better terrain and climate—often two crops per year—Burma's rice production per acre was less than half that of Japan, where heavy fertilization was the rule. The Burmese government bought, at the very low domestic price, three-fifths of the rice crop for sale to other countries at <u>twice</u> the domestic price. If rice production could be doubled with fertilizer, neither the two-fifths of the current production used domestically nor its price would increase significantly. On the other hand, the sales abroad would be increased from three-fifths to eight-fifths of the current production. At a one hundred percent markup over domestic cost per ton, the government, my client, would prosper handsomely.

The report had thousands of pages, including sketch plans and many economic analyses. I never was told, but I doubt if any of these recommendations were ever implemented before the pages rotted away in files of that awful climate.

BOHNING: How did you and Mrs. Othmer stand the heat and humidity?

OTHMER: My greatest ever health problem started and gave me a scare during the intense overwork of finishing work there during the summer of 1953. I felt and could see that I was losing, week by week, a noticeable amount of my vitality and energy.

Shortly before we left that June for Japan and then Burma, we had dinner with our good friends Herman and Gertrude Poitras at their home in New Jersey. Herman was vice-president and manager of production for Pfizer. Herman talked about the good work of a former student in my classes, Peter Regna, whose team at Pfizer had developed the production of Terramycin, and only shortly before that time had put it in production. (Dr. Regna later won the Perkin Medal for this.) Herman gave me a dozen pills, irregular in shape and therefore not salable. I thanked him, put them in a little envelope in my pocket to please him, and forgot them.

As part of vaccination regulations for travel to Burma, one had to have at six-month intervals a booster shot against typhus, in addition to other shots. I had had typhus shots many times—but always without any reaction, and I said that I was immune because I had had typhoid fever—a mild case one summer while in high school. I had the Indian doctor give me the usual booster shot one noon, and by dinner time I had a high fever and was more sick than at any other time in my life.

Mid was able to reach the doctor from India who came about midnight. He could offer no remedy or explanation, except that the vaccine had attacked my immune system, which was weak. Mid remembered the pills Peter Regna had given to Poitras, then to me. The doctor had no idea of dosage because he had never heard of Terramycin, and I would not have remembered then, even the name, but it was on the envelope. So I took two and went to sleep.

The next morning the fever was gone, and I had as much life as before. Actually, I was down about thirty or forty percent in vitality and this low level was not able to combat the mild form of the disease which the vaccine had caused. But for Peter Regna's medicine, I might not have survived to find out what was causing the trouble.

My health did go down in the next nine months to about fifteen percent of my normal strength and vitality, before a sequence of doctors did decide that the covering of my heart (the pericardium) had thickened and polymerized almost to the inelasticity of a shoe sole, one-quarter inch thick leather. This minimized the pumping action. Two days after a definite diagnosis (by now May 1954) my chest was opened, and my heart was pulled from side to side to be peeled almost completely. I recovered immediately.

An SAS flight—one of our longest ever, but part-way we had berths, like a Pullman—took us in August out of Rangoon directly to Stockholm where I was to give a paper at an International Chemical Congress. It was good to see again our long time friends, Dr. and Mrs. Karl Cederquist, and to have dinner immediately at the home of the great Swedish cellulose chemist Professor [Erik K.M.] Hägglund.

Our plane, due in at 6 p.m., arrived at 5:45. We checked in the Grand Hotel, taxied to Haglund's home, and arrived at 7:15, fifteen minutes late after four or five thousand miles of travel. The Swedes are almost as punctual as the Swiss, and our hosts and twenty-four other guests from a half-dozen countries were all seated and well into a delightful dinner. We did not stay late!

BOHNING: I believe one of your interests was with the Northwood Chemical Company and the solvent pulping of wood?

OTHMER: There was a sequence of many things that led to that. I was looking for a place for my brilliant student Raphael Katzen while he had to work to support himself during his graduate years. I did get him into this little wood chemical company, charcoal but not acetic acid, which we were hoping to revitalize and rebuild in northern Michigan. The family that took it over were very well fixed financially. Although they wouldn't invest money in capital goods like a chemical plant, they would invest in research in anything in which they were interested, part of the very large income which they had from their capital investments. Over one hundred years ago the Reynolds family had built a plant to make explosives for use in northern Michigan mines. When du Pont was organizing its monopoly in explosives later in the century, Reynolds sold and took stock in du Pont, which they never sold. But every Reynolds worked hard in industries they were developing or in philanthropies, although they also had other large assets.

One of their industrial developments was this research which Katzen and I persuaded them was promising in developing a method for separation of the two important parts of ordinary wood—the cellulose of the fibers and the lignin which sticks everything together. The idea was to find a means of usual wood pulping of dissolving the lignin from the cellulose, then to separate the lignin from the solution.

Part of that work was done there in upper Michigan. Then it was moved down to our department in Brooklyn. The technique which was developed and which Katzen did patent for the Reynolds started with small chips of wood, which were boiled in methanol to which chlorine was added (28). In the boiling liquid, with small chips moving around, there was a most amazing reaction. One would see sparks or flashes of light inside the liquid, boiling in the flask, so reactive were the constituents.

This liquid then dissolved the lignin in a form of a chlorine additive product which was soluble in the methanol, and separated away from the insoluble cellulose. The cellulose, with this rather mild treatment at the boiling point of methanol, 67°C, was a much milder treatment in a relatively few minutes than the usual pulping operation. The cellulose would come out nice and white in a very desirable and high yield. Dissolved was the lignin-chloride, as we called it.

We were never able to analyze or to define its molecular structure. However, I remember Katzen did come out with a figure of something over 500 as its apparent molecular weight. It had interesting properties as a very cheap, thermosetting resin.

That was Katzen's doctoral thesis and a very successful one. He finished it in the first year of the war, and he tried to enlist. His bad eyes and heavy glasses prevented it and he went without glasses for a month trying to adjust. He almost ruined his eyes—but no success. Northwood Chemical then sold the project to Diamond Alkali Company in Cleveland, and Katzen went there. Diamond ultimately decided that with the war problems they had coming on, they couldn't devote the necessary money and effort to commercialize it. Thus they threw up their hands at the whole project, which would have required considerable development work. Katzen went to work for the Vulcan Engineering Company in Cincinnati, and in time became chief engineer.

This cellulose-lignin project I have regarded as one of the best that I have ever been associated with, but it was folded. Nobody ever got back to it, although Katzen has told me he has some further ideas. He has been working on another parallel development which he expects will be successful as I also would expect of anything with which he was associated. For many years he has been head of the very successful engineering company carrying his name, which designs processes and plants for scores of clients. Also, he has been recognized for his professional leadership among the engineers of Cincinnati, and particularly in the American Institute of Chemical Engineers.

The Northwood Chemical Company ultimately also folded. The wood distilling part of the company never was successful after the war in competing with petrochemicals.

BOHNING: You were also involved with the American Sugar Refining Company.

OTHMER: During the war I was involved in many things. There were various projects for the Army, Department of State, and so on. I did agree to devote the equivalent of one or two days per week to the development of a research laboratory and department for the American Sugar Refining Company.

There was some logic to this relationship because my graduate work at Michigan had been concerned with heat transfer in evaporation processes. I had always expected to get into the sugar industry because the evaporation of sugar solutions is the important step in processes in which sugar is purified. Thus, the sugar industry uses as large a percentage of its invested capital in evaporators as any other, maybe larger. So I had always been interested in sugar.

However, American Sugar Refining was more interested then in emphasizing the public relations aspect of research. As a consumer products producer, advertising is most important, and they were thinking of photographs of a white-coated chemist looking into a test tube of liquid for publicity, and to give an aura of scientific progress. But I was given a very nice office in the Sugar Building at 120 Wall Street which I could then and still can see as directly across the East River from my long-time home overlooking the Brooklyn waterfront. I spent two days or so per week there for a couple of years, trying to set up a research laboratory. This was difficult.

While a pharmaceutical or chemical company or now an electronics company may spend three to ten percent of its gross income for research, the sugar industry is part of the food industry which moves slowly with regard to product changes and may spend something like a hundredth of a percent of their gross receipts on research. Their processes are standardized. Their equipment was heavy and expensive, and was used without change or improvement year after year.

Here the Brooklyn refinery of the American Sugar Refining was and is two miles or so directly back of where I am sitting. It then, and probably now, can refine 25,000 tons a day from imported raw sugar, and this is one of four or five such refineries on the east coast. This plant was built by the Havermeyer family in the 1880s; and I would gamble that some parts of that equipment have been there since startup there one hundred years—as they were forty years ago. I haven't been in it myself now for forty years. At that time, the chief engineer asked me whether the design of the evaporators which were in that plant, and had been brought over in the 1880s from Germany as I was told, should be followed in buying some new ones. I disagreed.

These were war years, and sugar was hard to find and closely rationed. But with hundreds of employees and fifty million pounds a day going through many steps, there was never a case of theft or illegal diversion of sugar.

[END OF TAPE, SIDE 4]

OTHMER: These people at American Sugar were wonderful to work with, very conservative in everything, and I did enjoy my time with them. When the war ended and things could be looked at a little more calmly, they asked me to be their full-time director of research. They wanted a full-time chap in a white smock. I said that I could not do that and they did find someone else who stayed for a couple of years.

Meanwhile, I did introduce one of my students who came to me when he was out of the Army. He had a bachelor's degree, and went to work and ultimately became the manager of the Brooklyn refining. Now Bill [Wilbur J.] Buschmann has for years been the vice-president in charge of all production for Amstar, the new name of what is now a conglomerate of industries. My recommendation of him to them may have been the best thing to come out of our relationship.

However, the company did support two doctoral researches here at Polytechnic. One was the development of a process of removing impurities from raw brown sugar which is brought up by ship from the processing at the "centrals" adjacent to the sugar plantations in the tropics, and then refined in our east coast cities. The raw sugar has three to five percent impurities, which absorb moisture and make it sticky, with a tendency to cake. We found that the raw sugar has practically all of these impurities in a sticky film on the surface. For at least a century, billions of bags of substantially pure crystals of sucrose have been formed in the centrals, centrifuged from black strap molasses, a film of which clings to the surface, then <u>dissolved with these impurities together</u> as the first step at the refinery.

This film of impurities may be washed off by boiling methanol, to a smaller extent by boiling ethanol which would have been the more desirable alcohol to use. They can be taken off, and a 99.97% pure sugar results. The unfortunate thing was that a trace of some dark material—we were never able to analyze it—was there so that the sugar could never be pure white. That is almost necessary for table sugar in this country. However, for the sugar for canning food products, bakeries, and other places where the sugar goes immediately into other food products, this product would be quite satisfactory, either as crystals or as a syrup.

A second disadvantage was that the best solvent for use was methanol. Even though it has a low boiling point, and even though when dried after the washing, there is a general feeling that no food product should at any time come in contact with methanol. On the contrary, in those years many plants used benzene for extracting soybean oil. The company however, had always been conservative, and never wanted to argue with the Food Administration on that or any point.

The equipment for this solvent refining would have been only fifteen to twenty percent as expensive as that for the usual present process, which depends on dissolving and recrystallization. For any plant expansion there would have been a very much smaller investment. The energy costs using the solvent washing would have been an even lower percentage of standard costs. Also, various relatively expensive by-products, including four or five vitamins, were readily separable from the methanol solvent. However, sugar refining has been a dwindling industry in more recent years as people have cursed sugar as being a worthless food. However, it is one of the cheapest and best food products. Sugar now seems to be removed from diets of every diet-conscious practitioner.

We also did accomplish another research thesis on sugar refining. We developed a new, better, and cheaper process for the refining of sugar as presently practiced with bone char. This solid, adsorbing material removes impurities. There again, the refining company never was interested in going to the nuisance of changing plants or modification of anything. I'm not sure whether we did get a patent on that. We did get a patent on the sugar refining by use of methanol, and some years later a second (29).

Those patents were then followed in Japan and this country by further developments. In the middle 1950s a pilot plant was built in Japan. The process was proven to be very economic, but the Japanese health people said, "Here, this was developed in America, but it will not be used there probably because of the fear of the toxicity of any residual methanol, so we will not consider it." So after the pilot plant had been built and the products were shown to be just what we promised, the answer was negative.

An interesting aspect was that the three or four percent of impurities included various vitamins, and we were able to separate the vitamins and some other valuable products as well.

BOHNING: The next topic I'd like to discuss is the idea of using temperature differences in the ocean as an energy source. You did quite a bit of work on that.

OTHMER: I have been interested in this since my days in Ann Arbor at the University of Michigan, when I first heard of energy production based on the idea of utilizing the temperature differences between warm surface water of tropic seas and the cold water deep below the surface. Thermodynamically, of course, any time two materials are at different temperatures, one can take advantage of the heat which is involved in heating one and cooling the other between these two temperatures. That heat can be converted into mechanical energy.

Many ingenious processes have been developed. One of these is based on ocean temperature differences, i.e., very cold water at the depth of a tropic sea with very warm water at the surface. This was first realized one hundred years ago by a French physicist as representing tremendous amounts of thermal energy and possibly therefore of mechanical energy.

In the mid-1920s a Frenchman, George S. Claude, was then well along in years after having made many ingenious developments, including the neon sign and an early refrigeration process. He came to America about 1926 to obtain money to build a plant on the north coast of Cuba to utilize the energy which could be developed because the surface temperature of the water was about 30°C higher than the water on the ocean floor several thousand feet below. He lectured at the University of Michigan when I was a graduate assistant and I admired the rather simple equipment he had brought to demonstrate this principle at a lecture in the auditorium in the Chemistry building.

Two large vessels of water were set on the lecture table, one at a temperature corresponding to the temperature at the surface of a tropic ocean and one to that deep down on the ocean floor. A pipe connected a small steam turbine to the vapor space above the warm water. The whole system was evacuated so that ultimately this water was boiling at a very low temperature, approximately 80°F. The vapors were of very low density, but they turned the turbine as they passed on to a condenser which was cooled by the cold water passed in from the other vessel representing cold water from the ocean depths. Suitable large dial thermometers showed the temperatures. The turbine turned a small electric generator and that was connected to a small light bulb. The house lights were turned off at the right instant and the small bulb lighted up from the electric energy developed from the generator spun by the turbine, which was turned by the very low pressure steam coming at a temperature of the surface of tropic waters of the ocean. Everybody cheered.

Dr. Claude evidently did get the money elsewhere in this country and he did build the plant. Unfortunately, it was wrecked before sufficient tests showed it to be a real success, when a hurricane hit that part of the Caribbean.

In the late 1960s, the idea was revived and I had always had an interest in it because of my early interest in evaporation techniques. We persuaded wealthy people in the Bahamas to put up money to design the process and to engineer and build a small plant. It was only to be about 10,000 kW or some small size like that, and would use the warm surface water off the west end of New Providence Island to supply the warm water which would boil at a vacuum to supply the very low pressure steam. That would pass through the turbine and then be cooled by water that would be drawn up from the four- or five-thousand-foot depth of what is called the Tongue of the Ocean. This interesting marine feature is shown on maps in my collection going back several hundred years. It is a very deep channel for deep sea water extending north to south just west of New Providence Island and running over a hundred miles. This deep current is the start of the cycle of the Gulf Stream. Cold water coming down from the Arctic at the low level of the bottom of the Atlantic Ocean goes through this Tongue of the Ocean. Then it passes westwardly through the Caribbean and circles through the Gulf of Mexico, and then comes out on the east side of Florida. Through the Florida Strait it flows north to form the Gulf Stream, which Benjamin Franklin first described scientifically two hundred years ago.

This flow of water of the Gulf Stream is in great volume—over twenty cubic miles per hour. If only a fraction of the heat in the surface water could be passed to the cold water stream deep below in the Tongue of the Ocean (and even with the low thermal efficiency available from the small temperature difference) there would be enough power generated to supply the whole demand of the United States for electricity. This is quite impossible from a practical standpoint because of the tremendous amount of water, the many cubic miles of water that would have to be handled daily in order to extract the low-value energy from the surface water and discharge it at the low temperature of the water at the ocean floor.

We did make a careful engineering design from the west of New Providence island, and we even rented a small submarine to study the precipitous drop of several thousand feet to the ocean floor. One of my friends who lives in the Bahamas is a top-notch engineer, and he was generating the proposal with the wealthy Canadians who spent their winters on New Providence Island. He compressed his 6'6" height into this little eight-foot diameter submarine and went down along the ocean floor to make the profile that would be followed by the cold water pipe. As I remember, the cold water pipe would have had to have been forty feet in diameter even for this relatively small plant, which indicates the tremendous volume of water that would have to be handled in order to generate any practical amount of electricity.

This was an advantageous proposal not only because of the steep descent to four thousand feet to the ocean floor, but also because in the Bahamas we could generate and sell electricity, and we could sell fresh and distilled water which would have been condensed from this low-pressure stream after it passed through the turbine. Much fresh water is needed by Nassau and the rest of New Providence Island since there is a quite insufficient amount of water for the population and for the tourists. Unfortunately, the project never was built because just at that time, there was a political turnover in the Bahamas to a semi-socialist government. The capitalists who had backed the project decided it was too risky to have to develop the unproven technology, in an unhappy political and economic climate.

BOHNING: What was the reason for developing this? Was there a company behind this or were you doing this on your own?

OTHMER: There were several of us who were interested and talked about this. My friend, the engineer in New Providence island, had friends with considerable funds. They were the group of Canadians who had developed the Island as a tourist attraction and who owned large areas of land in the western part of the island. The company doing the engineering was a relatively small engineering firm of which I was a director; actually no large amount of money was spent outside of our own time and effort. However, it was a very dramatic idea.

At a later time, I worked further in developing this idea of utilizing hot and cold sea water in the tropics. Another group based in the Virgin Islands would also develop another value. The deep sea water is very rich in biological nutrients. Fish die, seaweed dies, biological remains settle to the bottom of the ocean; ultimately much goes into solution in the form of materials which could be nutrients for a food chain of living plants and/or chemicals.

Therefore, the deep sea water may be used further after it is heated somewhat by passing through the condensers to condense vapors which give distilled water for sale. This deep sea water with hundreds of times as much nutrients as surface water may be used to develop living organisms of various types. This requires a biological chain which would start with some form of plant life that would grow by photosynthesis. Algae are a form of single cell protein, as one of various species of single cell life. Algae then can be fed to various types of fish life, suitable "strainers" of the type of clams or oysters, which strain out and thrive on the single cell protein life.

This other experimental program which I worked on at a much later time, was in conjunction with the Lamount-Doherty Geological Observatory. A small pilot plant has been built in St. Croix, one of the U.S. Virgin Islands, to study particularly this production of food as another product from such work. The unit had only three four-inch suction lines down to the ocean floor, several thousand feet deep. No attempt was made for power recovery or for making fresh water, since with such small pipes, the cold water was warmed in coming up. Here the deep seawater was pumped into large concrete tanks with an area of five hundred square feet and a depth of three or four feet. These were open to the full tropic sun. A concentrated inoculation of the single cell organisms, algae, was added, and then in a matter of three or four hours, by the photosynthesis of the sunlight, the algae would utilize practically one hundred percent of the nutrients. The crystal clear water that had been pumped into the tanks would turn to look like a pale beer, somewhat murky due to the single cell algae which had multiplied many thousand of times. This broth was then pumped through another series of shallow tanks which contained trays on which were thousands of small clams. These bivalves, by their straining of the algae in a matter of a few hours, consumed 99+% of the single cell organisms and developed in size at a very rapid rate, very much faster than in the commercial production our east coast.

Oddly enough, no one had ever been concerned with breeding clams; very little was known of an optimum culture method. But it was found in these experiments that some clams would grow under the same conditions one hundred times as fast as others. To separate out the fast growing ones, an ordinary screen or riddle separated the small clams from the largest ones after some time of growth from the eggs or spat. The larger ones remaining then had less competition and grew even faster. The smaller clams were cut up by passing through a hammer mill and fed to lobsters so that the food cycle could continue. (Maine lobsters grew much faster in the warm tropic waters than in their usual cold water.)

The principal product would be the clams, which have been grown from eggs to market size in six months. Oysters can also be developed but most of the work was with clams. Like all animal life, the clams have excrement. The water going out from the tank for clams was then passed to a third tank. There a form of seaweed having agar or carrageenan was grown. This can be extracted profitably, feeding on the biological wastes of the clams. The water from that was returned to the ocean practically free of biological materials or nutrients. Papers describing this introduction of Power, Fresh Water and Food from the Sea were published in several scientific publications, Mechanical Engineering, Science, as well as chapters in several books on marine resources (30).

BOHNING: Has this concept been followed up at all?

OTHMER: I received patents on it in this and some foreign countries (31). I discussed it with a company in Germany which had developed a thermal intensifier system by which the warm seawater from the surface of the ocean could be heated another ten or twenty degrees Fahrenheit by passing it through some special tanks that effectively absorbed heat from sunlight. They worked with the idea, and a company in Japan did license the patent, but I have long since put my efforts to other developments. The most profitable aspect obviously is the making of food—high value shellfish. That would be possibly ten times as valuable as the fresh water, and the fresh water in many places would be ten times as valuable as the electric power produced. So, while I had been interested originally in adding the product fresh water to the Claude energy process, the food or biological aspect—the most profitable potentially—is really not in my field. So I haven't done anything further.

[END OF TAPE, SIDE 5]

BOHNING: Did this work come before or after your work with desalination?

OTHMER: Actually, the fresh water aspects were parallel with my other work along this line. I had always been interested in desalination because of my work in the evaporator laboratory with Professor Badger. As a matter of fact, I consulted with the people in the Department of Commerce when they were planning to establish the office of Saline Water in the early 1960s and then had an early contract with that office to analyze the variation of multiple evaporation which I had developed under the name of Vapor Reheat Evaporation.

Part of my doctoral qualifying problem under Badger at Michigan was to develop a system of calculation for the design of plants using what is now called multiple flash evaporation. For many years this had been used in a very crude and small way by one company in the salt industry in Michigan. I had seen such operation in the mid-1920s when I was working under Badger. He had then given me this mathematical analysis thereof as a problem for part of the qualifying exam for the doctorate. I rather enjoyed solving it and developing this system of calculation, although it was very tedious and time-consuming before the days of computers.

We followed that work in about 1960 when I developed this Vapor Reheat system wherein we eliminated condensers of the ordinary type for the multiple flash evaporation. (The vapors from each flash evaporation stage were condensed on tubes to heat the water coming to the high temperature at the top of the cycle.) Our development was open expansion of the system by condensing the water with spray condenser; I called it the Vapor Reheat system. (At an earlier time, I had worked with a type of distillation which I called the Vapor Reuse system, the invention really of my long-time partner and associate Ted Wentworth, a system for concentrating dilute solutions of alcohol, acetone, methanol, etc. This is not to be confused with Vapor Reheat system of evaporation of dilute salt solutions to make fresh water.)

In the early 1960s, I had a contract with the Office of Saline Water, about as soon as it was established, to develop a system of optimizing the variables in the calculation of design of the Vapor Reheat method. This would involve trail and error calculations for thirty or more individual stages—heat balances, material balances, etc., which would be almost impossible to do by usual calculations.

Electronic computers were just then being developed. I had no skill in their use. As a matter of fact, at that time there was no language even to talk to computers. The interface between the operator and the computer was extremely difficult. However, I had a brilliant instructor in my department working for his doctorate. Robert Benenati later became a professor, and for several years head of this department. But in the early 1960s he, as a first class theoretician and mathematician, worked with me in this contract with the Office of Saline Water. He wrote the programs and developed the interface with the computers for doing this type of calculation. Another brilliant young man named George Goulandris also did his doctorate with Benenati and myself on this particular subject and they worked out the system of talking to the computer, using the computer, and particularly in completing this design and optimizing its several variables.

We did license the several patents that came out on this to one of the companies in the equipment business, Food Machinery Company, who expected to carry the development further but they never did since they have always kept to mechanical equipment (32). But over a period of years, I did obtain several patents on variations of this process. Somebody did build such a plant using Vapor Reheat in one of the undeveloped countries, where I had no patents, and I never investigated its operation.

Meanwhile, I became interested in other aspects of desalination and worked with several companies in trying to build and sell plants using this process, but we never were able to cope with the big architectural-engineering companies like Foster Wheeler, Westinghouse, and others who were in the business, and preferred to use processes which they had developed.

BOHNING: What was the Saline Water Conversion Corporation?

OTHMER: One of these engineering companies to whom I sold the patents was the Saline Water Conversion Corporation, which was a subsidiary of the Burns and Roe Engineering Company, which was concerned principally with the building of nuclear and other power plants. The chairman, Mr. Frank Roe, a man in his late eighties when I knew and worked with him, was very much concerned with the same water problem as I had been. We did make various designs based on his ideas and patents principally, rather than mine, but we were never able to sell to any of the groups which were developing this field. (His patents on the evaporation side fit in nicely with mine on the condensing side.) I had a fifth interest in this company because I licensed my various patents to it. At a later time, for tax reasons, Burns and Roe wanted to own the corporation entirely so that they could write it off as a total loss, and they bought back my shares. With this one hundred percent ownership they were able to realize on the total loss which could not have been done for both of us if they had eighty percent and I had twenty percent.

BOHNING: I'd like to digress because you raised an interesting point—the relationship to computers. When did computers enter into the curriculum here at Polytechnic?

OTHMER: Of course, they were first tools for research—from developments during and soon after the war, transistors, silicon chips, etc. One of the most interesting and pleasant of the many consulting relations I had with major Japanese companies was the development of processes and plant for the production of super-pure silicon for the transistors for computers and other electronic usage.

Such rods of silicon are made by continuous melt-crystallization, and a beautiful plant was built. Shortly after that my good Japanese friend whom I had worked with on other processes and plants died, the small company which had been spun off by one of the big conglomerates (Zaibatsu) was melded back in, and my long time work with several smaller constituencies of it was lost.

However, let me go back to computers at Polytechnic. Benenati developed a crude language about 1961 and with Goulandris worked the material balances and design for up to thirty stages of our vapor reheat evaporator. (Dr. Goulandris became a teacher of Computer Science elsewhere, and since has graduated to a fine computer position in industry.)

Dr. Benenati, besides developing his own language to work with our state of the art (early 1960s, main frame, IBM digital computer) developed a most interesting analog computer in order to solve his own doctoral problem. So he became a recognized computer expert, teaching courses in such for other departments as well. For some years, he was the head of our Computer Center, serving all of Polytechnic. Then the Electrical Engineering Department insisted that its name be the Electrical Engineering and Computer Science Department. Therefore, the Computer Center had to be under its jurisdiction and our President, Ernst Webber, who was the President of the National Society of Electrical and Electronic Engineers, gave in to his colleagues. Dr. Benenati then returned full-time back to Chemical Engineering, although his principal publications and consulting work have for years been in computer technology in chemical engineering design.

At least several others of my doctoral students also became extremely proficient in computer, always with the help and guidance of Dr. Benenati. Always we have had the most advanced main frame IBM computer. I remember in those early days of the 1960s, I had several Chinese students—three were named Chen—all doing doctoral these requiring much computer work, in which each became highly proficient. One of these problems, on the mainframe, took one hour to run <u>one trial</u> for his particular equations. This was a very complicated heat transfer and ablation problem as to what happens to the several layers of materials on the nose cone of a rocket when it re-enters the earth's atmosphere and is melted and worn down or ablated due to the tremendous friction and heat generated.

BOHNING: What about computers as a formal part of the chemical engineering curriculum?

OTHMER: I have been talking about computers in research work in which they continue to be evermore indispensable. Now computers are not only very much a part of research but are an essential part of all technical course work.

BOHNING: When did that begin?

OTHMER: I don't think it did as an instructional part before the late 1970s, although hand calculators had replaced the ever-present slide rule. Now, however, every freshman is given a personal computer. He starts working with that computer and takes computer courses as part of the general freshman curriculum. In our study rooms and laboratories, there are consoles with which the students interface with the mainframe computer. Benenati says that his expertise with a small calculator, not even a PC and some of the attachments, gives him equal facility to that which he had fifteen years ago with the mainframe. But that is largely due to his expertise. It may be in large part due to his own capabilities having developed commensurately with that of the successive generations of computers with which he has worked. He has developed and published much software for such chemical engineering use in design of equipment and plants.

BOHNING: I would like to do another session with you if that would be possible.

OTHMER: I'll be glad to see you some other time.

BOHNING: Thank you again for a delightful afternoon.

[END OF TAPE, SIDE 6]

INTERVIEWEE: Donald F. Othmer

INTERVIEWER: James J. Bohning

LOCATION: Polytechnic University

DATE: 15 January 1987

BOHNING: Dr. Othmer, the last time we talked we were discussing some of your technical work, and I wanted to continue with that this afternoon. Specifically, I wanted to start with something that had the acronym PROST—Pressure Recycling Oxidation Sewage Treatment, which uses enzymes. Could you tell me something about how you got into that work?

OTHMER: That is a very interesting development, to me, but like three or four other developments that I have spent a lot of time, effort and money on, it never has found what I believe it merits, a substantial market outlet. I don't know whether this is worthwhile because there has yet to be a commercialization of it. The basic ideas is that the fermentation of domestic and much of industrial sewage can be expedited greatly by supplying under pressure the required oxygen (usually as air) for the biological oxygen demand. Such a pressure system reduces very considerably the time of digestion and thus the enormous tankage and areas therefore, which are otherwise involved in the use of the usual approach which takes a civil engineer to survey and lay out.

PROST makes such treatment more of a chemical engineering operation. By highly intensifying the supply of oxygen in small bubbles, particularly under pressure, and with the PROST recycling, the processing time and particularly the size of the necessary tankage can be reduced to one-fifth or one-tenth of that normally required. Variations of this process have been used, probably bordering on or within the scope of the patents which I have secured (33), and these involve deep-pit fermentations in wells down some thousands of feet. These depend for high pressure on the hydrostatic head, and therefore obtained more rapid oxidation or fermentation.

Such wells have been used in Sweden and England. The comparable system above ground has also been used in Germany in very large installations and it would be an interesting question patent-wise to investigate the relations of these several systems. So far, I have never spent the time, money, and effort to develop a licensee of the process as I would envision it, but it is a successful process and many of its features are used in several countries of the world. In Japan, one of the large chemical companies did do successful pilot plant work, but then decided some other field of expansion was more nearly akin than sewage treatment to their main chemicals program.

BOHNING: How and when did you get started in that area?

OTHMER: Like many other things, creative results are a synthesis or symbiosis with other people's work. Hearing of how somebody is thinking or something has been developed along some lines, one can think, "Well, if I were doing it, I would do it a little differently."

So in many cases, in my own organization of thoughts and developments, sewage treatment has always been a relatively unfamiliar field, although I have always thought it should be one for chemical engineers to pursue. On the study of some civil engineer's process, I could see that with the knowledge which I had in related chemical processing, an improvement could be made to do it a better way. The extra-disciplinary approach, combined with a certain motivation, often results in new methods of doing things, as shown by many patent applications which, in being prosecuted, are noted by the patent examiner to have had from one to one hundred predecessor ideas very close to, and in some cases, entirely covering the idea which the inventor himself is proposing.

BOHNING: You said it has been licensed?

OTHMER: Yes. It was licensed in Japan. Unfortunately, the company that licensed it was a chemical company which needed a small plant merely to handle their own wastes. With success in demonstration, they finally decided, after finding that it would work in their particular waste materials, that they could go out and buy a sewage treatment plant from somebody commercially. This was a good choice for them instead of carrying on the necessary development work to perfect this; they had no real desire to get into the sewage treatment business. So while they did hold it up for a while on a tentative arrangement, it never was profitable to them to carry it out merely for their own use and they didn't want to go into business for others.

BOHNING: Is there any reason why it has not taken on in this country?

OTHMER: Yes. The reason was that it would require time and effort to develop and I just never gave the necessary time and effort to it. I still think it's good, probably the most economical for populations from say twenty in a small community up to millions.

BOHNING: One of the areas that I wanted to discuss with you was an area in which you had patents about ten years ago—wet combustion of organic wastes (34). Has this been more successful commercially?

OTHMER: This process I have licensed, and license fees have been paid for many years by the principal people in this business worldwide. ZIMPRO is the name. It is a system of Wet Air Oxidation (WAO) of organic material. WAO may be used for the partial oxidation of waste liquors to make fine chemicals from paper-pulp manufacture to the oxidation of spent or overage military explosives as a means of their destruction before they become overly sensitive. By far the largest use, however, has been in sewage treatment where sewage is fermented or oxidized. In WAO, the biological oxygen demand (BOD), is supplied under pressure in the liquid phase.

BOHNING: And how is WAO applied in sewage treatment?

OTHMER: I first heard of WAO in 1948, in Sweden where I had licensed the world's oldest corporation to use patents which I had developed for the production of wallboard (35). The world's oldest corporation is the Stora Kopperberg Bergslags Aktiebolaget, of indefinite age, nobody knows, except that back in 1270 there is a record that Stora sold a large block of stock.

In these modern days of corporate restructuring, the rather complex organization of this first company in the chemical industry of "limited" liability to shareholders is noteworthy. Each stockholder had in addition to his proportional equity, the right to buy from the Stora (Great) mine his proportional amount of the copper ore mined. This was smelted with charcoal made from wood from the surrounding forests in many cottage industries within a mile of the great open pit mine. Altogether, this was a very sophisticated financial and industrial organization for the Middle Ages.

Stora is a most interesting chemical processing industry, which up through the time of our Revolutionary War supplied most or all of the copper to the European world, including North America. In this century, Stora expanded into wood and other operations in Sweden, and particularly pulp and paper.

It was in that field that I licensed to Stora, the world's oldest corporation in the chemical process, or any other industry, a group of patents which I developed for making wallboard, comparable to the Masonite hardboard. This used wood wastes rather than whole logs. My friend, Dr. Karl Cederquist, then Director of Development for Stora, had developed WAO as a means of commercialization of peat as a domestic fuel.

Oddly, I had first seen Cederquist's name on a paper on wood chemicals (36). This was in 1927, when I was with Kodak. For some reason I assumed this was the final one of a series of a scientist long publishing in this field. When I did meet him in the 1930s, he told me this paper, written while still in college, had been his first paper of a very large number. He was then, and still is only some five years older than I am. In his several interesting posts he was for years Chairman of Chemical Engineering at the Royal Institute of Technology in Stockholm.

Sweden has no gas or oil and practically no coal, but it does have large amounts of peat. Peat is usually found a long distance away from other, all more valuable, fossil fuels. It may have 90% to 95% water, chemically combined, and it is a slimy, nasty material with which to work.

When I was Falun, Sweden—for 700 years the home of Stora—demonstrating my wallboard, Karl Cederquist showed me that by heating peat to about 200°C under pressure, the peat would lose its colloidal properties, which makes it impossible to separate by any mechanical means, the water from the solids. It then may be centrifuged to give 45% to 55% solids and an aqueous liquid containing salts, sugars, etc. from the partial oxidizing treatment.

The solids may be pelleted and dried, to be used like coal. This much was known. However, Cederquist found that by blowing in oxygen, as such or as air, he could combust partially the peat material and supply the heat necessary to raise it to 200°C.

On a large scale, it is impossible to heat the peat otherwise, even though it is 90% or more water, because the slimy colloidal material sticks to any heat transfer surface. So he heated it under pressure in an aqueous medium and pumped in air which oxidized some few percent of the peat and gave the heat necessary to raise to 200°C.

The Patent Office in Washington could not conceive oxidizing (burning) under water a part of the fuel to supply heat, and held this was quite impossible. Cederquist asked me if, when I returned to the States, I would go and talk it over with the Patent Office in Washington. I did this, and in due time he got his patents because I testified to the patent examiner that I had seen it done (37).

Shortly thereafter, this was by now about 1950, I met another chemist, Dr. [Frederick J.] Zimmermann, also a member of the Chemist's Club, an excellent organic chemist. He was oxidizing wood pulping liquors to make vanillin and other fine chemicals, by heating these liquors under pressure with added oxygen or air. He had tried to oxidize, or did oxidize with other oxidizing agents, but found that the simplest method was merely to blow a carefully controlled amount of oxygen or air into the liquors at this higher temperature and pressure.

The odd thing was that Zimmermann, living and working in the pulp and paper industry in Wisconsin, and Cederquist got their patents separately, basically conflicting with each other, at about the same time in 1951 (38). Sterling Drug Company, a maker of fine chemicals, thought this WAO, what was called the ZIMPRO process, might be used not only for the making of vanillin and other fine chemicals, but also for general use as an oxidizing tool in synthetic organic chemistry.

So Sterling bought the process and established ZIMPRO, Inc., some time in the early 1950s. Stora made a deal to stay out of the business which ZIMPRO then developed; and by now, 1987, has built some 200 plants, most all of which supply the BOD of sewage and many industrial wastes.

One chemical use has been the reactivation of spent wet activated carbon by WAO of the undesirable organics which have been absorbed. This is simpler, much cheaper than drying, burning out the absorbed solids and wetting again.

Also steam power often can be generated by WAO more cheaply and efficiently from peat, some lignites, and coal, high in water or sulfur, than by a usual boiler with modern appliances. Many organic sulfides may be reduced, and overage military explosives destroyed by WAO.

The contribution which I made was to improvements in the several accessories of the heat and cooling media and equipment, for use with WAO and also a system for doing it on a smaller scale by a batch or semi-batch operation instead of the continuous process which both Zimmermann and Cederquist used. These ideas and a half a dozen covering patents were licensed to ZIMPRO, who have paid me royalties for a dozen years. I also licensed to ZIMPRO a modification of this process wherein the wet organic material, after it had been stabilized and lost its colloidal properties, could then be subjected to gasification under certain conditions to make syngas which would be used in any one of the numerous places where syngas is used for high volume productions (39).

Probably the most valuable of these patents, however, is one providing for the desulfurization of coal, the development of which has been financed by the Department of Energy (40). Sulfur, especially elemental and inorganic, is selectively oxidized from coal (lignite, or peat) by WAO. This may be done at the mine mouth if the high sulfur coal is to be piped in a slurry. Or the high sulfur coal may be <u>completely</u> oxidized in a WAO to generate steam much more efficiently and cheaply than under a boiler unit with modern accessories.

BOHNING: While your ideas on this go back to the 1940s, are your patents more recent?

OTHMER: The last patent and the one which ZIMPRO then reproduced on an aluminum plate with a fine compliment to me on my 80th birthday, was much more recent, only about five years ago, and that is the one on production of syngas as a downstream product of the wet oxidation process. The basic theory of the wet-air oxidation (WAO) indeed makes a very interesting and valuable process.

WAO has been important in the disposal of waste, including for example the safe combustion of overage military explosives. As far as I know, WAO has not been used for the synthetic chemical aspect which Sterling Drug had originally thought worthwhile and for which reason Sterling Drug had bought the ZIMPRO patents, technology, know-how, and organization. Another interesting item in the history of the chemical industry, Sterling Drug, in late 1985, decided after forty years that WAO and waste disposal was not in their field of small volume, high cost chemicals, pharmaceuticals, etc. So two years ago, Sterling sold ZIMPRO to, of all groups, the Michigan Technical University, a state university, up in the northern peninsula of Michigan.

BOHNING: Michigan Tech?

OTHMER: Yes, a Research Corporation of Michigan Tech is the present owner and operator of this corporation which does, I suppose, $100,000,000 of business a year and certainly must have been profitable in its over forty years of chemical processing, mainly of sewage. It has a very high reputation, and is an engineering organization of very high capabilities and achievements.

BOHNING: The next area I wanted to cover was pipeline heating. You have, again, done a number of things outside of classic chemical engineering.

OTHMER: Yes, pipeline heating was also out of my field, and it depends on a phenomenon of electrical technology—the inductive heating of a steel surface which is carrying alternating current (AC) by the same AC flowing in reverse direction through a conductor, parallel to and relatively close to the steel surface. Here I developed an improved invention because of my review of the technology developed by an engineer of a corporation that I had consulted for on other matters, and involving their use of this co-called "skin" effect of electrical heating.

The point was that even though I'm not an electrical engineer, I recognized in my review of this patent that the patented processes they had developed for utilizing this so-called "skin" effect of inductive heating would not sustain serious competition unless major improvements were made to plug holes in their patent coverage.

Unfortunately, the involved corporation, or the inventor himself, with the typical inventor's arrogance (I know because I am an inventor), refused to take the suggestions freely offered. I was told in substance, "If what you have is something you believe is worthwhile, take it yourself and do what you want with it. The inventor says there can be no improvement." The result was that I did file and obtain without difficulty a patent, and offered to my friends at no cost but my cost for obtaining the patent (41). The other inventor said, "No one can improve what I've done." And his management did not wish to dispute his position.

I did find other people to license the patent—and subsidiary ones—and they have paid me royalties on one hundred installations or so over a period of many years until the first patent expired.

There are other worthwhile subsidiary or daughter patents which I obtained also. These basically were for heating fluids or semi-solid fluids, tar, sulfur, semi-plastics, heavy oils, so they could be pumped either in or around processing plants, or in long distance pipe lines. One example might be a two-hundred mile pipeline handling a very waxy curd which, if shut down (no pumping heat of friction) someone said would become "the world's longest candle."

Other aspects of this invention and of other later subsidiary patents which I received are still in effect. I have never had the time or energy to develop all of these. They include the prevention of ice formation on cables of bridges, like suspension bridges, or particularly of cables of high tension lines which sometimes are overloaded and break when sleet ice forms on them.

Another such benefit will be the de-icing of third rails for electric trains, where sleet ice formation occurs and prevents the brushes against the third rail giving contact after the shut-down for a few minutes of the train at an intermediate station. Sometimes within a matter of several minutes the ice formation will prevent the starting of the train due to ice preventing contact. Another application is a reduction by 50% of the amount of copper that is required for electrical conductivity in such low-power usage as domestic wiring.

These ideas—some patented—have been demonstrated, but again, I never had the time or energy to develop them industrially. This last is not so important as it looked some years ago when a government survey said the last of the earth's economic copper ores would be used in this century. However, satellite and radar for airborne telephonic communications, along with optical fibers, have reduced copper needs manifold, and now comes cryogenic superconductivity with materials other than copper.

[END OF TAPE, SIDE 1]

BOHNING: But there are patents if you want to play with them.

OTHMER: Well, present ones have only about three or four more years to run. Others are not filed as yet.

BOHNING: In these last two areas, where you developed patents that you didn't pursue, did you have students working?

OTHMER: No. These are only "gray" area patents because I'm not an expert and, at first, knew almost nothing about their fields before I became involved. I merely can see that somebody else's processing work could be improved by some modifications, when they are almost self-obvious. But they also can be considered "gray matter" patents because they are only developed by the gray matter of the brain in reasonable study rather than by an experimental program. However, they represent very interesting problems to solve. Some dozen of such patents have been prosecuted in the patent office by me.

Thus, I have made the drawings, I have written the applications, I have had no help from anybody except the typing, and then I have argued the case with the patent examiners. I have had a great deal of respect for my treatment by many examiners, both in correspondence and interviews in their offices. Sometimes they have been very helpful to me in their profession, and with ability in the technology involved. In other cases they have been jealous, antagonistic, and anxious to capitalize on the slightest blunder of inexperience. There have been two or three of the latter.

One Chief Examiner kept me waiting outside his fine office in the old Commerce Building for a half hour past our appointment. Ushered in, I noticed the fine furniture, desk, and many feet of empty book shelves. The half dozen books I did see —two or three were the only ones on the desk, one half opened face down—were gaudy paper-covered adventure novels.

He had no valid defense to my argument but in forty-five minutes justified to himself my trip to Washington by changing one word "vaporization" to "evaporation" in several claims.

The patent examiner represents that division of the government which is the eternal negative. "No, no, no, no, no!"

is their eternal and infernal answer to every positive suggestion that is made. One has to come back at them with pages and pages of arguments. This makes it a very interesting game. It offers a greater feeling of accomplishment and interest and enjoyment than, let's say, playing chess.

My good wife, Mid, has many times said that she was a patent widow because I would spend my spare times for some weeks rebutting <u>ad nauseum</u> the argument of some examiner. Sometimes I lost, of course. However, I have on occasion gone to the patent court in Washington to argue my own case there. That has been interesting because I found there was no place for an applicant. There are attorneys' lounge rooms, attorneys' entrances and so forth, but no place for the applicants, because it is almost unheard of for the applicant to represent himself in these cases.

I have had the great experience, twice in one year as I remember, which no patent lawyer with whom I have talked can duplicate. This is the allowance of all claims, twenty-five or thirty in one case, within six months of the filing date, and without the necessity of an office presentation.

BOHNING: I was going to talk about this later, but why don't we do it now since you started talking about your work with patents. How did you develop this ability? Did you ever have anybody do your patent work or did you do it from the beginning?

OTHMER: I came into the patent field starting in 1927 when I started to work with Eastman Kodak and its wonderful group of patent attorneys and experts. Kodak, probably more than any other company, has been based on secret know-how and many patents. Often patents seemed preferable in their development of Kodak's monopoly, but Kodak now (May 1987) will pay dearly to Polaroid for the court held infringement of Polaroid's instant photography.

I worked with Kodak's patent department in filing many patents in the first eight months after I came to Kodak, no less than two dozen foreign and domestic patents on the processes and equipment which I was developing for azeotropic recovery of acetic acid. Naturally I learned much from these experts as to their work. After I left Eastman and while I had my own lab in Rochester, I developed a major improvement in the process and then I found a patent attorney there. I worked with him in developing the subsequent improvements and processing which I later then sold back to Eastman. Again, when I came to New York, for my first several patents I worked with an attorney, who unfortunately then died.

Later, with his former assistant, after three or four cases, I found that I would spend less time writing the patent and making the application myself than I would in arguing and demonstrating and educating the patent attorney in the field of my interest. This saved friction in these arguments and discussions and saved time, so gradually I came to do it myself. Always a fair draftsman, I found that the figures were no problem. Ultimately, I have for many years now developed my U.S. patents. This has been with the aid of the MPEP, the Manual of Patent Examining Procedures, the thousand pages of rules revised once or more a year. This, like all of the Patent Office practices, is not written to half the public. Rather, it is rewritten to the Examiners, to kill them in every possible way to discredit an application.

One is not able to carry on such work in any other country so I have had to depend upon foreign patent agents. Thus more recently, in a domestic legal case in which I was an expert witness, the opposing lawyer attacked, "Some citation says you have over 150 patents. However, the patent office lists only 87? Isn't there quite a difference between what you and the government say you have?"

My response, "The 150 is actually the minimal number because it includes foreign patents. I don't know how many foreign patents were filed by Eastman Kodak, and I don't know how many foreign patents I have filed. I have never counted up how many foreign patents I have filed myself, but I thank you for checking in the patent office that I have been granted 87 U.S. Patents, as I had never totaled this exactly."

BOHNING: When you arrived at the patent office to argue your own case, how were you received?

OTHMER: Courteously, but of course with a little surprise by the tribunal of three experts. These were not three outsiders but three patent examiners, aside from the examiner who was my opponent in this exercise. He, again, tried to discredit the invention, and I was trying to prove the novelty of the invention. The disadvantage in such a case is, of course, the fact that the three examiners who are the judges are associates and pals of the man who is the inventor's opponent, which may give a certain amount of bias against the inventor.

In one of these cases, my opponent was the examiner in the case referred to above. I had absolutely no respect for him as either an engineer, a scientist, a professional, or even an employee of the United States. He kept me waiting on another case when I traveled to Washington to call on him. Nobody was there or had gone in or out of his office. With covers up, where he had laid it down on his desk, was a paperback novel he was reading. So I had no respect for him.

BOHNING: But otherwise you were reasonably successful in prosecuting your own patents?

OTHMER: Yes, I have been reasonably successful. As I said, sometimes I have lost. Probably one out of eight or ten, I have missed, which is not so bad.

BOHNING: In those cases where you did lose, did you attempt to pursue that?

OTHMER: Always. I fought until I lost completely. Not always did I feel that I should have won. I argued to a point of my agreement. In some other cases I felt that it was entirely unjust to lose. But that, of course, is the normal reaction of any loser.

BOHNING: Well, let me move on to the area where your results were on experimental work, as were most of your patents. One is the affinity series of metals. When did that work begin?

OTHMER: My interest in the affinity series of metals started about twenty years ago and was the result of ideas that I developed in discussions with Dr. Gerhard Holland of Frankfurt, Germany, and some developments which he had underway with a Dr. [Rudolf] Nowak. Oddly enough, soon after I became involved in affinities, I found that Charles Toth, one of my previous students, saw a first minor publication and got in touch to say he was developing processes for aluminum production in this country which would depend on affinities. I then worked with Toth and assigned patents that I developed to his organization.

The theory and practice is the result of relative affinity or tendency of gaseous chlorides of metals to react at high temperatures with oxides of other metals. This relative affinity varies remarkably at different temperatures.

This represents an affinity series, very much like the electromotive series of metals involved in aqueous solution, wherein a noble metal may be replaced by a less noble one. The development of this theory came about from the work of Drs. Holland and Nowak from studying the production of titanium oxide and titanium metal by the reaction of chlorides of other metals. Thus the oxides of iron, aluminum and silicon are all more stable than that of titanium. The chlorides of any of these three metals react with the oxide of titanium to give titanium in the $TiCl_4$ form. This then may be converted to titanium.

High pressures and particularly high temperatures are involved, up to the possible limit of various materials of construction for industrial plants. Incidentally, in this it was found that some chlorides, normally not thought of as a useful material in anything but aqueous solutions, can be used as a high temperature refractory under some particular conditions.

The technique, the organization of this series of materials of metals, is a fascinating theory which then in its understanding and practice has not had recognition in the design and/or the understanding of processes for the industrial winning and separation of many metals which are rather difficult to obtain.

BOHNING: How many different metals?

OTHMER: In our early work we ordered some eight or ten at two different temperatures. Later we placed in order some thirty metals (also carbon and sulfur) for chlorine, also a bromine affinity series in relation to oxides, at temperatures of 700°C to 1800°C.

BOHNING: The major reason for the studies is the separation of the metal from the ores.

OTHMER: The development of this thermodynamic ordering was to allow better understanding of existing processes for separation of metals and to predict new processing techniques for the separation of the metals from ores and from each other by high temperature gaseous reactions.

BOHNING: Iron, also?

OTHMER: Iron was one of the first studied practically by Dr. Nowak, the chief metallurgist of Dr. Holland. Nowak developed and patented a process based on much careful pilot plant work. This process seems to be more economical and otherwise preferable with some ores than the standard blast furnace (42).

BOHNING: Are there any licenses on this process?

OTHMER: I think not for the iron. What I have been especially interested in has been the thermodynamic understanding of this order of affinities.

Meanwhile I can foresee useful processes coming from this understanding. I do have four or five patents which I obtained on the application of this theory to winning aluminum, and several more on winning titanium, also titanium oxide from otherwise difficult ores (43).

I did make (with Dr. Nowak and two of my graduate students, Mr. [Lutfi] Durak and Mr. Trivedi) what I have since thought was a poor presentation of this theory and Durak's results for some thirty or more metals at ten different temperatures to the Metallurgical Society of the American Institute of Metallurgical Engineers. (This was never published because the reviewers said the presentation and conclusions were merely the placing in order of thermodynamic values which anyone could calculate!)

The second graduate student, Trivedi, carried the theory farther, but I have been too busy with other things to get it the recognition which I think it deserves. For example, it does explain exactly what was happening in a quite novel process for smelting zinc, using the relative affinities of it and iron for oxygen and chlorine. I spent much time and effort on this development some thirty years ago, but someone else received the patents first, without discussing the theory or its generalization.

Going from a bench scale through a pilot plant is so expensive and was something I never spent the time or money or effort to develop for any of these metallurgical processes. I think this is a great technique, both from the scientific standpoint and from the extractive metallurgical standpoint, and I believe a few hundred thousand dollars over a couple of years' work would be spent profitably.

BOHNING: And what were your relations with your friend in Germany?

OTHMER: Dr. Gerhard Holland is quite an entrepreneur. He had been introduced to me by another one of what the Germans ten years after the War called the "Wunderknaben," that is the <u>brilliant boys</u> who made great successes after the War. I had several matters with Hans Zimmer, one of the most interesting and dynamic men with whom I ever worked.

Hans Zimmer was interested in acetylene and I was a third partner in a process for its manufacture with John Happel and Len Kramer, one of his students at New York University. John Happel had received his doctoral degree under me in about 1950 and then became Chairman of Chemical Engineering at NYU. The process for making acetylene from natural gas was developed in the early 1950s.

Acetylene is a wonderful intermediate for organic synthesis, but unfortunately we never got our process off the ground. Acetylene usually uses one less step in synthesizing a compound, and is considerably more efficient than ethylene in producing any of the many compounds which are made from either. Hans Zimmer had read an article which I had written in 1955 (44). He called me from Frankfurt and said, "I need an acetylene process and I'd like to come over and talk to you. I can't be there tomorrow but can be there the day after tomorrow. How's that?" So I said, "O.K. I'll be here."

So he sat opposite me and told me his story, a very interesting historical development; he is a fascinating man. His hobby was high speed motor cars, and he had been a test pilot for testing Messerschmitt fighter planes during the War. After the War he decided to go into the chemical industry. He had some background and quickly accumulated a great staff of devoted chemists and engineers. He said, "Now, I've been in this about six or so years and I had set out to beat Du Pont at nylon and synthetic fibers." I said "Well, how have you done?" He said, "Every year after the first year, I doubled my staff and I doubled my gross." I said, "That would be two to the fifth or sixth power." He said, "Yes sir, that's it." And he had done just that.

He wanted us to develop and build him a pilot plant for acetylene production which we did and shipped over by air for his operation. Unhappily, his people pushed by him too hard for results, burned out the pilot plant before useful results, and it was never rebuilt.

I visited him several times during two or three years, and his organization looked like a Mid-western State University campus. He had his chemistry building, his engineering building, his pilot plant building, his machine shop, and so on, with big green lawns in between. This was on the northeast side of Frankfurt. An impetuous, imaginative, highly motivated excitable man whose way to relax, so he could get back to sleep nights, was to read cookbooks.

From time to time, Mid and I were guests in his magnificent home in the Taunus Mountains, fifteen miles from Frankfurt. Hans had his Zimmer AG group of 2000 people, a great process development, design, and construction engineering organization, one half technical, one quarter of these Ph.D.s. They were building processes and plants, largely for synthetic fibers, around the world. He and I formed a 50-50 company, P.P.M. AG to develop my patents in water-desalination, but he never lived to get this off the ground.

On Saturday mornings a group of his top people, all of whom worshipped him, came to his home and worked out under a physical culturist. Hans then led a trot up a mountain path, and dropped dead from a heart attack at 57 years.

Mid and I were enroute via London to see him the following Wednesday about the acetylene business, and had dinner Tuesday night with other friends in Frankfurt, Dr. Dieter Behrem, head of DECHEMA and ACHEM, and family. I called at 7:30 Wednesday morning (he was always in his office by then). His secretary recognized my voice, said, "Yes, Dr. Othmer, we do have that appointment for 10 this morning, but that is the time of his funeral—in the great machinery assembly hall." Zimmer made great contributions to chemical processing, particularly synthetic fibers, in developing as well as industrialized countries.

Hans Zimmer had introduced me to Dr. Holland, with whom he was working on various aspects in photographic technology. Holland was developing several inventions on the side, because principally he was a banker. His doctor's degree was in business management. His engineer's degree was from Darmstadt. But he was mainly interested in developments in the fields of, at that time, optics, and then with his outstanding metallurgist, Dr. Nowak, he was developing a process for making titanium, which depends on chlorine affinities.

I worked with him off and on for years trying to sell the process. We would have sold it either to the Japanese or to somebody in this country, but Holland, who was primarily a banker, reasoned, "I have put a million dollars into this development, and therefore somebody who is going to go into the next step must put a million dollars up front, and then we will go ahead on some arrangement." But his philosophy never succeeded in getting it off the ground. I think his patent will expire before he does license this process for titanium.

That was how I happened to study the thermodynamics of the titanium process, and then I made some other developments along these lines. With Nowak I published a paper in Chemical Engineering Progress (45).

One of my former students, Charles Toth, had been wounded in the Hungarian uprising in 1957. He came to this country in 1958, and had been in my classes here, but had left and gone down to New Orleans to take a job.

He decided to develop an idea that he had had in making aluminum, using a chlorination step, quite different than the classic Hall process which had been used since the 90s of the last century, almost 100 years.

So when Toth saw my publication on relative affinities of metallic chlorides and oxides he called me. I then consulted with this company for some years, but he turned out to be a better stock manipulator than chemical innovator, and after ten years the stock I took as payment still can't be sold. His program has been one continual escalation of stock operations and business in India, Switzerland, Costa Rica, Brazil and then back to this country, and with companies that are always getting started and so far never succeeding.

His technology has been an interesting commentary in metallurgical possibilities. Now, however, he has produced aluminum chlorides from clay, and this may allow the use of immense reserves of clay in the country to make aluminum, instead of depending on imported ores. (I think his original idea came from an old German patent, never used, of about 1890 which I later happened upon.)

BOHNING: Let's move on to something else.

OTHMER: One item occurs which will fit in somewhere. I happen to remember something about Walter Lucius Badger who, in 1924 to 1927, was my professor at Michigan. Then he was one of the outstanding chemical engineers of the country. He had a master's degree from Minnesota in about 1911 in physical chemistry, and he said he learned chemical engineering from manufacturer's catalogues. This is an interesting way of saying how really practical he was, particularly compared with the highly mathematical concepts of physics which are so important in chemical engineering education today.

Another isolated footnote to chemical industry may go back to our discussion about patents. One company that I worked with in New York was the work of a very competent and motivated chemist, Dr. [Vaman Ramchandra] Kokatnur. Kokatnur was an Indian who had come to this country in the 1920s and had a large number of patents, probably twenty, in each of two fields.

One of his developments, and this was the reason he contacted me and we worked together, was in so-called azeotropic distillation.

He used azeotropic distillation, or more simply steam distillation, not as I did for separating volatile liquids, but as a means of removing water in organic synthesis processes, where the other materials are non-volatile. (So often water is the by-product of a desired reaction, and his removal of it by steam distillation speeded up the reaction of the mixture of chemicals.)

I had students on their master's and doctor's researches working with him. They worked on more or less informal fellowships on these process developments. Usually kerosene was the withdrawing agent to steam distill out the water. We independently pursued this idea; in fact, it was a simplification of azeotropic distillation as I had developed for continuous production of esters and acetamide.

Continuous processes were developed for nitrating toluene, producing phenol, an especially desirable form of anhydrous caustic soda, etc. No patents were filed; I was too busy with many other things, and I don't know if anyone in this country ever used it. However, years later, after the War, I was told in a German company that they had used the toluene nitration process. Similarly, in Japan, a company used the caustic soda process.

Kokatnur called his company, however, Autoxygen, "self-oxygen," and that was because the other system that he had many patents and inventions in, was the development of both new or old organic oxides which would be useful by themselves, or could be used in the further synthesis of rather complex compounds. Sometimes he combined the two of his fields.

I mention him as an introduction to tell of a rather interesting dinner which he also went to with me. It must have been in the late 1950s, and I never knew why either of us were invited.

[END OF TAPE, SIDE 2]

OTHMER: It was given in celebration of some anniversary of the patent office or some celebration like that. Probably 800 or 1000 people were invited to this gala dinner and it may be they were chosen if they had 50 or so patents or something like that. I don't know. But both Dr. Kokatnur and I had invitations and we went down together to this celebration.

People in the patent office spoke and there were various other interesting activities and displays. The menu was after the manner of famous patented inventions. For example, the cocktails were Manhattan cocktails, each one put up in a little specially blown bottle, probably holding about four or five ounces, in the form of a snake coiled in a helix. His head and open mouth came up as the neck of the bottle, which was corked with a plastic cherry in the top, since you put a cherry in a Manhattan cocktail. On the smooth front of the bottle was a gold paper, a reduced facsimile of some old patent which had been granted long before—for whiskey, some ounces to be taken as a snake bite remedy. Everything else was also named or in the form of some famous invention. Thus there was an "Incandescent Salad." This was a half of an avocado, filled with slices of grapefruit; on the top was a brilliant maraschino cherry. This incandescent salad was in the shape of an incandescent light bulb and the red cherry was the light. It was quite an affair that I, of course, will never forget. So much for that.

BOHNING: What about the Othmer plot?

OTHMER: I think I mentioned that shortly after I had left Kodak and while I had this laboratory in the building of the American Chemical Products Co. in Rochester, the father and son, H. O. and Ted Wentworth of Cincinnati, Ohio, called on me. They were engineers and builders of distilling equipment and plants. Their company, the Vulcan Copper and Supply Co., built copper equipment for the chemical industry. Several years younger than I, Ted Wentworth was the class of 1928 at MIT.

We have worked together on many things since. First it was acetic acid, and the plant discussed above at Roulette, Pennsylvania. In the middle 1930s of the Depression years, corn was so cheap it couldn't be sold, and was used as fuel in Nebraska to heat houses. Also it was converted into alcohol to make gasohol to be used as motor fuel. This started in Nebraska, Iowa, Illinois, the corn belt out there in the middle 1930s, just a little after prohibition was repealed.

Thus in the late 1930s, the need for a better system of making absolute alcohol to blend with gasoline became apparent. My friend Ted Wentworth developed (I was a consultant, but it was his development) a new system of making absolute alcohol.

This was a modification of the azeotropic distillation, which I had been doing with acetic acid and which had first been used for alcohol shortly before the turn of this century by Professor Sydney Young in Dublin. The new system involved the use of pressure in order to get reasonable results with a new entrainer liquid—ether. The distillation had to be done at a high pressure and we did not know how high the pressure should be for optimum conditions. We ultimately decided it was about 150 pounds per square inch. Meanwhile, I tried to develop a means for predicting this maximum pressure by differential calculus, the ordinary determination of the point where adjacent differentials are the same.

In an attempt to evaluate this, I tried various geometric plots; also algebraic and arithmetical systems of listing columns of variables and of their differences so as to determine if any successive ones came out to be the same. This study showed that when values of pressures of two compounds are compared when taken at the same temperatures, the differential of the logarithm of the vapor pressure of any compound is always a straight line function of the differential of the logarithm of the vapor pressure of any other compound. The slope of the plotted line was then found to be the ratio of the latent heats of the two compounds, which is a constant for all temperatures, and lines pass through the origin for all compounds.

This is a very simple method of plotting the vapor pressures of any compound to give a straight line. Hence an equation with only a single term gives the vapor pressure, temperature, and latent heat relations exactly.

More useful, however, was the integral form. At the same temperatures, the logarithm of the vapor pressure of one substance is equal to the logarithm of vapor pressure of any other substance times $\underline{m}$, which is the ratio of the latent heats, plus the integration constant. This gives a simple relation, log $\underline{P}$, the pressure, is equal to $\underline{m}$ times the log of $\underline{P'}$, which is the vapor pressure of another compound at the same temperature plus $\underline{C}$. That was a very interesting relation because it put vapor pressures as a straight line function with only a single constant and the ratio of the latent heats. We developed, by using critical values, a variation which would eliminate the arbitrary constant and use only the constant, the ratio of the latent heats.

However, I found that while this was a very interesting law, and indeed a most valuable relation, it didn't solve my problem of maximizing the pressure for this distillation. In fact, it never did give me the answer for which I was searching, that is the optimum pressure to operate this azeotropic distillation.

Naturally, I considered that one doesn't just happen onto the discovery of some valuable relation and then tell people that you <u>accidentally</u> bumped into this new phenomenon. Instead, you write out a brilliant syllogism of mathematics and physical chemistry—after the fact—and develop this all out from the axioms and say, "What a bright boy am I to have developed all of this," That, of course, is exactly what I did.

So sixteen printed pages of a full-dress thermodynamics paper showed how simply and accurately one can develop a means to correlate and predict vapor pressures and latent heats of vaporization of all substances, based on, of course, the familiar and basic equation, which Clapeyron had developed just a century before (46). Also, in this single paper, there was shown the rigorous tie-in with a dozen related physical chemical properties; that is freezing points, heats of fusion, dilution, hydration and chemical reactions, entropy and enthalpy charts, vapor-liquid compositions, etc.

I presented the paper at a meeting of the American Institute of Chemical Engineers in Buffalo in the Spring of 1940, I believe it was. At the same time I presented, with Ted Wentworth, a paper on the system of producing absolute alcohol by the process which had been his development in which I had helped in minor amounts, but in which I had not found the optimum pressure that would make it the most economical. However, it was far more economical than the other system and it is, of course, still in use and in plants being built with some more recent improvements in this country and abroad.

One of these plants to produce 25,000 gallons of pure absolute alcohol per day Wentworth started up only a few years ago, and produces the impure grade for gasohol at a lower cost than the other processes.

Now, this thermodynamic development turned out to have so many facets that I used it as a playground for my students for years in developing many other relations that could be idealized in this same straight line function. Over a period of thirty or forty years, we published more than fifty papers on this thermodynamic relation and the various aspects which it may use to relate, correlate and predict many different thermodynamic functions.

To me the interesting aspect is that it shows so many properties of matter indeed are related to each other and particularly to the exact variances of any of a score of properties in all materials to those of each other. Some of these properties, in addition to those mentioned, are solubilities of solids and liquids also, particularly of gases in liquids as controlled by temperature and pressure: viscosities of liquids and gases, adsorption of gases, liquids, and solids in liquids and solids, surface tensions of liquids, total pressures of binary liquid solutions, relative volatiles, activities, liquid densities, boiling point elevations, azeotropic temperatures and concentrations, equilibrium constants for hydrocarbons, critical properties, osmotic pressures, Henry's law constants, gas volumes, equilibrium and rate constants for chemical reactions, diffusion coefficients in gases and liquids, humidity charts, equation of state of gases and their mixtures requiring no empirical constants compared to eight to twenty or more in usual ones, etc.

Other people have used this reference substance technique also for such things as determining the flow point of melted glass and similar other out-of-the-ordinary variables and quantities. One of my Japanese friends sent me a slide rule that some Japanese company had put out to use this principle in picking off vapor pressures of any compound.

Other relations have developed to correlate at least twenty or thirty different physical properties of solids, liquids and gases. One was that of my brilliant student, Dr. Hung Tsung Chen. He developed an equation of state for gases which had only two or three constants as compared to the eight to twenty constants in other equations of state. This new equation can be used rather accurately for determining or correlating gas properties. Of course, the simplicity and ease of use of these methods of interrelation, correlation, and prediction of data are of less value now, when one can plug into a computer an equation with 20 or 30 constants, usually empirical. (Of course, those large numbers of constants may not have been determined as yet! And sometimes determined from hardly more points of experimental data than the number of constants).

BOHNING: How were your students supported when you used this as a playground for their thinking for a long time?

OTHMER: With regard to thesis research, when I came to Poly it was the rule of every department in this school that every bachelor's candidate had to present before graduation a thesis, a bound thesis. That policy continued until after the War when we had so many students and didn't have a staff that could devote the time to individual problems for everyone. However, always of course, there was the master's thesis and the doctor's thesis. All of our graduate courses were in the evening (I think I have explained that).

If a part-time evening student, therefore, was working in an engineering firm or in one of the big chemical offices, laboratories, engineering departments or what, I always asked, "Can you do a thesis that will be related to your work?" I didn't say, "I have a job for you to do," or, "This is what you're going to do." I said, "Would you find something of interest? You will spend a lot of time on this research, a lot of effort, a lot of your motivation. If that can be devoted to something in which you have an interest, or particularly if it is part of the problem or work you are doing with your company, you will be very interested, and feel you are getting a double reward." I don't know whether I discussed this with you before. Anyway, that was always my philosophy. I always gave the student that choice. It was always, "What would you like to work at? Have you any choice? Are you working with a company? If so, on what?"

Usually, the student was delighted. In one case, one of the very big chemical companies with offices in New York, employed one of the men who was working for a doctoral degree. He had his master's degree, and he was doing top notch work, of course, since he himself was top notch with great motivation and capability. He wanted to develop the processing of different materials and separating steps for making some organic chemical, in which the company was interested. He had to get the approval in writing from his boss, on which I always insisted.

The boss replied to him, "You can't have Dr. Othmer work with you on our problem. He is a smart guy and he works with many students and knows many companies. How can you tell him enough so that he can help you or know what you're doing." To counter this, I proposed that I become a consultant for the firm on this particular item alone. Of course, I've been a consultant for many firms and I have no trouble with keeping secret our relations and their knowledge. So I said, "I'll be a consultant for your firm for $1.00 a year during this thesis period." Thus for two years while he was working on his doctoral thesis, they paid me $1.00 as a consultant to him on his particular problem. Incidentally, we never published the results. They never wanted it published.

However, you asked me quite another question. In those cases where we had student fellows, or I did have a little money coming in from Esso, now Exxon, I doled this out to some needy students rather sparingly. However, if the student was going full-time, that is, he didn't have any company that he was working for, I would quite often let him take one of these thermodynamic problems for a thesis so that was often the basis then of a paper. This had no cost for experimental equipment.

As a matter of fact, this one that I hold in my hand is a copy of This Week's Citation Classic, the output of the organization of Dr. Arnold Thackray (47). The paper is "Correlating Diffusion Coefficients in Liquids." This was one of such problems. It was selected for publication as the Weekly Citation, having been most referenced by other chemical engineering authors, and then selected for inclusion in the bound volume containing a selection of important papers in chemical engineering.

It was not the candidate's doctoral thesis. Instead, I often used ideas or thoughts in this field of "Correlating Data" as a part of a doctor's qualifying problem so that quite often the student would work this out as part of a doctor's qualifying problem and then we would publish it. I'm trying to remember. I think there were two of these correlating papers that were actually doctoral problems. One was by Hung Tsung Chen in which he developed a comprehensive equation of state using these techniques to develop an equation of state without empirical constants compared to the usual ones with eight to twenty empirical constants requiring very much data to evaluate (48). There may have been another one or two, actually doctoral problems, and there were several master's problems.

BOHNING: I want to ask you what the journal citation is on the one that's in that Weekly Citation.

OTHMER: You take this. It was published, as most of these were published, in the old Industrial & Engineering Chemistry, although they were also published in Japan, in Spain, in Germany and in France, all in different languages and they totaled a very large number in Industrial & Engineering Chemistry. As a matter of fact, when the old I&EC closed down in about 1960 or whenever it was, one of my students went through my bound volumes. I have a complete file here in my library, from the first volume starting in 1906. My student found that I was author or co-author, with my students, of 120 articles in the volumes, starting in 1928, one paper on a part of my doctoral thesis and one describing the theory and first model of my still for determining the "Composition of Vapors from Boiling Binary Solution." This was the largest number under any name in that journal's history.

As a matter of fact, while we're talking about old times, in 1928 I remember being in Washington and calling on, in the Mills Building, the editor of Industrial & Engineering Chemistry, Dr. [Harrison] Howe. We talked, I think it was, about the publishing of my doctoral thesis. Dr. Howe was then, and was for many years, editor.

BOHNING: I did want to talk about the Encyclopedia of Chemical

Technology. That's an area that I have a great deal of interest in and I would like to go right back to the beginning. How did that originate?

OTHMER: During the War years, in the early 1940s, Dr. Raymond Kirk was head of the chemistry department and I was head of the department of chemical engineering. Each of us had our offices on the fourth floor of the old building at 85 Livingston Street and, of course, we talked often. Out of these conversations came the idea that there should be an Encyclopedia of Chemical Technology to take the place in the English language of Ullmann's Encyclopedia in German (49). This had been available for years.

We discussed this off and on and didn't get around to doing anything during the pressure of many jobs that each of us accepted during the war years. Then another came into the picture—Dr. [Eric] Proskauer. He and other associates each had a lifetime experience in publishing and had their doctor's degree in chemistry, and at least Proskauer had written one or two books on chemical solvents (50).

They were interested in having their corporation, Interscience Publishers, publish just such an encyclopedia. Their experience in publishing and their large knowledge of the field, the markets for such a book, and particularly of the people in the field along with Ray Kirk's knowledge of people in chemistry made the proposal sound like good sense.

Ray was a very easily met, friendly, sociable person whom everybody loved. He had many contacts. Ray was not an outstanding scientist; that is, he never published a large number of papers, but he had a great empathy for people. That made him a very good person, a capable person for the job of getting people together who would do the writing, because obviously a first essential was that there be a very large team of authors.

It must have been late 1945 or 1946 when we organized this idea of the Interscience publication of the Encyclopedia of Chemical Technology. Interscience hired an office, the back of a second floor front office (I believe it was some attorney's) in a small building right across the street from our buildings at 85 and 99 Livingston Street.

Our first meetings were regularly once a week. One or more afternoons were devoted to general planning. It was decided soon that what we wanted was a comprehension of our title of Chemical Technology broken down into possibly 1,000 articles. That number, incidentally, was not far off what actually developed in the first edition.

So we went into this project which would take so much time without any formal contracts or even understanding of the relation of either Ray or me to Interscience. We really looked at it as more of a total public service to our profession than an occupation for any kind of gain.

[END OF TAPE, SIDE 3]

OTHMER: As to this number 1,000, we wrote out lists of titles, and for weeks we discussed back and forth while studying what would be the 1,000 titles, as nearly inclusive and not overlapping too much. Anyway, we were organized by late 1945 and the first volume was copyrighted in 1947 (51). Yes, I forgot, but I see the copyright was made out to the Interscience Encyclopedia, Inc. and books were distributed by Interscience Publishers. I had forgotten that there were two corporations. Both Interscience Corporations were merged into John Wiley and Sons after the first edition. Ray and I, with Proskauer and others, did accumulate first the list of topics; then we importuned the 1,000, slightly fewer authors. The first volume did come out in 1947, pretty much as we had planned.

BOHNING: Let me interrupt for one moment. Could you say something about how Kirk was to work with when you were selecting these 1,000 topics. How long did it take you to come up with your master list?

OTHMER: We had a list after five or six meetings and we tried to refine the first ten or fifteen percent of that carefully and immediately, for the first couple of volumes. The items ahead of topics assigned were always being worked over, and topics added or subtracted as we saw what was already <u>done</u>, the text of subjects and volumes as they appeared. Our list ahead was continually being refined as it was being shortened by articles going into type.

Obviously, we realized that we need not plan everything <u>a priori</u>, since we also estimated that what we were after was a total of some 15 to 20 volumes, each of about 1,000 pages 7 1/4" x 10 1/4", wide margins, large type, excellent paper and binding. We did have to pick out titles and authors for the first volume or two definitely, of course, as soon as possible and then had to pressure the authors for manuscripts. Ray spent most of his efforts on author persuasion and the rest of the work was surveying a sort of <u>terra incognita</u>. Fortunately, we realized that all didn't have to be planned, perfected, and published overnight. We hoped to get about one or two volumes a year and work each article and page over carefully as we came to it.

So that's what we did. We obviously had to sharpen the focus on the first effort, the first volume, and then came the second volume more routinely. However, we did do better than we expected. Volume II, I now see, was copyrighted in 1948.

You see, we were working on those two volumes pretty much. We then, in addition, got as assistants Anthony Standen and Janet Scott (I see her signature is not on that volume) as hers, Standen's, Kirk's and mine are on every other volume of my personal copy of the First Edition. Both Tony and Janet had considerable technical ability in the fields of chemistry and of publishing.

So the staff increased as did the rate of production. For example, Volume 10 came along copyrighted in 1953, you see. So we were then doing better than a volume per year, and that continued.

Ray Kirk was in his early sixties. When he reached the retirement age of sixty-five, he planned to become the full-time Editor of the Encyclopedia. Unfortunately, Ray died after we had gone through the first fifteen volumes and the fifteenth volume which also included the Index to the first fifteen. By then we had seen that we had neglected some things which had fallen between our general topics. Also, some developments had been made during the period of our recording work, so we had to get out two supplement volumes. The first supplement came out in 1957 and that was just after Ray Kirk had died, unfortunately but suddenly.

I then wrote a slight memoriam in the first supplement volume. He died very suddenly and so he never, then, had the opportunity of carrying on as the full-time editor of what we, and Wiley, realized was going to be a continuing operation.

After he died, and without discussion, Interscience paid his widow and me each what amounted to somewhat less than $1,000 per volume for the work which we had done in organizing and editing this first edition. The corresponding Spanish Edition of sixteen volumes was entirely gratuitous on our part (52). It had to await the completion of the English edition.

It was a very successful venture from the standpoint of Interscience. It was also a very good part of what Interscience had to offer in selling out to John Wiley, who bought out Interscience out at just about this time.

So the second edition was then carried on by Wiley with Tony [Anthony] Standen as the Executive Editor (53). Now he was the official Editor, a title which had been reserved for Ray, although he and Janet Scott had done so much of the actual work for the first edition. Janet Scott retired before the start of the Second Edition.

The second edition then came along at a much greater speed because it was under the aegis of John Wiley, and a larger staff of people was put on it. (Index to 22 volumes and Supplement, 1972).

Dr. Martin Grayson was the senior editor and with him was as Associate Editor, David Eckroth for the 3rd Edition (54). We had an editorial board established of Herman Mark, Othmer, Charles G. Overberger, and Glenn T. Seaborg. The Index to the 24 volumes of the third edition and the one volume of its supplement was published and copyrighted in 1984.

Since that time and in December of 1986, Dr. Martin Grayson left Wiley and will be in another position.

One additional point that was interesting, to which I did not agree entirely but, of course, I had no say as to the business end of this, was the publication of a condensed edition, smaller printing, double column pages and otherwise the so-called <u>Concise</u> <u>Encyclopedia</u> (55). In this, every article has been condensed so that the whole is in a single volume about twice, I would say, the wordage of any single volume of the edition.

The usefulness of this, to me, will be for those companies that may have one or more complete sets in their main library, but then may have scattered condensed volumes around where people can see. Then, if they want they can call to see in the main library what they want.

Many companies, of course, have many copies. One time, years ago when I was involved in a very major patent suit in the Federal Court in Wilmington and asked to use the library of the Du Pont Office, they asked me if I would kindly sign the first volume of the Encyclopedia. I said, "Oh, yes," which I've done in many cases, and I signed that and then they brought out three more first copies. They had four complete sets.

In the Argentine, many years ago, the library only had at that time, the first and second volume of the first edition of the <u>Encyclopedia</u>. These were so worn. I never saw books in a library that were more worn. I should mention, however, that that was in English, but there was also a Spanish edition published later. It came along but not until the first English edition was finished and that delay was interesting. I had unconsciously thought that Volume I, Volume II and Volume III would follow the English along in Spanish, but, of course, that's entirely impossible because Spanish alphabetization is quite different from English. For example, a word like <u>streptomycin</u>, is <u>e</u>streptomycina in Spanish, so it would have to come under an e rather than an s.

We had many problems with late authors, which is always hazardous to an editor's temper. I remember one particular one. We had asked somebody at Celanese to write the article on Acetic Acid. It never came, <u>it</u> <u>never</u> <u>came</u>, IT NEVER CAME. And of course "Ac" comes pretty early in the alphabet. Actually it should have been on p. 138 of Vol. 1. We had to put it under

Ethanoic Acid. So, on p. 138, Vol. 1, under "Acetic Acid" we said "see Ethanoic Acid," and it appeared in Volume 8 p. 386. For reasons which appeared logical, about half of the articles on compounds related to acetic acid appeared under A in Vol. 1 and half under E in Vol. 8. (As far as I know, no subscriber was so irritated at the Editors' frustration as to cancel his subscription to the Second Edition's remaining volumes.)

The new edition will probably not have its first volume out until late 1989 (56). A certain time in between is necessary, I suppose, for sampling the market and judging how it will sell again.

BOHNING: I'd like to go back again to the very early days of this Encyclopedia. You said that Kirk brought Proskauer to you and introduced him to you?

OTHMER: Kirk knew Proskauer well. I should have introduced Ray Kirk to you. Up over my clothes closet he is in facsimile, a life-size bust in plaster. In fact it is the original, from which the bronze was cast. It is a wonderful likeness of his powerful yet kindly face and the bronze for which this was made is upstairs. They were going to throw this plaster one out and I said that I would like to have it looking down from up there.

BOHNING: Herman Mark knew Proskauer before this time. Do you think that's how Kirk met Proskauer?

OTHMER: Mark came to Poly in the early years of the War. I don't know when Ray Kirk first knew Proskauer, but the chemical community is not too big and they got together somehow or other. I don't know if Proskauer and Ray ran into each other through Herman. Herman, of course, is another very sociable animal, as Ray was. Both loved everybody and everybody loved each of them and so both knew everybody.

BOHNING: I'd like to come back again, if I may, to the early topic selection. How did you and Kirk work together? Did you work smoothly together in terms of making those topic selections and picking authors for your topics?

OTHMER: I'm sure that we didn't each one start out with complete lists and then compare. I'm also sure I never went through a complete list of 1,000 topics in my own mind. I probably started listing 20 or 30 unit operations, the same number of unit processes of chemical engineering and then every organic and inorganic metal, alloy, compound, etc. in commerce. A chemist would start more theoretically in listing materials, lab methods, equipment, etc. Other people threw in their titles, cards were made, assembled, regrouped, etc.

And, of course, we then worked it all out, cut and try, depending much on authors' suggestions and criticisms of classifications, interrelations, etc. We obviously had many suggestions from authors.

The basic idea was that this would not be a dictionary, in that it wouldn't have 100,000 definitions, and we set out with the idea that we would probably be able to encompass the 100,000 or one million definitions into articles showing their interrelation. Some of the articles are unduly long and some of the articles are unduly short.

I can't help but give the same comment I make about every book—you open it up and <u>you find what you find</u> between the covers. That is the only way you can learn the contents. Some articles are very comprehensive in our work, some even are almost treatises on some field. Others give insufficient coverage. Obviously, we had to rely on the recommendations, the judgment, and then the results of the people we asked to do the work. The basic sin is to give a general answer to most readers and a comprehensive start for research into the literature for the specialist.

BOHNING: Did you do any editorial work on the books?

OTHMER: Oh, yes. Ray and I were listed as, and were, the editors of the First Edition and we did much of the horse work on the articles. We had other people.

Interscience hired a couple of people almost immediately, with offices across the street from Poly in back rooms, but then they moved to another building where we had the equivalent of probably a half a dozen offices with a dozen people working on the Encyclopedia as the show got bigger. Obviously, to start out with you're very much interested in just the first volume, but then you expand your field and so on and so on. But your point is well taken.

All of us, Proskauer, Kirk and others, did sit around these afternoons. We had our first meetings in Ray's office. We did kick ideas around and who we would ask and so on—topics, possible authors, contacts of each, desirable length and comprehension of articles, inter-relation to others, etc.

Part was done by telephone and part by correspondence, and we called on friends for suggestions, writings, criticisms. We had remarkable help from practically everybody. I regarded our efforts as donated <u>pro bono publico</u>. Everyone felt anxious to cooperate in the same spirit. Almost always, the one asked for an article (at very low per word compensation) or any other suggestion or help, regarded the request as a compliment to his professional stature and responded in that vein. Usually companies accepted such requests generously as a compliment to their expert, with a by-line, including their own name!

Moreover, we soon found a system for much of the job. Obviously, considering the chemical elements and materials, the compounds, the uses, the physical properties and so on, gives a framework to start with in listing articles. Then you start with different operations of chemical technology, all of the unit operations of chemical engineering such as distillation, evaporation, filtration, and so on—absorption, adsorption, mass transfer, fluidization and thermodynamics, quantum theory. Some of these things just come off the top of your head and there is no trouble to write down the first fifty, eighty or one hundred of such topics and then you just fill in. And of course, when you get four or more people together, kicking around a list, you build it up to what you want. Then you have to start paring these things down and putting more of the individual items into more inclusive items.

BOHNING: What were Standen's and Scott's background? You said they had a scientific background. Were they chemists or chemical engineers?

OTHMER: Both obviously had much publishing experience in the chemical field. Janet Scott then, did leave, and of course I had nothing to do with the business arrangement. She became part of the Webster Dictionary Division with regard to chemistry and related things. Standen had written a couple of books. One, I remember, was on the humorous side. "Science is a Sacred Cow," where he wrote 150 pages with his tongue in his cheek about the sacrosanctivity of science and how we bow very low and sometimes adulate science without full recognition of all its implications (57).

BOHNING: And Wiley had taken over?

OTHMER: No. Interscience had offices on Fifth Avenue at someplace in the 20s, the second or third floor, as I remember, and we met there a couple of times. Interscience Encyclopedia itself had an office for a couple of years in one of the high rise office buildings, only 100 yards from the old Polytechnic.

BOHNING: Did the change-over from Interscience to Wiley affect your participation in any way?

OTHMER: No. As a matter of fact, Wiley then negotiated a deal with me (Kirk having been deceased) with regard to the second edition volume, but they call it the Interscience Division of Wiley. I don't know now how long that is going to continue, or not. But this division has other books. They have the Plastics Encyclopedia which also comes from Interscience and in that case, as you indicate, from the relation with Herman Mark (58).

BOHNING: You said you started without a contract.

OTHMER: We never had a contract and I just don't remember any discussions or business negotiations ever. I wouldn't have expected it from Ray, but looking back at myself, since I had much more contact with business arrangements than Ray had had, I am a little surprised. Looking back, all I can think is that I felt the Encyclopedia would be a good thing for the profession and I would like to help make it available.

Ray had, as I say, the idea that when he reached sixty-five, which could be more or less at the end of the first edition, he would retire from Poly and work full-time for Interscience as Editor-in-Chief. I don't know whether he knew whether Interscience was going to be part of Wiley or not. I never did. But unfortunately he died.

BOHNING: Was the first edition finished when he died?

OTHMER: My obituary was in the first supplement volume, of which there were two for the first edition, and practically all of the work of the second supplement, and of the Index was completed. He died in 1957.

[END OF TAPE, SIDE 4]

BOHNING: What kind of reception did the Encyclopedia get when it first appeared?

OTHMER: It got an enthusiastic reception. I never have known anything about the number of volumes sold, but I always imagined that for the second or third edition, it must have been in the range of 8,000 to 10,000 sets. But that's just a guess. I never have had any indication. It was none of my business. I think probably the usual academician, when a third or second edition goes by, just doesn't take any more royalties even though it may be selling many times as many books. I just don't know.

BOHNING: They certainly have maintained your name and Kirk's name as the banner on the Encyclopedia.

OTHMER: Yes. People don't call it the Encyclopedia of Chemical Technology. They call it Kirk-Othmer or they call it K-O. I remember one time in Yugoslavia, somebody called me in the hotel and said, "Is this Dr. Kirk Othmer?" This was years after Kirk was dead. And I said, "Yes, this is Othmer." And he said, "Well, Kirk, I wonder if could I meet you?" He as many others have thought that Kirk was my first name.

Kirk was a very much loved, very personable, very social person in this country, particularly in academic circles. I never knew him to do consulting work, although he may well have done so. During the First World War he was active at the Chemical Warfare Service, but otherwise he was always in academic pursuits. Overseas his name wasn't known. But I have traveled millions of miles overseas in twenty-five or thirty-five countries, and people have known my name. It's a wonderful handle—Kirk Othmer—a wonderful handle.

For example, as I indicated, many, many times, going into a chemical plant either with a group of visitors from some convention or independently otherwise, they can't show me this, they can't show me that or they can't show me the other thing, always secret. There has always been an electron microscope. I'm talking about thirty years ago, when the electron microscope was new. "Oh yes, here's our room with the electronic microscope." Well, that had all been sanitized before I'd come in, but I could look at the machine and recognize the proud boast of ownership.

But then we would get to the library and they would bring out the first volume and ask if would I sign the first volume of the Kirk-Othmer. It was always a great handle to conversation. And, of course, I've always been unhappy that Ray never lived to enjoy it. Ray was at least ten years older than I. He would long since have been out of it anyway.

BOHNING: I had some closing questions that I wanted to ask you that are somewhat general in nature, as opposed to the specific ones we've been talking about. You've been a member of a number of organizations for fifty years or more and one of those is the Chemists' Club. Could you say something about your association with the Chemists' Club and what it was like fifty years ago when you first joined.

OTHMER: Well, there are several differences, of course. One thing fifty years and seventy-five years ago, before that, before I knew it, the Chemists' Club was indeed more a chemist's club. The fact is now I don't now know that we have a single active member who is an academician or actually works in a laboratory.

There were originally many professors and laboratory chemists or chemical engineers.

For example, we have the Bogert Room, which is the main dining room, named after [Marston T.] Bogert, and he was president of the Club in 1908. But he was the last professor, i.e., a card-carrying professor, to be the president of that club until 1974 when I became the president. Not one in over 50 years, and I was not known widely as a professor but better for extra-curricular activities. Of course, there have been none since.

I mentioned in this book here about the Citations, i.e., my former student John Happel, who had been Professor and Head of the Department of Chemical Engineering at New York University, before New York University merged with Poly. He is an Emeritus Member. I have never made a survey, but even among the Emeritus Members (I'm a Life Member), that is the old timers, I don't know of anybody else who is an academician.

The point is a club membership is now a "perk" or a fringe benefit for a chemical salesman or a chemical executive. I would guess that 80 or 90% of the membership dues and other fees at the club are paid by companies. Of course, an academician, as you know very well, doesn't have any perks or fringe benefits that would begin to take care of such. I don't think that there was such a difference between professors and people in industry when the club was founded or up until the First World War or after the First World War, as exists now, but I guess it's that perk. I think probably in those days, very few of the people had this fringe benefit, but that's the system now.

For example, here at the Polytechnic, my predecessor as Head of the Department was Dr. John C. Olsen and John C. Olsen got his doctor's degree from Johns Hopkins. He was an analytical chemist, but he was a member of the club and when I came in 1932, he introduced me to the club and I became a junior member and was so for years.

Then I built a home in Pennsylvania where I engineered a plant, which I have mentioned before, for acetic acid, and Pennsylvania became my legal address. The saving of New York State income tax was the principal reason. Then, since I was a legal non-resident, I became a non-resident member of the club. Somebody came up with the idea after this had been going on for some years, "This guy Othmer, he's got a residence and he spends more time in New York than he does in Pennsylvania, and he has an office at the Polytechnic and he ought not to be a non-resident. The dues are about half as much." So somebody said, "We don't like this."

I said, "Well, O.K. I can't afford to be a resident member because I can never get over there for lunch. It is too far and I always have a home where I can walk to for lunch. So if I became a resident member I would be thinking every time I paid my bill, how few lunches have I had there and how many should I have had because I just never had gone there so much. So I said, "However, I'm a practical man. I'm going to live to be 100 so I will be a life member." Oh, yes. The resident fee in those days was $100 per year and the life membership was then, and still is, ten times the annual fee. So I sent them a check for $1,000 and said I will be a Life Member. Nothing happened. This was probably in October of one year, but I forget which year. I guess I went to the club from time to time, I don't remember about billing. But anyway, sometime after the New Year one of the members whom I knew called me and said, "Let's have lunch at the Whitehall Club," a club down by the Battery, to which he belonged. He was chief sales director for Esso Petrochemicals. We had lunch and he said, "Gee, our friends would like you to be a member but they don't like this idea that you've never been an active member."

I said, "Well, I read the Constitution and the By-Laws and it doesn't say anything about having first to be an active member." He said, "Yes, but everybody else has always been an active member for about ten years or something like that before they become a life member."

Well, I said, "That's my decision, because I'm too far away to come often for lunch, but I expect to live to be a hundred, so I can divide a life payment over a lot of years." So they made me a Life Member and took the overdue check for payment.

The transition between what you might call a chemical executive and chemical peddler's club and an academic or professional chemist's club is considerable. The Chemists' Club building originally had the top four floors as laboratories rented to chemists. As consultants, they would have a private laboratory and office there and worked independently or with one or several assistants.

Actually, as the scientific aspects of chemistry, or the chemical aspects of chemistry became less important for members, those laboratories disappeared. Also, important programs of research are now done by teams, heavily financed by big corporations in large installations. The last laboratory I remember was ten or fifteen years ago, and after that all the spaces have become offices. So the Society of Chemical Industry and two or three other associations have offices in the building. The four or five floors below the laboratory-offices have been hotel rooms for the use mainly of non-resident members when in the city.

And as you know, just as of this last month, the Chemists' Club building has been sold. Now, the plans are for the Chemists' Club to have the lower part of this building for fifteen years, up through the dining room and the library. Fifteen years will be the agreement and the upper part will be taken over by the new owner. The Club will be paying rent and the upper part will be taken over by a new owner who, by the way, is a dentist, who is going to inaugurate some sort of a health spa. Nobody seems to know what the new program is going to be, but we have fifteen years and we will have enough money to pay off debts and, hopefully, to see us through these fifteen years.

Meanwhile, while we are talking about the history of the Chemists' Club, I should note that the only way it has been able to survive since about the time when I was president in the mid-1960s, has been by taking in other old city clubs who also are in financial trouble. City clubs have always been luxuries, and, as such in the gradually increasing socialistic tendencies of our social and political and economic life, they are being phased out.

So city clubs are becoming very expensive for their decreasing number of members to support, and are being phased out. Thus have gone the Banker's Club and the Lawyer's Club, the magnificent establishments on the top of the two of the big buildings down in lower Manhattan, and many other clubs. While I was president, we took in the Engineer's Club when it went under. This was on a tentative arrangement which never worked out. (I believe that my presidency was the last which left without a deficit—due, however, to several gifts to balance the budget.)

Then we took in the National Democratic Club, which had given up its building some time before. That union was interesting; most Chemists' Club members are rather conservative and the idea of bringing in Democrats en masse was disturbing. Many of our members thought they would recognize Democrats immediately by horns on their foreheads. On the contrary, we get along and live with them, and they were just as likable as any Republicans.

Now, more recently, there have been one or two other clubs, the Brown Club and the Squadron A Club, and now in the last year, the Mining Club. This is the one which is most nearly akin to the Chemists' Club and probably will be the best supporter of the Club. Anyway, it has become a little bit more cosmopolitan in many ways, more sharing just the same niceties that we always had. In fact, we would have long since gone broke if it hadn't been for these other clubs, all originally with their own establishments, which had to be given up in this changing economic climate.

BOHNING: You said when you came here your department chairman introduced you as a member. Were there many other members from Poly?

OTHMER: Dr. Olsen had been a member for years, and suggested I join as a junior member. Nobody else ever from faculty has been an active member. Two or three years ago, while I was head of the Honors Committee, Herman Mark was made an honorary member. But otherwise nobody from this school, either chemistry or chemical engineering, ever has been a member, and Herman Mark never uses the club.

Of course, you have to recognize that a metropolitan city club is primarily a luncheon club, and it is not so important what profession you belong to, but you ought to be living or working within a ten minute walk of the club house. And of course there are twenty thousand, thirty thousand, fifty thousand people in offices within a ten or a fifteen minute walk and they really have a more logical basis for membership than chemists as professionals who are at a distance. I should have mentioned, in passing, that of the professional clubs, and the Overseas Press Club have been satisfactory additions to the club. Each of these had their own memorabilia. Now we have so many mineralogical exhibits from the Mining Club around, and the other Clubs have also brought in various things of their heritages—their art exhibits and so on—and each time it has been a contribution to the interests of the club.

BOHNING: At the time that you joined in the 1930s, were there any members of faculty from the metropolitan area?

OTHMER: Yes, there were several, but the percentage from academia to those from industry has been dropping regularly from a high at the founding to practically zero now. Lincoln Work from Columbia is the only professor whom I remember offhand in the 1930s, but there were others. Link Work went into industry, but that was well before he was president of the club. Our main dining room, the Bogert Room, is named after Professor Marston T. Bogert, of Columbia, the president in 1908. The next president who was a working professor didn't come until I became president in 1974.

Probably 85% to 90% of the fees and other costs of the club come as fringe benefits from the member's employer, and who ever heard of a professor receiving such a fringe benefit?

Thus academicians are not equal prospects, and there are a couple of other things that have been bad for the club. One is the fact that the chemical affinity is not a strong enough drawing bond to make people come from across the city for luncheon.

But the other aspect is that the big corporations, which had their headquarters in that Manhattan neighborhood, now have moved out. Union Carbide was just around the corner at 42nd and Madison for many years before they built their building on Park Avenue twenty years or so ago. Well, immediately fifty people dropped their membership because when they moved to Park Avenue it was five times as far to come and, perhaps more important, Carbide established its own luncheon club.

And of course, American Cyanamid, and Stauffer, and you can name many others that were in the Grand Central area, moved to the suburbs as affording cheaper real estate and less commuting. The chemical industry itself has moved away from its club.

BOHNING: What about Brooklyn? You've been here for fifty years. Do you have any comments about Brooklyn of fifty years ago and what it was like when you first arrived.

OTHMER: Brooklyn, when I first came to New York, was somewhat of a laughing stock, and the then current vaudeville arts always had one or more jokes about it. To some extent, people still like to make jokes about Brooklyn, but this is only about one percent of what it was fifty years ago.

A mixture of immigration settled in Brooklyn as the bedroom for New York (Manhattan), since cheaper living quarters could be had in Brooklyn. They developed a "Brooklynese" dialect that was a hodge-podge of languages. The German, the Yiddish from North Europe, the Italian, the Spanish and so on, mixed into a language which was called, very definitely in those days, Brooklynese. That, by now, has almost disappeared.

We've had great influxes of other ethnic groups in more recent years, but they haven't changed the language much, and that Brooklynese, such as the "toity-toid" for "thirty-third," has almost disappeared. One doesn't hear it any more.

It was noticeable at Polytechnic when I first came. I will never forget that in about 1934 one of my students went to the black board and was explaining something there and he said, as he was pointing to his diagram of the equipment, "and the erl" comes out here.

He stopped dead, and blushed a fiery red. Nobody laughed, nobody showed any amusement, any emotion. And he looked up again and he said, "and the oil" comes out here. Nobody commented, nobody laughed. I thought that was a wonderful courtesy to him, because he was so embarrassed. I remember that he had a Dutch name. I was very proud of my class of twenty-five or so.

Anyway, we don't have such mistakes now. Our language in Brooklyn, due to television and radio and other things that have melded our language, has removed the Brooklyn accent, as they called it. It wasn't an accent so much as it was mispronunciation.

BOHNING: Did you live in your house over in Brooklyn Heights for all these years?

OTHMER: No. I don't know whether I mentioned this before, but Dr. Olsen, my predecessor, was a very successful chap. He was a great legal expert, gave expert legal testimony in all sorts of fires, explosions, acids, breakage of glass bottles, etc. He would put all the glass of a broken bottle together to show the bottle and tell you why it broke if there was an explosion of a pop bottle or what not, and with great good humor.

While I'm talking about Dr. Olsen, I should say he wrote various books on analytical chemistry and one on chemical engineering, and was the secretary of the American Institute of Chemical Engineers for twenty-five years and then became its president.

He made his money in real estate in Brooklyn and when I arrived on the scene, he said the one place for a faculty member to live is Flatbush. I escaped from him one noon and took a walk on Brooklyn Heights, half mile to the west of the school. The school was at Borough Hall in the center of the Civic Center and I decided I would live where I could see the harbor and the East River and the great buildings of Manhattan and walk to work.

So I've always lived there, first, for seven years on Columbia Heights, a half a dozen houses away from where I'm living now, and later in a cut-off half of a big apartment on a top floor, with a big terrace overlooking everything. This was closer to the Institute and had an even better harbor view.

Then in 1944, during the War, I had been on some interesting assignments in South and Central America. I became involved with various assignments for a group of the State Department, although I was also a consultant to the War Department in several of its fields, the Ordnance Department and the Chemical Warfare Service, but this one assignment was for the State Department.

Some attorney in Guatemala found that you could ferment bananas and by the Weisman fermentation you could make alcohol, acetone, and butanol. Well, he was interested only in the alcohol. In Guatemala and Nicaragua, and Honduras, and Costa Rica, they depended entirely on the United States for their petroleum products, that is gasoline and kerosene. We had no ships to send down with the gasoline. We had no gasoline or kerosene to send to them. And we had no ships to bring out their bananas which was their _quid pro quo_.

So this attorney in Guatemala brought this to the attention of the State Department and said, "This is something Central America needs. We want to make alcohol from our bananas which are just rotting in the plantations. We can't use them. We want to make alcohol to use instead of the gasoline we can't buy for our automobiles." The State Department said, "You people all signed up in the War against Germany. We'd like to let you help us and we'd like to help you.

[END OF TAPE, SIDE 5]

OTHMER: "So we will help you by seeing if we can't do something. Please come up here to Washington and tell us about it." So he came up. Do you know when the War broke out we had only 250 passenger airplanes in this country? About 200 of them were DC-3's, which would seat only a maximum of about 21. Then there were a few old Ford Trimotors and other miscellanea. None would fly except during the daytime, and between the Americas they came through Brownsville, Texas.

He got to Brownsville, where everybody was being searched, passing customs, also security. He said, "You mean they are going to look in my briefcase?" and somebody said, "Of course. They are going to look through everything, look at everything including the clothes on your body."

So he said, "Gee, I feel sick. Where is the toilet?" They showed him and, to save his secrets, he spent fifteen minutes shredding and flushing all his papers down the toilet. But anyway, he did get to Washington.

The State Department had called me on previous missions to South America of equal interest, but little value as far as the war effort was concerned. They said, "Dr. Othmer, will you go down?" And I said, "Yes, I will. But I'm not a microbiologist. I'm an engineer. I can engineer a plant, but I have to have somebody to develop the fermentation." They agreed, "You find a microbiologist and we'll ask him to go." I did find an outstanding one, Dr. [Leland A.] Underkofler.

Lee Underkofler has long since retired, but at a later time I introduced him to the job as a chief chemist and production manager of a fermentation plant over in New Jersey, and he wrote the classic book on biotechnology fermentation (59). So Lee Underkofler and I went down. He went first. I couldn't get away immediately for some reason.

He went to Guatemala City and saw the fellow, but nothing there seemed to offer a good place to work. But he was recommended to the United Fruit Company and I heard from him before I left. He was down near San Pedro Sula in Honduras, near the Coast in the mosquito area. So that's where I met him. This was the research center of United Fruit, and certainly was the best place in Central America to do any such work. So I went down in late January of 1944. It was awfully cold in New York while I was having a wonderful time and as was also Lee, missing all the cold February weather. (I was a bachelor then, so I had no family responsibilities. I married rather late in life.)

We did find that you could make a gallon and a half, or thereabouts, of alcohol from a stem of bananas and they had millions of stems of bananas. United Fruit Company, to keep their people active on the payroll and happy, went around and chopped the banana stems and let them fall and rot. Of course, all the equipment to bring the stems in was there in place.

It was then a question of where we would build a plant. I looked around and found that there was an old alcohol plant that oddly enough had been built by the president of my friends, the Vulcan company in Cincinnati. He built it before the First World War on the coast of Honduras. We came back and made our report. I'll tell you this story and you can leave it in or not, but it is interesting to me.

We were there for a month at United Fruit. We worked from about 7:00 a.m. to 3:30 p.m. The laboratory was closed early, because of the heat. Lee Underkofler did wonderful work in his fermentation technology, with only fair equipment available.

There was a little hotel, a wooden framework house, and we lived there. I had an excellent room, and he did also, of the possibly twenty or thirty rooms. And we were entertained nicely by the manager of United Fruit, which was quite an industry there. We got over to San Pedro Sula, fifteen to twenty miles away over awful roads, and made interesting friends, including the mayor and his family, who had an elaborate dinner for us. We would get through with work about 3:30 p.m. because of the heat, after starting at 7:00 a.m.

Before dinner we would stop in the bar and play poker and dice. We never played for money, but we would play for drinks. Beer was five cents and whiskey ten, or something like that. We never drank too much either, but it was fun. I always hate to pay money out. I don't think I'm unusually stingy about money, it's just that I hate shopping and I hate paying out here and there. I'd rather pay a bottom line. So I would always sign my chit when I lost for drinks to the other four or five in the game. Thus, I didn't pay cash at the bar. We were going to pay when we left, as we were going to pay for everything else.

So it came to the morning that I was leaving, after a month or so. Underkofler and I were not leaving at the same time for some reason, possibly air connections. I was going to Mexico to stop there on another project. Anyway, I saw the manager of this little hostelry, the boss of the place, and I said, "I'm leaving at noon and I'd like to pay my bill. I'd like to pay for my room."

He said, "You're a guest of the United Fruit Company." I said, "No. I'm not a guest of the United Fruit. I'm working for the Department of State. I'm paid by the government of the United States." "Oh," he said. "Yes, but my orders are that you are our guest." "OK, let me pay my meal bill then?" "No, you didn't understand." So then I said, "O.K., I'll pay my bar bill." He said, "No, you are the guest. You don't understand. Those are my orders. You'll have to talk to the manager."

I went to talk to the manager and he said, "Look, Dr. Othmer. We have been in the banana business. We've spent many, many hundreds of millions of dollars in ships, land, equipment, some small amount on research. We never studied the fermentation of our only product. This little experiment which we're not paying for, is important knowledge—your keep here is so small an amount compared to the facts that you have reported to us." "Well, OK."

I don't know that I was ever more embarrassed than when I had realized that I had had a bar bill running for a month from my gambling losses, small as they were, and found that I was doing it all on the house!

I came back to my interesting and wonderful apartment I had then in Brooklyn Heights. It was a whole floor of a building, private elevator stop and vestibule. It had views on all four sides, including five rooms looking over the harbor. It was very nice and I had it for a very low cost because rents had been very low (from the Depression) when I signed the lease. The deal was that I would immediately leave if they could do better. I had first refusal.

Meanwhile, what happened in the Brooklyn Navy Yard, all the engineering offices were in lower Manhattan, just across the water, so everything in real estate had been going up, up, up. When I opened my mail there were two registered letters. I took them in order, since they were ten days apart. The first one said, "Please come in and talk about renewing your lease." That is, I had first refusal. The other one was, "Since you didn't come in, we have rented your apartment. Please get out."

I shrugged my shoulders because I had been living around the area as a bachelor and I had a little black book with a dozen addresses of possibilities. Now <u>they were all taken</u>.

I went to a real estate agent then and he said, "There is no place in the area with the view you insist on having. I have a couple of numbers," and he mentioned this number, 140 Columbia Heights. "But nobody wants that," he said. "It's just the parlor floor of the oldest house on Columbia Heights, built in the 1830s and nobody wants it. It's so dark in the middle room, and has so many other disadvantages, no closets, awful kitchen, awful bathroom."

He didn't even take my name, but I remembered the number. When I looked at it and I saw the view, I said, "Well, that's it." I found, from a neighbor, that the landlord actually lived—retired—in Florida. I had to make a proposal. The history of the house was very interesting, but I won't go into that. But I did make a proposal with a 170 word telegram and I got a wire back saying, "Yes, you can do anything you want to in this floor, but you can't change it so that it cannot be used again as a one family house." It had been a one-family house of the Pierrepont family until six or eight years before. It has a magnificent view of the harbor, the Statue of Liberty, opposite Wall Street, Manhattan up to Radio City, the Brooklyn Bridge, etc.

I made some essential renovations immediately, adequate for a bachelor, and the huge amount of sculpture, paintings, and other knick-knacks I was collecting. This was the first of March, 1944.

I lived there as a bachelor for over six years. When I was married in 1950, my wife, Mid, came to live with me on that same floor, and more improvements were made. By 1958, the house had changed hands, again to an oldster who then wanted to sell it. I found that if I wanted to assure this as a permanent home we would have to buy it, but I couldn't get any of the people in the house to go along. I didn't want to be a landlord. However, we then did feel that we had to take it over. So we always lived there and it is a twenty-minute walk from Poly and I probably will always live there. And even though we ought to leave because Mid has bad arthritis and should be in a dry climate, she doesn't want to leave and I don't want to, either. A compelling reason—we have too much of all sorts of collections to move!

More space did come. One good friend in the garden apartment below died within a year, and we took it over, as his wife moved. This was a duplex with an interesting Japanese garden.

BOHNING: Just a few quick questions to close. I have a couple of comments that you made. One, in particular, I was intrigued by. You said that conversion of wood to chemicals was your ticket into the chemical industry and then you followed that up by saying many new ideas in bio-mass conversion were really re-inventions of the chemical wheel, or cycle, no better than they were fifty years ago. And you said the same thing about solar energy, which has been worked on for many years, and now within the last few years, we are looking at essentially the same thing. All of this is only really reinventing what we already know. Do you have any other comments about that?

OTHMER: What I would say would be merely a reiteration and giving examples of it which I don't know are too pertinent. Everything, every big strike in the chemical industry or chemical thought of chemical engineers, comes into being and then it passes on. Some interesting developments came during the War. One reason, of course, is that economies are quite different during war-time when materials _must_ be provided—and _damn_ the cost.

My friends in the Vulcan company did build a big plant during the War out in Washington state for converting wood into alcohol. That only lasted as long as it was government financed —just long enough to be sure that the technology was there even though the war was just over. I don't know if there have been any improvements in big plants since then. Certainly there has been a lot of research processes and papers in the last ten years. Bacterial systems and enzyme systems have been tried but bacteria or enzymes have never been found that are efficient or fast enough in devouring the cellulose and giving as the principal off-product, alcohol. The real cost, however, is in the growing and collecting of cellulose raw materials. So I don't think there really is much to add to that except that it certainly is a fact that people keep turning it over as an old, old idea.

Of course, many other ideas, sometimes a century old, are invigorated, even brought to life, by _modern developments_. Solar energy from photosynthesis, heliostatically controlled mirrors focusing on a boiler have developed into _an orbiting satellite_ with a vast expanse of area devoted to such solar heat absorption and utilization. There are many other ideas, impractical with the hardware of their era, which may be made operative by mechanisms developed in other fields.

Let me mention one other thing that might have been important in the history of the world's chemical industry. Mid and I have done a very large amount of world travel. One reason for that was that in the early 1950s was probably my largest single assignment ever. I don't remember if I mentioned Burma before.

In the early 1950s I was asked to make what amounted to a survey and report on the chemical potentiality, interest, demands, needs and program for a chemical industry in Burma. Burma was about 23,000,000 people at that time. They wanted me to go out, bring Mid of course, and spend three years in Rangoon. We would be given a big house, with eight servants, right on Lake Victoria which is the desirable residential area where the top British politicians, merchants, and bankers lived before the separation. That would be my job and our residence for these three years.

I said, "No, I can't." I was head of the department. "I can't go, but I can do that sort of work better in New York, if I get out to Burma from time to time to get a feeling and keep in touch with the facts picked up by people there." So what was agreed was that we would go out there two or three times a year to Rangoon to pick up data and impressions, then come back to New York, where I had many friends, expert in different industries, to plan and estimate costs for plants and products.

Rangoon, in relation to New York, is just half way around the world. So we always went around the world—east or west was the decision—because I had many, many contacts and contracts and work to be done in Japan, usually also something in Germany or Denmark or Spain, of course friends, and always some <u>prospects</u> for work in India. (India is a long gamble on which to spend time for any project!) As I said, we were married in November, 1950, and during the first twelve years of our married life, I took my wife around the world thirteen times.

I had gone around the world in the summer of 1950, before we were married, to be sure it was round; it turned out to be, and it was safe to take her. So we had many, many good times in Japan, each time the guests of two, three, or more of big chemical companies, anxious for plant designs, and reports on current developments in the U.S. chemical industry.

But as to doing this job in Burma, there were so many fascinating aspects to tell of the economy, social and political life of a undeveloped country teetering on the brink of communism —it then lost. I can only take time for one or two.

At that time, one of my friends who just last year won the Perkin Medal, one of my students of years ago, Dr. [Peter] Regna, had been, with others, developing terramycin with Pfizer. And the manager or the Chief Chemical Engineer, Manager of Production, was another chemical engineer by the name of Herman Poitras, who was a very good friend. His wife was a friend of Mid, before we were married. So we saw them often.

Before we went to Burma the last time, for the entire summer of 1953, we were Poitra's guests over in Saddle River, New Jersey. He said, "Don, we've just been making some new pills, something like penicillin. I have a bottle of them, 50 or 100 that are irregular in shape and thus couldn't be sold. Maybe you ought to take this dozen with you. You might run into a need."

This business in Burma I could discuss for a long time. It was crazy, and all the work we did was entirely out of proportion. I almost killed myself overworking that particular summer. Every time you left the U.S., among other indignities, you had to have shots for typhus. When I was a youngster, 16 years old, I had had typhoid fever, and so typhus shots never were any big deal, and never "took." However, I was very much run down in the summer of 1953 in Burma. It was hot, awfully, and every afternoon between 1:00 and 2:00 an inch of rain would fall—so everything steamed.

Time came for my booster typhus shot. I was very run down physically, and later had to have my heart taken out and peeled off (pericardectomy). That was coming and I didn't realize any symptoms, except general debility. I went to the doctor for the shots. I forget his name. He was an Indian I had been working with because he seemed to know more about the pharmaceutical industry than anybody else, and he also was a practicing physician. (He had suggested that for Burma there should be a plant made for medical alcohol; when I asked how much for the country of 23 million people, "Oh, probably 125 gallons per day." Of course, he meant British gallons. Somewhat more!

He gave me the typhus shot, and a few hours later I became deathly sick. What happened, my heart was failing, and I was down to maybe 60% of my normal energy. I knew I was going downhill but I didn't know its cause. But I didn't think anything about taking this shot, as it had never before "took." It went to work on me and I developed a tremendous fever, and someone got him to us at the Strand Hotel in the middle of the night—this same doctor, the best doctor in the country.

But he couldn't help. He just said, "I don't know what can be done. You have a fever that I don't understand." I said, "I have these pills." And I gave him the name—terramycin. He had never heard of it because it was very new. I, of course, had no idea as to dosage. But I took one pill, another one in about four hours. The next morning that fever was gone. Actually I have yet to tell Peter Regna that story. Some time I will. Peter also was very important in the development of streptomycin.

The work in Burma meant thousands of pages of reports, evaluations, drawings, calculations, graphs, etc., all of which, I'm sure, were just filed away in some bureaucrat's office. Nothing ever happened. They have all since rotted away in their musty storage systems.

The reason for interest just then, was that Burma was going "Commie." Our government paid all of my expenses and my costs outside of Burma. The Burmese paid all the costs in Burma. But it was going Commie.

We met and spent time with the Russian ambassador to Burma. Burma was just then getting in under Soviet influence. He, one of the principal Russian diplomats, having done a good job in Burma, was moved to be the ambassador of Yugoslavia which then had problems, and was a much bigger post. The answer, of course, is that since then Burma has gone socialistic. However, the economics and design, preliminarily, of course, of plants, products, and their potential interrelation, to each other and to export and imports, all of this was fascinating and the work, the answers and conclusions, unused as they have been, have been one of my great professional experiences.

BOHNING: Do you want to talk about those a little bit?

OTHMER: We went there, as I say, two or three times a year for several years and had the broadest scope of a program. I had to question where you get the raw materials, their costs, plant, labor and management costs, and so on.

Sometimes, I found I was on a different side. I am a capitalist, and here I was recommending to the Communist government there that they make fertilizer and give it away free to every farmer. But I was working for a "Commie" government, as a client. The "Commie" government had quite a different format than ours. The first point was that farmers were only growing two-thirds as much rice as they would grow with fertilizer. Their domestic consumption was about one-third; two-thirds was exported.

Their production per acre compared to Japan was only about one-third, and usually they could get two crops a year while Japan can only have one.

Another point was that the domestic market was so low as compared to their export market, and, of course, the people of Burma could only eat so much rice. Thus domestic demand was fixed, the government bought all over that at the local price, very low compared to the international market.

But as you can see, I figured they would get one-third more rice by fertilizing. That excess rice would all automatically go to the government at the local price, and the excess over domestic use would be sold on the outside market for about three times the value in the local market.

If you give the farmers fertilizer, you get 1 1/3 of the former production. But one-third of the crop could be consumed locally, so you would have all of the former total production for export compared to two-thirds, and this at three times the local price. Actually I have oversimplified; the profit would be greater to the socialist government.

You get so much more to export so your profit on your export would be so much, you could well afford to give fertilizer to every farmer. Burma never built a fertilizer plant. Never has, although I designed roughly and costed three from the relatively small amount of indigenous raw materials.

And there were other interesting aspects of their life. Every Burmese woman had to have six children. Why? Statistics show that two were lost in childbirth, two were lost before they came to biological fulfillment, and so the other two were necessary to perpetuate the population. DDT saved so many lives there and I guess it is still used in Burma. I hope so, because it is the only way they could take care of insects, a cause of their high disease rate.

They were such lovely people but I never got to know them. None of many Americans there on similar missions got to know any Burmese intimately. We never were in a Burmese home. Nobody in

Burma at that time, I was told, had an income of $50,000. Nobody in the country had $50,000 capital. All they had seemed to be happiness and carefreeness. They had these. Everybody was so sociable and kind—their joviality, friendliness, friendly joking were engaging.

Nobody wanted to do any work, they didn't seem to need to, and had no motivation for money or things. Yet they were so generous. Everybody had a bottle of water and a bag of rice hanging on their gate in the country, or anyplace else. Any beggar could come along and take freely of the water and rice.

All they wanted from the United States was an automobile, which they knew they never could afford, and secondly, gory Western movies. Anyway, that was a great experience in Burma for Mid and me. It's too bad the results could not have been considered seriously and possibly utilized.

BOHNING: You said, in 1977 in your Chemical Pioneer address, "My generation has been fortunate in being part of the most interesting period of life on earth, past or future" (8).

OTHMER: Oh, yes, I remember.

BOHNING: How have things changed in your long career? If you had a crystal ball, what might you see in the future?

OTHMER: I still feel sincerely what I said then. Maybe that's because I'm an optimist and I have always enjoyed what I did, and I never had to do anything that I didn't enjoy doing. I mean, everything I had to do, I've taken in stride and enjoyed it—particularly, of course, when the results were positive. Life has been a pleasure. It has encompassed an interesting time indeed in world history. I remember, of course, before the outbreak of World War I, being a Boy Scout during that time. I never knew the bad parts of misery and hovels, or the luxuries on the other end of the scale.

I do know that when I got into the charcoal industry, after I had been with Eastman Kodak and when I first came to Poly, most of the charcoal made was sold for cooking use to "cold water flats" as they were called, places where there was no gas, and no hot water. Charcoal was used as the cooking fuel in thousands and tens of thousands of flats in New York City in the 1930s. But I didn't know intimately how bad life was. Certainly, it's better now, even though most people can still cry for the good old days.

[END OF TAPE, SIDE 6]

OTHMER: I do remember some of the awful things of the Great Depression, although I was pretty well insulated by being well established in 1929. But that was an experience I hope we don't repeat. World War II found me at about age forty too old to fight but well enough established professionally to be asked to make numerous trips by the State Department to implement supplies of chemical materials and to have designed an important step in the manufacture of millions of pounds of the great chemical explosive, RDX. Also, some five plants used my processing to make chemicals which would have been used had there been gas warfare. In an early design session, I remember a non-technical general, who recognized the extreme corrosiveness of the chemicals used, asked what would be an impervious metal. I hesitated, then said, "Well, I suppose gold." "<u>That's</u> <u>it</u>," he said, "There are thousands of tons of gold at Fort Knox which are doing nothing to win this war. We'll put them to work." "Yes sir," I said, smiling inwardly.

That gold will have to be mined for other purposes, from its present lodes in Fort Knox; it was only fifteen or so years ago that the last of the hundreds of thousands of ounces of silver was returned to Fort Knox after serving for the war and subsequent years as heavy-amperage conductors in some industrial plants.

Awful as were the wars, the second one brought out the nuclear threat, and yet I note that we have had the longest period in history of the last centuries without a major war. We have been at this relative peace for forty years now. For some time, twenty-five years seems to have been the longest time between big wars. Of course, there have been a lot of minor ones.

The only thing that one can say that prevented big wars is the threat of nuclear war, and this has kept the two superpowers away from each other. Good, bad or indifferent, it had its effect. What's going to be in the future, I don't know.

As for common living, don't look for automatic vacuum cleaners that will run around the house and clean the house, or automatic potato peelers which will take potatoes and turn them, pronto, into a dish for the dining table. I think we've done pretty well in mechanical contrivances for our ease and pleasure.

Certainly, we have made robots in many manufacturing steps. Sometimes I've considered the time it would take to program a robot to change from one simple program to another, like scratching an itch at the back of your neck. You don't have to program your arm to do it. You just reach up and scratch to the back of your neck wherever you feel something wrong. Just think of the time it would take to program a robot to do something simple like that, and from that to, say, brushing your teeth.

Then consider reprogramming a robot in any manufacturing operation.

We will use robots. We will get simpler devices, better and simpler programming. We will enjoy maybe more of the luxuries of life, through our senses of hearing and seeing, of music, of art, and so on. I think it may be we can learn to enjoy somewhat the same things more by greater and more sensitive devices.

We have our televisions which bring us so much and our stereophonic equipment which also brings so much to our senses. But, I don't know how we are going to improve the enjoyment of life. We certainly have, in my lifetime, and I can only hope that the next eighty years of life will bring as many great things as have the past, and including those I can't conceive of any more than my father could conceive, the day I was born, of modern miracles. But I am not necessarily as optimistic as to believe that the new miracles are going to evolve. And I won't say that I believe the world will be worse off if they don't materialize.

BOHNING: One last thing. What would you say to a young person entering the chemical profession. What is in store in the profession?

OTHMER: Well. I think the professors will take care of what the young fellow has to study; and all I can tell him is he wants to be very lucky, as I have been.

BOHNING: Is there anything else that you can think of?

OTHMER: Oh. I could go on forever. I have tried to think of things for you which were of concern to history and other people. I have had fun with happy reminiscences.

BOHNING: I would like to thank you very, very much.

[END OF TAPE, SIDE 7]

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