

THE BECKMAN CENTER FOR HISTORY OF CHEMISTRY

DONALD L. KATZ

Transcript of an Interview
Conducted by

James J. Bohning

at

Holland, Michigan

on

22 August 1986

Donald L

Katz

JH

3/15/96

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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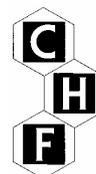
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DONALD L. KATZ

1907 Born in Jackson County, Michigan on 1 August
1989 Died in Ann Arbor, Michigan on 29 May

Education

University of Michigan
1931 B.S.E., chemical engineering
1932 M.S., chemical engineering
1933 Ph.D., chemical engineering

Professional Experience

1933-1936 Research Engineer, Phillips Petroleum Company

University of Michigan
1936-1942 Assistant Professor of Chemical Engineering
1942-1943 Associate Professor
1943-1964 Professor
1951-1962 Chairman, Chemical Engineering Department
1964-1977 A. H. White University Professor
1977-1989 Emeritus Professor

Honors

1950 Hanlon Award, Gas Processors Association
1959 President, American Institute of Chemical Engineers
1959 Michigan Engineer of the Year, Society of Petroleum
Engineers
1962 Distinguished Lecturer, Society of Petroleum Engineers
1963 Visiting Professor, National School of Chemistry,
Rio de Janeiro
1964 Carll Award, Society of Petroleum Engineers
1964 Distinguished Faculty Achievement Award, University
of Michigan
1964 Founders Award, American Institute of Chemical
Engineers
1967 Warren K. Lewis Award, American Institute of Chemical
Engineers
1968 Member, National Academy of Engineering
1968 William H. Walker Award, American Institute of
Chemical Engineers
1969 Honorary Member, Phi Lambda Upsilon
1970 Mineral Industries Award, American Institute of
Mining Engineers
1972 Distinguished Public Service Award, U.S. Coast Guard
1975 Murphree Award, American Chemical Society
1977 Gas Industry Research Award, American Gas
Association
1978 Lucas Gold Medal, American Institute of Mining
Engineers

- 1979 Award of Merit, Michigan Historical Society
- 1983 Selected as an Eminent Chemical Engineer, 75th
Anniversary of American Institute of Chemical
Engineers
- 1983 National Medal of Science
- 1984 Designated Distinguished Member, Society of
Petroleum Engineers
- 1986 Honorary Member, American Institute of Mining
Engineers

ABSTRACT

The late Donald Katz starts the interview by briefly referring to his current projects but then describes his family background and his genealogical interests, stimulated by his 1952 trip to his father's birthplace in a German village. Katz went to a small country school in rural Michigan but was encouraged by a church minister to attend high school in a nearby town. After working in a machine shop, Katz entered the University of Michigan at Ann Arbor and majored in chemical engineering. He put himself through college by hard effort; cleaning offices, restaurant work and some summers back in machine shops. As he continued into graduate studies, Donald Katz acted as George Brown's assistant, helping other graduate students, junior faculty and with patent cases. He recalls courses, coworkers and faculty at Ann Arbor and gives three anecdotes illustrating the influence of surface chemistry, as taught by Floyd Bartell, at later stages of Katz's career. When Katz started his research career at Phillips Petroleum he was assigned to reservoir studies and he summarizes some of his activities during this period. An invitation by Brown brought Katz back to the University of Michigan and he details his early researches and consulting work. The war years altered some of his teaching and research responsibilities and led him, for instance, into heat transfer investigations. On Brown's promotion to Dean, Katz took over as departmental chairman for several years. During this part of the interview Donald Katz describes his involvement in the introduction of computer education into the chemical engineering curriculum, both at Ann Arbor and nationally. Other recollections follow; safety and the hazards of bulk chemicals; pipelines; the underground storage of gas and air; the origins of the Handbook of Natural Gas Engineering. Katz concludes his interview with some thoughts on the changes in the academic chemical engineering profession over his long career.

INTERVIEWER

James J. Bohning holds the B.S., M.S., and Ph.D. degrees in chemistry, and has been a member of the chemistry faculty at Wilkes College since 1959. He was chair of the Chemistry Department for sixteen years, and was appointed chair of the Department of Earth and Environmental Sciences in 1988. He has been associated with the development and management of the oral history program at the Beckman Center since 1985, and was elected Chair of the Division of the History of Chemistry of the American Chemical Society for 1987.

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INTERVIEWEE: Donald L. Katz
INTERVIEWER: James J. Bohning
LOCATION: Holland, Michigan
DATE: 22 August 1986

BOHNING: Perhaps we should start by you telling me what project you are working on at the moment.

KATZ: Right now a book that I have been working on for five or six years. At my age I'm having trouble problem solving; some of the solutions are in the mathematical area and I'm not very good as my math background was relatively poor. I didn't have any decent courses as a student, and I took my last formal course on graphical methods in 1930. A couple of people were going to help me but they backed out, so finally I found a young man who has his Ph.D. in chemical engineering in the field of reservoirs. Robert L. Lee is American-Chinese, and he wanted to study with me. Lee got to know me, knew of my books and my papers, of course. He said he'd like to work with me for a year; I talked to him about the book and he agreed to do it. He's doing it on the computer, he types it all out, including the figures and since the first of August he's got three chapters almost in their final form. We have great hopes of finishing it this winter and using it in a short course I give for industrial people in June. I give one every June mostly by myself but with a little help from others.

I'll be eighty next summer, so when that course is done I'm going to turn my attention to historical matters. I'm working on a paper with an Iranian boy at Stanford who is coming to see me the second week of September. I still work with a few of the gas storage companies; I will review reports from three other companies this fall.

BOHNING: When you turn your attention to items of historical interest what areas would you be looking at?

KATZ: I'm looking at the area of reservoir engineering and how this knowledge was put together during my career. If a doctorate student were available I'd work with them as I'm working with Lee. Feed them the material, discuss it with them, and let them write it up. That's what I'd really like to do. When I read my Red Book [The Settling of Waterloo, Michigan] (1), I think, "Oh, my gosh, did I write that?"

BOHNING: Yes, I know you've written a lot about your family

history. You were born 1 August 1907 in Jackson County, Michigan. Where in Jackson County?

KATZ: In Waterloo Township, on a farm that my grandfather John Schnackenberg bought in 1859. My mother was his youngest child; eventually, after she had married my father and they had three children, they came back to live there, and finally bought the farm. I was born in the same house as my mother, a brick house built in 1861.

BOHNING: What about your father?

KATZ: My father was born in Germany, in the little village of Hochdorf, near Nagold. Hochdorf is a village of 1,000. Hochdorf, which means high village, is about six miles from Nagold and about 15 from Stuttgart. My grandparents had seven or eight children and they came over as a family. My father was then the youngest and they had one more child, a little boy, Uncle Jacob who was born in this country. They came to Manchester, Michigan. Manchester is a village fifteen miles from Ann Arbor.

I would like to talk about my trip when I found my father's birthplace. In 1952 we planned a trip to Europe with my family, my son Marvin was then sixteen and my daughter Linda fourteen. We had planned this trip a couple of years in advance and took the liner United States on its maiden voyage when it beat the world record on 3 July 1952. We had our own car with us, a little Studebaker Champion. We spent two weeks in England and the rest of the fifty-two days on the Continent.

My father died when I was eight, in 1916. When I was young I asked my aunts and uncles where they originated. They spoke broken English, indeed, my father's mother did not speak it at all. They could remember Hochdorf and that it was near Nagold. So we had planned to go to Nagold when we were in Europe. We had been to England, Holland and Belgium and were in Germany, coming down from Heidelberg where we had stayed the night. When we neared Stuttgart we couldn't find Hochdorf, even on a good map. Finally we asked somebody at a filling station and they showed us where the village was. Then we went on to Nagold to stay the night. The names on the stores were just like those in Ann Arbor; Waltz, Snyder, and so on. We stayed the night there, in a hotel dating from 1650 or something. The next morning we went to the village and on the way we saw girls binding wheat, we saw them cradling [to cut with a cradle scythe; ed.] wheat, we saw them loading oats. This was in early August 1952. We found the village Rathaus [City Hall] and told them my father's name and they pulled out a file showing my family record, which said 'acht November nach America ausgewandert'. They emigrated to America in November 1883.

They came as a family. The brothers and sisters of my grandparents had come earlier when they were single and aged 18 or 20. My grandmother was also a Katz like my grandfather, although they were not related for ten generations back, as the record showed. A third of the village of 1,000 people had our name. An aunt and uncle, my grandmother's brother and sister, came to Ann Arbor in 1854. Another aunt came to Manchester around 1863 or 1865 and one came to Saline. My grandfather's brother came to Union City in southern Michigan, near Marshall. I learned all of this from my aunts and uncles when I questioned them when I got home. Quite the nicest day of my family life was at our house in Ann Arbor in the large living room, where I can seat fifteen or twenty guests and we had colored movies and slides of this village. The village that my aunts and uncles had left sixty-nine years earlier, and which they had never heard from since. I showed them the old house, no longer occupied, which the people in Hochdorf pointed out to us. "Oh," my aunt says, "that rack on the second floor. That's where I used to dry my dishes in the summertime".

It was astounding that in this village there was the original built-in heating system; cows on the first floor and people on the second. We learned the church had been built sometime in the 1400s. We've been there six or seven times now and we've learned that they all became Lutheran churches within weeks after the Reformation started. At the Rathaus I got copies of the birth certificates of my grandparents on one side and the grandparents on the other and their families. So I had three sets of sheets from there. At the Lutheran Church, where they also had records, they wanted me to be sure that I understood so they had an interpreter there, a returned missionary from South Africa who knew English. We know very little German; my son had taken German in college, and he had quite a time trying to use it. Anyway, the minister was very concerned to show us a shelf of books, he said, "The mayor's office copied my books in 1890, but mine go to 1630. The earlier records were burnt in the Thirty Years War and that's why there are none before 1630."

The village people were very poor in 1952 which was not long after the war. We had taken four collapsible cases full of clothes with us on the ship and we gave them these clothes. We knew that Europe was going to be poor. They were elated of course. When we came home we sent the minister a check and asked him to take a little time and go through his books to copy our family's entries. Next April we got two packages that thick; a numbering system, your number is one, your father is two, your mother is three, your grandparents are four and five, six and seven. The numbering system got up to 1,012. We had 103 families out of 128. Not only names but also the family sheet for them, showing brothers, sisters, birth dates, death dates, who they married, who their children were; it was a fabulous thing. These records were the church records; they started with marriages, deaths and births, then eventually had these family sheets. The family sheet I have in my book on the settling of Waterloo (1) is an adaptation of what they had on two sheets, but including a place for addresses, and

a place for a little biographical sketch of the person. Otherwise they're the same as they had in Germany.

After I'd got these records I showed my aunts and uncles and cousins the pictures of the village they'd left; the women prodding the cows to bring the field grain in on a wagon. I had four aunts still living then, aged from 65 to 80. They could see that if it hadn't been for their mother, they could have been the ones prodding those cows. I couldn't help but decide that some day I must put it all together in a book. That was in 1952. At the same time I finished my Handbook on Natural Gas Engineering in 1957 (2), I completed it (3). I printed about 300 copies, and mailed a copy off to every person who was in the book. We were going to Europe in May of 1957 and I got them into the mail before leaving. Since then of course, I have had more copies distributed and I'm nearly out of that book, The Katz Family from Hochdorf (3).

BOHNING: What brought them to Michigan?

KATZ: Obviously somebody came in the great settling of Michigan. Between 1850 and 1860 was a period of extensive land purchase. My mother's family came because they had brothers and sisters already in Michigan, but they were the only ones that came as a family, and they had waited until their parents had died. My father became a farm helper, a hired man, and that's where he met my mother, the youngest daughter who was staying at home.

BOHNING: How many brothers and sisters do you have?

KATZ: I had two brothers, they're both gone. I have a sister still living, she's eighty-five now, just had a birthday.

BOHNING: You were the youngest?

KATZ: I was the youngest, we children were two years apart. When my father died, I was eight, I had brothers of ten and twelve years old, and a fourteen year old sister. We lived on the farm.

BOHNING: Did you continue to run the farm after your dad died?

KATZ: Well, we weren't really old enough to run a farm, but we had a hired man who four years later became my stepfather. Herman Rothman was a neighbor who came and worked summers. In the winter we did the chores ourselves, and he went to work in a

factory. He married my mother in 1920.

As a boy I got a good schooling in work. The three boys had the chores to do. My job was to get the wood in for the house, kindling and firewood and the chunk wood for the big stove in the living room. I looked after the chickens and I milked three cows, and at one time I cleaned the stable; I'm not sure if that was all the time, but I had jobs like that. My brother looked after the horses and the pigs, and helped milk; my other brother looked after the sheep and fed the cows, so we really got used to working as children.

BOHNING: You attended a country school?

KATZ: Yes, a country school; we walked cross country along an old railroad to get there. The school had been larger when my mother went there. When I started, there were as many as 15 or maybe 18, first to eighth graders. The rules were that you had to go until sixteen, at least four months a year, even if you did pass the eighth grade. But the numbers dwindled, they got down to ten or twelve, and it turned out that no one was in the same grade with me on a regular basis. I was put in class with one or two regular students, a class ahead or behind so I did much reciting alone. We had a nice little library there, I guess I read all the books.

BOHNING: What kind of books?

KATZ: Grimm's fairy tales, books like Heidi in Switzerland. They got their money for their library from the unspent dog tax. The dog tax was presumably to recompense for livestock damage, especially sheep that dogs had killed, and the money that wasn't used in the township was distributed to the schools. That's where we got our money, and it was a nice little library, with about one hundred and fifty books or so.

BOHNING: Do you remember any of the teachers you had?

KATZ: Yes, Mannie Archenbraun, who was the only man teacher I ever had. Mae Frinkel, who had only had ninth grade education herself, only one year of high school. Lillian Schmidt, she had graduated from high school. I believe Mannie Archenbraun had been to college a year or something like that. I remember Lulu Smith when I was in maybe fifth or sixth grade. Recently, I happened to go to the wedding of one of my graduate students, Donald Robinson, a boy from British Columbia, who went to our church, the Methodist Church in Ann Arbor. His bride, Barbara Smith, is the niece of my country school teacher, Lulu Smith. By the way, Robinson received the Donald L. Katz Award from the Gas

Processors Association this last April. We've seen him over the years; he has been teaching at University of Alberta in Edmonton. He was head of chemical engineering there but he's now running a consultant group, D. D. Robinson Associates. When I was writing the Waterloo book, I went to see Lulu Smith in a retirement home. She had a picture of the class, one that I didn't have, so it's in the book there. It was very interesting to visit and meet her again.

BOHNING: As you were going through school, let's say before you got to high school, had you given any thought about what you were going to do later on?

KATZ: In those days we were relatively poor country people. We were poor for money; it wasn't that we were hungry or didn't have clothes to wear, we were not poor in that sense. For example, my brother and I were playing baseball, and our friends said they were going to get a new bat and that we had to chip in a dime apiece. We didn't have a dime to chip in so we had to sit out the spring season once; because we didn't have a dime, they wouldn't let us play. It was that kind of background. Of course we carried our lunches to school, we could put a potato under the stove in the winter and have a baked potato. I enjoyed school; I completed the eighth grade, passed the eighth grade county exams in 1920 when I was twelve. The sixteen year old rule applied. However, I went two more years for about four months in the winter. I was very interested in reading and I liked school very much. I was planning to go one more year and leave when I was fifteen. The minister at our little Methodist Church, Ralph Harper, was a student at University of Michigan. One Sunday in the summertime he brought another student with him and they said they wanted to talk to me and my mother. They got us aside after church on a Sunday afternoon and said, "How can we make it so that this young man can go to high school?" We talked about it. One of my father's sisters lived on a farm two miles from Grass Lake, and so we finally decided I could go to high school at Grass Lake. The school district paid the \$60 tuition a year at that time, so it didn't cost me anything, only for my books. My aunt kept me for a couple of dollars a week. I had to walk two miles each way on a gravel road, but sometimes I rode with the milkman. They had a strict four year curriculum, no choices, but good teachers. My folks had given up farming by then, because it was a very poor life, and my stepfather had been ill, that would have been in 1922. He decided to work in the shop in Jackson so they got jobs in Jackson. They had a friend who lived alone and they decided to go and live with her. When they moved to Jackson at Thanksgiving, I changed to Jackson Junior High School, where the subjects were easier. By that time Latin and History and Algebra were very easy for me because I'd been working hard in Grass Lake, but English was never my good subject.

BOHNING: Did you have any science related courses in either

Grass Lake or Jackson?

KATZ: Yes, I had a course in science at Jackson one semester, and it was of great interest to me. I remember when we came to astronomy and was told that it takes forty years for light to get here from a star and I said, "You mean if it disappeared thirty nine years ago, we wouldn't know it yet?" As an eleventh grader I was very much interested in chemistry. I enjoyed chemistry with Mr. Newark, who later became a teacher in chemistry at the Junior College, or Jackson Community College as they call it now. As a matter of fact, the American Chemical Society had a contest on Agricultural Chemistry at that time and I wrote a report, not a very good one, for that. If you won you got four years at Yale, and that's what got me interested in chemical engineering. I didn't really know the difference between chemistry and chemical engineering. I took a year in physics and I had no trouble getting good grades in all my courses, except English.

BOHNING: The chemistry course, was that in Jackson or was that at Grass Lake?

KATZ: Jackson; you see I only stayed at Grass Lake for three months. I was living at home for two years and I roomed in Jackson for one year. I did most of my school work at six thirty in the morning. I had to walk a couple of miles to school but when I got there I could study for another hour or so. We had study hall in those days, but Jackson High School had no Gym at that time. I had good grades and I ended up by being the salutatorian of one hundred and seventy five in the class.

BOHNING: Did you become close with any of the teachers there?

KATZ: Yes, Mr. Johnson, the history teacher. I love history, ancient history. He brought me the books that had been sent to him to review for use next year and he'd give them to me to read. I had a good math teacher, I went through trigonometry which I thought was just a bunch of rules. I learned them and lots of algebra, and took an advanced course in algebra in my senior year. I had no problem with math at high school.

[END OF TAPE, SIDE 1]

I graduated school in 1926 and we'll have our sixtieth reunion this September. There were two of us still working at the fiftieth reunion ten years ago; I'm still working but I imagine the other man isn't. But the men are mostly gone, all my boyhood friends from high school were gone at the fiftieth, the ones I really knew well. I did not know these people socially in the usual sense because I did not live at home in Jackson.

When I got through high school I looked for a job, I thought maybe I'd be an apprentice. I went to the Chamber of Commerce to see the Secretary, he said, "Why don't you be a tool maker?" So I went to the shop and was interviewed, but they had no opening for a tool maker. There was a school for auto mechanics in Detroit but it cost quite a bit of money, which I didn't have.

I had worked in a shop in the summer between my eleventh and twelfth grades. The Morrison Stamping Company was a division of the Hayes Wheel Company and had a large machine shop where we made disk wheels, brake drums, and things of this kind. I worked there in the summer of 1925, then I got a job there when I was through high school, starting probably by the first of July 1926 when I graduated. Now I was almost nineteen when I finished high school. I started work there but I got laid off during the year. But I had the opportunity to run these big machines, to feed plates. I ran a blanker to make brake drum blanks and I ran a press to make the drums, I ran disk wheel trimmers, and I ran the former. Anyway, I got to run these big machines. It turned out to be a very noisy shop and I attribute my loss of hearing that started when I was fifty-five to that. Part of it is hereditary, I know, because my son who's now nearing fifty is feeling the need to get a hearing aid, so I'm not really sure.

So I worked there but I got laid off sometimes. My stepfather worked there too, and my brother had worked there until he went to the railroad as an engineer. When I was out of work, I would help my stepfather shingle the barn or put on a roof or something. I did some graveling on the roads.

Just before school started I went to see Mr. Bliss, the principal of the high school, about going to Michigan State. Now I had never visited Ann Arbor, twenty-eight miles away but I'd never visited it. I'd been through it twice on a street car, but you see that's Washtenaw county, and we had looked at Jackson as our county. I went to see Mr. Bliss, an elderly man with a Master's Degree from the University of Michigan, and I asked for a recommendation to Michigan State. He talked with me a little and said, "Why Michigan State, why can't you go to the University? It's closer," and I said, "Well, I just don't know anything about it." He said, "I don't either but I'm willing to bet my last dollar that it's much better at Ann Arbor than it is at Michigan State." Of course he was so right; it turned out that we were the second or third school in Chemical Engineering in the United States, pushing MIT. Three schools had 90% of the doctorates and Ann Arbor was one of them.

When I went there I had seven hundred dollars, six hundred in the bank, and a hundred I had loaned my brother when he bought his house. A cousin, my father's oldest sister's son, who was a school teacher had been in Ann Arbor for his Master's Degree in Education, and he'd been working in a restaurant at noontime, and he'd also worked cleaning at a civil engineer's office. I asked him to get a little job for me if he could. He did, so I went to work in a restaurant on the first day I went to school there. I

worked from 11:30 a.m. to 1:30 p.m.; I got a dollar a day and my lunch. I went to the civil engineer's office and I had to wait a week before I got the job, which paid fifty cents an hour, for four hours once or twice a week. So I made eight to ten dollars working noons and two afternoons a week. The first year at Ann Arbor cost me three hundred out of my savings; I had a room that I paid about four dollars for, I had lunch at work, so then I had to buy dinner, books and pay my tuition.

The next summer, 1928, I worked at the Chelsea screw plant. My folks had lived in Chelsea, a village about ten or eleven miles from our farm; my stepfather was working at the screw plant. My brother was working the farm, he had married in the meantime and was living in a home in Chelsea, and I'd go there weekends from Ann Arbor. We worked sixty-five hours a week, five nights of thirteen hours. I ran three automatic screw machines and although the machines were noisy, they'd make you feel sleepy. It was a nice experience, it was warm, we had our meals that we took with us; my stepfather ran a screw machine too, and I learned to grind drills, grind taps and things of this kind and to run screw machines. I saved three hundred so I was back to six hundred dollars, and I went into my sophomore year and I decided to go summers thereafter. I went summers in 1929 and 1930, and I guess it was in the latter that I took physical chemistry. I took physical chemistry laboratory from Professor Lee Case and was the only student taking the course. He personally described each experiment to me on the board, and then I liked physical chemistry very much. I had taken my general chemistry with Professor Hodges; I had Professor Roy McAlpine for my qualitative analysis and Professor Malouch for my one semester quantitative. I got a job my junior year in chemistry as Professor McAlpine's assistant, in the laboratory for qualitative analysis.

I had some nice little experiences. One Saturday afternoon in a little room on the fourth floor, I was making up the dry samples for qualitative analysis. As I was sitting there making the records in my book, I heard a noise from a box up on the shelf. "Oh my, what did I put in there?" I looked it up; it was ammonium nitrate and zinc dust. I went to the lab the next week and took some zinc dust and ammonium nitrate, shook them together and let the mixture sit in the sink and within two to five minutes it would ignite. This is a little side dish to the story, but later on I saw the first ammonium nitrate plant making ammonia from natural gas out in Calgary. They had two rules: never bring organic material into the plant, they were careful even with paints. The other, never allow anything to go above 250°C. By then I was familiar with the Texas City disaster in 1947. So I have some understanding about what happens when ammonium nitrate and other things get together.

I went on taking summer classes as well as the normal semesters. In the Fall of 1929, my math teacher had gotten me a \$100 scholarship which was handed to me at the end of the summer session. Then I wondered what to do with the money. Well, I had

never been out of state. I had a friend who'd been out in North Dakota, and I went to see whether he'd like to go with me but he couldn't. I bought a Model T Ford for forty dollars, paid five dollars to a friend in Waterloo to grind the valves and get it ready. It was a touring car. When my summer school was over, I got in the car and drove to North Dakota. Worked in the harvest fields for a few days at five dollars a day. Thirteen hours a day, and it was 105° out there; I got sick with the water, the water was muddy in the jug. I slept in the barn. That was between Bismark and Minot, North Dakota. So I started back on Highway Two with my model T and I had several flats with my not very good tires. All gravel roads except a mile ahead and a mile beyond any city. But I made the three thousand mile trip with 105 gallons of gas; then you could turn the carburetor down. I slept in the farms, I'd go by a farm and park off the road. I had a horse blanket, and a bread box for my vittles and shaving material.

I got back to Northern Michigan; my folks knew I was coming back, and they were up in the Northern Peninsula and we met there. I was coming back from the Soo Locks at Sault Ste. Marie. I went around Hancock with my folks, then came back across the straits but it was so rough on the ferry. The emergency brake didn't work very well on my Model T, and it was so rough on the ferry that I had to sit in my car to keep the car from rolling into a wave and ruining my radiator. But I got home and I went on with my studies.

I knew by my junior year that I was going to try for the doctorate and had outlined what I needed to do. I took three semesters of German, which I figured would be enough to pass the exam, so I took French for my fourth language semester, I took a year of French. I took my qualifying exams early, and I took extra hours in the summer. I really finished the required hours for my bachelors degree by January 1931. By August 1931, I'd finished all my course requirements for the doctorate. I'd taken my languages, I'd had my twenty-one day problem, and I took the qualifying exams and passed them. I had become [George G.] Brown's assistant in January of 1930.

In my sophomore year the restaurant where I worked needed a boy for the evenings as well, so I started working from 5:15 p.m. until 7:30 p.m. I got my dinner and a total of ten dollars a week. The restaurant changed hands about that time, at the end of my third year there, and the new owner raised my wages to \$12. I closed the place many times; I was responsible for locking up and running it the last two hours of the evening. So I was doing very well financially. I gave up my cleaning job at the end of two years. I became an assistant to Brown and earned as much as \$500 a year, in the lab, as grading assistant and his consulting assistant.

He had a lot of cases, but one of them was a big one, the Root Refining Company patent case with Universal Oil Products. I became Brown's consulting assistant, and we did a lot of phase

behavior work. In the meantime I'd taken my doctorate degree problem on phase behavior, calculating vapor/liquid equilibrium. Brown and I did not fully understand the trial and error concept that you have to use for material balance, and I thought I could find a way of avoiding it. In the lab where I was the assistant I ran the Podbielniak column using light hydrocarbons. I was familiar with a fellow named Professor Good who worked with Professor Leslie on vapor equilibria at oil temperatures. I also ran distillations with the 2-liter 6-foot column. I helped Roy Wilson who was getting his doctorate degree with Brown. Brown got Union Oil of California to take vapor and liquid samples from a crude distillation fractionator, so that Roy had a liquid for the vapor phase and a liquid from a plate, and I ran the distillations. I'd work all night sometimes, have an alarm clock to wake me up and take the sample off. It must have been two liters that we ran. I devised a molecular weight apparatus and I measured the density of the fractions. I don't know how many distillations I ran; he must have had 30 samples, and Roy ran some but I probably ran fifteen or twenty of them. I actually helped make the column itself, with a glassblower from physics. Also, for example, Brown brought in a sample of material that was sent to him from East Texas. It was an oxygenated material. We had a copper condenser on our fractionating column, and when we ran the distillation we got two layers of liquid condensate, one of which was blue. We never knew quite what it was, but it may have been oxygenated propane or butane.

I enjoyed the lab, I enjoyed working with Brown, and I gradually became his chief of graduate students. I had fourteen people working with me at one time. Brown was at MIT working with Warren K. Lewis and other people there, and one afternoon (as he knew I was at the restaurant in the evening) he spent forty-five minutes on a phone call to a student, which quite impressed me.

Two streams, one from a Dubbs cracking unit and one from a crude oil distillation unit were combined in a separator at 900°F and 500 psi. They gathered the vapor and liquid samples, and collected a sample of the crude oil from the separator. I also had the crude oil sample before it had been heated. In any event, for my thesis I put in the vapor/liquid equilibria and the equilibrium constants that I'd developed. I ran the vapor/liquid equilibria, and the issue was, how much of the bottoms came from the cracking and how much from the crude oil? Brown and I devised this material balance concept. If you take so much reduced crude oil and so much cracking stock, by running the calculations separately, but in the proportions in which they were mixed on a molal basis, you can find the amounts of cracking stock and crude that contribute to the liquid state. Brown used that in the Root Refining suit and I put it in my thesis. Of course, I did other things in my thesis too. I finished my thesis in May of 1933.

BOHNING: What were the other things that you did?

KATZ: I got the Bahlke and Kay phase behavior data that they had run at Standard of Indiana (4). I had one of the Sydney Young type columns in the lab where I could measure mercury displacement and I tried to match what they had done. I made vapor/liquid equilibria calculations of natural gas material. Brown and Souders were working on the fugacity concept which they published in 1932 (5). I made equilibrium constant charts for the boiling points of fairly high hydrocarbons and I extrapolated out to 1200°. I ran distillations down to very low vapor pressures, maybe four millimeters. We went to very low pressures, when I worked on the vapor pressure chart.

Hal Coats had taken his doctorate degree with Brown on the vapor pressure chart; I extended it and found some new data in the literature. I started to read the literature of phase behavior and I found Sydney Young's articles on the PVT properties of iso- and normal pentane (6). I found the PVT properties for hexane vapor in the superheated region which I added to my thesis and we ran a compressibility factor chart. Pressure volume temperature relations; I looked at the phase behavior of the pure substances, and read a good many of Sydney Young's papers and was very much impressed. I went back and found his thesis. There was another person, a German, who had a method and I went back to read his early work in German. Our paper was published in December 1933 (7). Brown had given it at a meeting somewhere, and the paper was published; "Vapor Pressure and Vaporization of Petroleum Fractions."

BOHNING: I have some questions if I could back up a little. Before we leave Michigan for Oklahoma. There was a period before you went to Michigan when you weren't in school. Were you still doing a lot of reading at that time?

KATZ: Yes, I went to Jackson library, but it was only novels and such things, not science. I didn't do much reading when I was working. We drove back and forth back to Jackson. I had to get there at six-thirty in the morning, and work until five at night, ten hour days, you see. Anyway, I did not do much serious reading; I was discontented, I didn't know what I wanted to do. But I had a good incentive to go to college. I was running a big press that made the Chrysler 14 inch brake drums. I had to dip the steel disks in dope, as they called it, an oil/water lubricant mix. The plunger was only raised about two inches and I had to get the blank in there just right. If I missed it they had to stop the machine, which took four or five minutes, and then reverse it. The foreman would have to grind the dies again, so I was very careful.

I was running that machine one day when it was so scored that they had to stop it. We had a wonderful assistant superintendent at the shop whose name was Popp. He was my

friend, he gave me breaks in running the machines and other jobs. For example, my stepfather was the shipping agent and in the summer of 1927, when my stepfather was on vacation, he let me take his job. I shipped, put the stuff in the freight cars, had to make records of what was being shipped, sealed the cars up; he really was my friend. Well, the superintendent was not; he was very stern. When my machine was being honed down I sat down on a bench by my machine and he came up to me and said, "What are you sitting down for?" I said, "Well, I've just been handling about twenty tons of steel, and there's nothing I can do as I'm waiting for the foreman to clear the scene". He said, "You can't sit down in my shop." I thought that was a very good incentive to go to college. But I did want to learn; it took a little effort to learn because I'd been out a year and I was twenty when I entered college.

BOHNING: Were chemistry and chemical engineering combined at Ann Arbor then?

KATZ: Chemical engineering had moved into the East Engineering Building in 1923 but prior to that time they were together in the Chemistry building. They were closely allied and many of the chemistry faculty had taken chemical engineering when they were undergraduates and their doctorates in chemistry. I got to know the chemistry people very well.

BOHNING: But how did you select that as your major when you entered the University?

KATZ: I really didn't know the difference until I got there when they had a Freshman Week, for the first time. We had a whole week of introduction to the university, and it was fine. I had Professor Upthegrove as a mentor, he was a metallurgist. At first I had to struggle pretty hard, but I had the time; I only worked noons and two afternoons a week. I got good grades except in my English, where I always got B's. Things went along well. In my sophomore year I was taking twenty hours and then I got the evening job as well as the noon job. That second semester I got several B's, as well as a couple of A's but I never got any C's in college. This was my hardest time, when I was taking physics.

[END OF TAPE, SIDE 2]

As it turned out I was working as much with Brown. By my junior year, I was working almost forty hours a week. But I knew what I was doing, from eight in the morning until eight at night for six days, but I could go home on Sundays whenever I wanted to, which I did mostly. Being older I was stronger physically so I was able to do things. I had a good rooming house between the restaurant and the University and I really enjoyed what I was doing. I had made my plans to get my Ph.D. relatively early.

BOHNING: Can you tell me something of the faculty? What was the department like when you were there?

KATZ: One of the things that brought me back to teaching in due time was that I had pretty good teachers. Very good teachers in some cases, and the others were not that bad. All the teachers I had were professional people. I had Brown of course, who was an engineer. He taught me my combustion engineering material balance calculations. I really enjoyed that. He made me his assistant a year later. Professor Baker was interested in electroplating and he was the Chief Engineer of the Houdaille Hershey Company in Decatur, Illinois in addition to his teaching position. Professor Warren McCabe was an up-to-date teacher who showed us material balances and optimization and this concept of energy availability. He got an award later for the concepts that he'd published, and, of course, for the McCabe-Thiele diagram in distillation (8). So he was relatively famous. A. H. White, our departmental chairman was president of AIChE. In 1929 I was aware that my teacher in organic chemistry, Professor Gomberg, was the president of the American Chemical Society, and the head of our department was the president of the American Institute of Chemical Engineering. I was aware of it at the time.

BOHNING: They were both presidents at the same time?

KATZ: They were presidents at the same time. I did not have A. H. White as a teacher but he conducted our seminars.

BOHNING: You had Gomberg as a teacher of organic. Could you tell me something about him?

KATZ: He was very quiet and modest. When he was president of the American Chemical Society he told people in his department that he didn't want to countersign ACS application forms while he was president to avoid any suggestion of influence. One day a student walked up to him in the laboratory and said, "I'd like to join ACS; would you sign for me?" Professor Gomberg hesitated a moment, didn't know what to do or say; he was rather modest. The student says, "Well, you're a member aren't you?" And Gomberg said, "Yes" and signed it without another word.

Anderson was one of three young faculty I had in organic chemistry, as laboratory or recitation assistant in charge. Gomberg gave all the lectures. He was a good lecturer, and I enjoyed organic chemistry very much.

BOHNING: When did you make the decision to go to Engineering?

KATZ: We went directly to the Engineering School. The first year you are in General; you check Chemical Engineering off in your sophomore year, but we were separate at all times. We took the same chemistry as the chemists, I guess they took one or two more courses. I think they had to take an extra course in quant and they had to take the lab, but I took the lab and physical chemistry in the summertime. Professor [F. E.] Bartell, I must say something good about Professor Bartell. I took his colloid course as a graduate student and learned surface chemistry in a qualitative manner.

Bartell was a good teacher in the sense that he let you know what his research was. I don't think the other chemists told us about their research. I made at least three research advances in my career based on a feel for surface chemistry. I don't know if it's the time to mention what they were?

BOHNING: Well, we can while we're talking about it; we will be backing up again later, but if you want to.

KATZ: Let me tell you what they were. Bartell was working with an API [American Petroleum Institute] grant on surface properties and developed a wetting tube device. The tube was packed with oil-field sand, one fluid was put in one end and the other fluid in the other end so as to develop an interface between them. A pressure difference arises between the two ends from which you could determine the wettability. I learned about this work (9), about wettability and adsorption.

When I went to work for Phillips in the summer of 1940 I was working in Oklahoma City as an oil-field engineer. The question was whether the field should be water-flooded or whether some other way of secondary recovery should be tried. I went to the well when they were bailing the sand out and I put some in a Bartell tube, and I found out that water could not drive that oil out, it didn't move one iota. It became clear that the sand was oil-wet. This was the first observation that sand was not always water-wet; it was oil-wet in this case. I went on to show that dry sand placed in any crude oil and tested in a Bartell tube had lost its water-wettability. Asphalted substances in crude oil adsorb on sand and the only way to get them off is to fire it. You can't even get them off by Soxhlet extraction. That was the first one.

The second was with mercury. There are problems with boiling mercury in a stainless steel tube. At the time, I was consulting with a company in the field of liquid metals. I visited General Electric where they had a mercury boiler, and I met the man who found how to correct the problems they had with it. He found that 0.20% of magnesium, and 0.002% of titanium added to mercury will enable it to wet steel. I showed the change

in the boiling coefficient with addition of various substances as compared to film boiling in their absence. I had this as a paper with one of my doctorate students (10). I remember I showed it to McAdams, the famous heat transfer man at MIT. That was another example of my understanding of wettability and its application.

The last one was a problem I discussed in my seventieth birthday lecture and I intended to bring you a copy. The Bureau of Mines ran a meeting, "The Dumping of Liquefied Natural Gas on Water"; I was Chairman of the Committee of the Academy of Sciences Research Council for the Coast Guard on Hazardous Materials. They brought a problem to me. They dumped 100 cc of LNG [liquid natural gas] on an aquarium in the lab; they did it many times and the ninety eighth time it blew up, ruptured the glass in the aquarium but it did not ignite. Nobody got hurt but it was a shocking experience. Next they ran twenty gallon dumps on a pond and recorded them with a 16mm movie camera, running at 32 frames/sec. They watched the cloud of LNG go off into the atmosphere, and blow away with the wind. I think it was the fourteenth dump, when on the second frame after it hit the water it went up. They likened it to a stick of dynamite; this was unknown behavior. David Burgess who was in charge of the Bureau of Mines in Pittsburgh came to me and my Coast Guard Committee. He asked whether hydrates or something had formed. I told him that hydrates don't form and decompose in 1/32th of a second.

I spent a year working with various people. I had Bob Reed at MIT, Sliepcevich at Oklahoma, Churchill all working on the pioneer LNG ship to bring liquid methane into England and France. I talked to a lot of people, and finally I found a Japanese Journal of Physics article describing what they called the superheat limit (11). When you try to heat immiscible liquids, because it's a liquid/liquid thing there is no nucleus for initiating boiling. The critical temperature for water is 700°F and at about 575°F it will spontaneously decompose in a shock wave type of explosion. We decided that must be what was happening: but why?

Well, I got invited to go to Shell Oil Laboratories in Houston in September 1971. When I got there I found some bright young men had been running this experiment where it was very clear. They took a five-gallon bucket, put a gallon of pentane in it, they had a gallon of LNG on a trunnion, put the bucket on some two by fours over a tank, and had Plexiglas for protection. When they dumped the gallon of LNG on the pentane there was a shotgun report. It knocked out the bottom of the five-gallon can, it broke the two by fours, but it didn't rupture the barrel of the can. Now it was clear to me; I found out what the problem was. It was the wettability of the hydrocarbon. They showed from their experiments that if the LNG was less than half methane, and the rest ethane, propane and butane, that it would wet the surface. The surface heat transfer was so fast that it reached the superheat limit of temperature rise; and it went so fast, they said in a nanosecond. Anyway, I had this feel [for surface

chemistry] which I attribute to Professor Bartell.

BOHNING: You commented that he was the only chemist who let you know about the kind of work he was doing. I get the impression the chemical engineering faculty were just the opposite.

KATZ: That is correct. In the first place our system was this. As an undergraduate you had to choose a research problem, you had to go to seminars and listen to what everyone was doing; as graduate students we had a weekly seminars where you heard each doctorate student tell you what he was going to do, and when he finished, what he had found out.

There were 80 graduate students in 1932, at least 45 of them were working on their doctorates. At one time Brown had fifteen working with him. I could tell you at the end of the year what everyone was doing, and what they were trying to find out. We really knew where the research was going. McCabe was working with crystallization, Baker was working with electroplating and that sort of thing. A. H. White was working with natural gas or Gas Association projects, with heating wood and so on. Brown was working on thermodynamics, equilibrium and that sort of thing, and Badger was working with his evaporator. Not only that; they would give you a problem coming from their consulting work. We knew they were all doing consulting work.

BOHNING: You developed that industrial relationship very quickly then?

KATZ: Yes, as a matter of fact Brown had consulting jobs, including one with Phillips Petroleum. Ted Legatski, one of our graduates from earlier days had come to spend three months with Brown on a patent case and I got acquainted with him. He was the one that got Phillips to hire me in late May of 1933.

BOHNING: What type of laboratory experience did you have as an undergraduate?

KATZ: In engineering we had the fuels lab. Combustion of oil, gas analysis. We had a unit operations laboratory where we ran large scale equipment; we had a salt evaporator that ran for twenty-four hours trying to make salt from a brine. We ran heat exchangers, metering equipment, things of that kind. It was a good course with large scale equipment.

BOHNING: What texts were they using?

KATZ: We used Walker, Lewis and McAdams (12), and I guess a part of the time we were using Badger and McCabe's notes, as they were getting their book ready at that time (13), but they didn't finish it while I was an undergraduate. They probably finished it in 1933 or 1934. In thermodynamics we had Brown's lectures, and we used that book from California, Lewis and Randall (14). Later I met Randall, but never Lewis.

I gave my first papers as a young chemical engineer; I went to the Chemical Society meetings later on. I'm a fifty-year member; I joined in 1932 to get Industrial and Engineering Chemistry as a student member.

BOHNING: What effect did the Depression have?

KATZ: I was not too depressed, but everyone around me was. My landlady lost her house. I married the landlady's daughter so I learned more about that later. My folks were very hard up on the farm. They went to California while I was a student to see my mother's brother, just before he died. I told them that if they went I would give them fifty dollars. At the end of five years in Ann Arbor I had \$1000 in the bank. I got married that Fall, and I had an automobile; I'd done very well. But I saw the Depression through the poor people in the restaurant I'd worked in, and I was well aware of it. My first wife graduated in 1930 and went to be a teacher at Bellaire, Michigan. I used to go weekends to see her three or four times in the year. I drove the car up there, got permission to drive the car home; we had a driving ban at the University in those days, and they really did enforce it.

BOHNING: Do you remember any of your student colleagues?

KATZ: There was quite a group of colleagues. Pete Mercus was a Dutch boy from Grand Rapids, and he and I worked together. One summer when Brown was away for the summer I was running some of his projects and did several things through his office. A gallon can of oil was sent to us to find out what sort of additive it contained. Well, that's kind of hard to do but I promised to try; we aimed for a budget of five hundred dollars. I hired Pete Mercus to do part of that. We ran the iodine number, we ran distillation on the oil, surface tension, interfacial tension, index of refraction; we did all kinds of things but we couldn't find anything. We got a Sears and Roebuck lubricating oil, and ran parallel tests, and we could see nothing in that difference. That was the kind of job I did one summer. Pete Mercus was my close friend and co-worker.

I learned something from an accident he had in his research. He was working on a Gas Association Fellowship and was running the combustion with oxygen in a bomb calorimeter. He had gotten

a new pressure gauge, a differential gauge from 300 to 600 pounds instead of one starting at zero. He put the gauge on, had his bomb all hooked up and was putting soap solution to check for leaks, and he happened to duck down, looking at something on the floor, when the gauge blew out over his head and took the window out across the laboratory. I heard the noise and went into the room -- he just sat there, pale white. We found out that the oxygen and the oil left in the Bourdon tube had reacted explosively. Pete became a vice-president of Shell but had to retire at an early age; did a lot of sailing in the Virgin Islands area but I didn't see much of him, and he's gone now.

Bob [Robert Merrill] Hubbard was another classmate. He complained he had to go to Europe one summer and couldn't be with us, but had to be with his parents. He was running a rolling ball viscosimeter with pentane. He was there when I came back [from Phillips], in fact, several students were still there when I returned. He taught at Virginia most of his life, he's retired now but still living there. These people that came back to the University to get a doctorate degree were the ones that were really hurting; [Richard] Lee Huntington went to Oklahoma. To show you how bad it was in the Depression, Lee would be a classic example. He had been running gasoline plants with a bachelor's degree from Oklahoma. He went to MIT a year; I never knew whether he passed qualifying exams but he came here and did a thesis with Brown and got his Ph.D. in October 1933. I'd only been at Phillips since the summer. I'd learned that he was in town and when I asked was told he was down at the auto lab. I went down to the auto lab and there he was cleaning gasoline cans. I said, "Lee, there's something wrong here. I'll try and talk to the management." That day Dean Carson from Oklahoma was coming through town looking for a man to head up his chemical engineering department. Lee came to see me and said, "I learned he was here, should I inquire?" I said, "Don't worry about your job with Phillips. They send you cleaning gasoline cans after ten years in the industry and with a doctor's degree. You call him." He called Carson at Ponca City, got the job and left.

Don [James Donald] Lindsay had also been in industry. He had a family; I think Lee Huntington did too. Don finished his degree and he went to Texas A&M. There are quite a few people like this; Roy Wilson went back to Standard of Indiana and hadn't finished his degree, and I was on his committee when he came back in three years. Otto Miller was one of my classmates, he went to be Chairman of Chevron. I was in California when he became vice-president for Refining, and I saw him there then. I've worked in California some. George Holbrook stood in the Ph.D. line with me at commencement. We kept in touch throughout and I succeeded him as AIChE President.

BOHNING: You had mentioned earlier the connection with Phillips that led to your getting the job with them. Had you looked at other possibilities?

KATZ: Yes, I'd looked at Texaco. The man at their laboratory up the river in New York State, I believe Poughkeepsie, almost had a job for me. He was their California man who was acquainted with me. He said, "Just wait, I'm going to get you a job," but he didn't. I went to Phillips, because of Ted Legatski and George Oberfell, who headed up the liquefied petroleum gas marketing in the Michigan area, Pontiac and Detroit, that area. He had an office in Detroit. I went there and interviewed about a week before graduation. About the end of May I got a wire offering \$125 a month, and of course I accepted.

Karl Kammermeyer, who went to Iowa eventually, got the same wire. But his wife had a job at \$110 per month as a secretary at the University, and he said he could not afford it. Anyway I went to Phillips on 23 June 1933.

[END OF TAPE, SIDE 3]

BOHNING: When you took the position with Phillips -- had they told you in advance what your responsibilities were going to be?

KATZ: No, except that it would be in the Research Department. I got there and met the thirty-two or so people in the research lab. It was a hot bed of unspoken problems. In 1933 no one had been hired and no one had quit for four years. No one dared quit for you couldn't get a job elsewhere and Phillips held their men. They had not hired any new ones until two or three people joined about a month before I got there. They had a lot of research going on in refining, especially in converting light hydrocarbons to useful products, but nobody was studying oil reservoirs even though the oil division was the largest division in the company. They said, "Nobody knows anything about crude oil production, natural gasoline and the reservoirs from which they come. We're going to give you the job. Go out and watch them drill wells and see what it is all about, then come and tell us what it is that we ought to do."

I was a little disappointed because I was geared towards refining through my work with Brown on the Dubbs case; the Texaco job would also have been refining. But I went at it and it was fun. At that time my wife didn't come with me [in the field]. In the summer of 1933 they took me to the Seminole area where there was a well being pumped by a bottom hole electric pump. Reda pumps. Reda Pump Company was in Bartlesville. The Phillips employees owned stock in it and they were promoting it in a general way, and it was a good idea, of course.

They had a bottom hole pump that was seven or eight inches in diameter with 120 centrifugal stages and an electric motor of the same diameter. I watched them pump a well at 4000 feet. I watched them drill wells. I talked to the superintendent and the engineer and the roustabouts on the job in Seminole, producing oil. So I got the feel for that. I was sent to Oklahoma City.

The Oklahoma City field was up to 1500 to 2000 pounds pressure; just the south end of the city had been drilled at that time. They didn't know where the rest of it was, they didn't know how big it was, but it was a gusher. They had 100,000 barrel wells there. The Oklahoma City Wilcox sand was terrific.

So I got acquainted with the engineers there and I watched them when they were going to put in the Reda pump. I watched them clean out a 6500 foot well and I began to learn about temperatures and pressures. I got gas samples, and brought them in for analysis. The Bureau of Mines at Bartlesville was a great help to me. I went there to see them and met their chief engineer who later became head of the East Texas Salt Water Association and chief man at Humble, Carl Reistle.

Ben Lindsly had a bottom hole sampler which went to the bottom of the well, got oil under pressure and brought it out. He ran the shrinkage, the solubility of the gas in the oil; put it in the glass window still and watched the gas come off and the oil shrink. He measured the quantity of gas and got compositions. Phillips would run the analysis for him at some later time. I had learned to run this and I found out that Phillips had a bottom hole sample at another reservoir and I requested a sample. They sent it to me in a little cylinder, and I ran the first bottom hole sample in Phillips. It had 26 mole % methane in the liquid, it was really something. I found out later that Mike Haider, who later became chairman of Exxon, was the engineer at Seminole and had run a sample in the summer so those were probably the first two that were run.

It became clear that if I was going to help the gasoline department I needed to know equilibrium constants, especially for methane and ethane. They got a man to build a cell for me, and I got the equipment to run it. By December of that year we were running vapor/liquid equilibrium constants from the samples. We were all through by September of the next year. I ran the liquid analyses, and another fellow ran the gas analyses. Karl Hachmuth ran the equilibrium equipment; he was a Michigan man, whom I got to know well. He was three or four years ahead of me; he ran the unit and correlated the data. We found that the equilibrium constants would go down with increasing pressure and then turn around and come up to an apparent convergence pressure. It was really something brand new which nobody had appreciated previously. That was one of the projects and one that was finished within a year and half after I'd started.

Another: I suggested a field unit to go out and test wells. To get a bottom hole pressure gauge, some temperature measurements, for which we used maximum reading thermometers, upside down and straight up. We got the sampler, and we measured the solubility and shrinkage curves of the samples in the laboratory. By that time, they had a young man that was loaned to the Bureau of Mines, and was working at a laboratory in Oklahoma City. He became my assistant in January 1934; Bill Barlow later became the vice-president for research for Marathon.

He died relatively young from cancer. Anyway, we did field trials; maybe thirty-five analyses of crudes by the time I was through in September 1936. We went and sampled every new discovery well. We ran a flow test on the well, measured the pressure flowing at various depths, the gas/oil ratio, composition of the gas off the separator, and took a bottom hole sample. From the bottom hole sample we calculated the flash vaporization to show how, if the gas/oil ratio changed, as it normally would in a constant volume reservoir, what the gasoline content would be. So that was another project.

Then it became clear that they had never run core characterizations so, about my second year, I recommended a core laboratory to run the analyses. For example, they had a core of two hundred feet in Oklahoma City. They'd only run the porosity, but never the permeability. So I established a core laboratory. By that time I had three young men working with me, and we started running core analysis. We ran core samples and estimated the possibility of a water flood in a shallow oil field near Bartlesville. Also I got gas samples and found out that the gas composition had liquid in it, and so I began studying the reservoirs.

I did other little things: for example, when I was there first, the Reda pump and the motor had a connector that had to float with the temperature change. It had to be floating, so they put grease in it. The temperature rise in the Oklahoma field was 132°, but that was a shallow field and in a lot of places it might be 150°. The grease disassociates; they asked me to make a grease that did not disassociate into oil and soap. I did some study into grease compositions, and I made up a steam cylinder stock, aluminum stearate, and I made a high pressure cell to show that under pressure it didn't disintegrate with temperature. I went out to the Phillips grease plants in a little town named Dewey, just north of Bartlesville, and Johnny Raven, a full-blooded Cherokee, made me the grease for the Reda pumps and they used it for several years.

The second important thing I did there was when somebody asked what good the solubility and shrinkage curves were. I thought we could use them to make calculations, what we call material balance now. In the fall of 1933, in a week's work using data from the Oklahoma City field, I showed that with the solubility and shrinkage curves, the gas/oil ratio, production life, and the pressure changes, we could tell how much oil had to have been there to give that kind of behavior. I had published it as a paper in 1935 for the SPE (15), and it is the mechanistic method of getting a material balance on a reservoir.

We had great problems in communication. For example the man at Bureau of Mines, Lindsly, couldn't believe that methane was in the liquid state. He, a physical chemist, opened a book and said, "It says here that methane cannot be made and held in the liquid state above -116°F." I said, "Well, that's a pure substance; as a mixture, of course it is possible." The liquid

shrank when you took the methane out of it. He said it was in the gas there. He didn't understand it really. I asked him if he were present when the Humble people presented the material balance in an equation form; they called it the quantitative effect of rock pressure on gas/oil ratio or something like that. "Did you know that you calculate how much oil was there from that formula?" "No, nobody ever told me that." He had heard the paper, but he didn't understand.

Schilthuis, who was a good friend of mine, was the research man at Humble. He redid this formula, and had a parallel method at the time my paper was done in 1935. Of course, it's his equation that is used, and that's where my shortcomings in math were evident. I should have written an equation too, as well as my mechanistic method. I did the oil material balance, I believe, and he did the gas material balance. I think that was the difference. They were identical procedures, but the equation is needed for putting it on the computer and making it easy to run. Anyway, that was the second thing I did, besides my vapor/liquid equilibria, both of which were to become papers. Phillips was generous in letting me do that.

They were even more generous in this way. I knew four vice-presidents personally, well enough to go and sit with them. Oberfell was in charge of research. The vice-president for oil was Dimmit; I would report to him and his chief engineer, Don Knowlton. In effect I was working for them, but in the research department. Sands was the vice-president for economics, and I taught him how to convert gas analysis into gallons of propane and butane, and he was interested in that. The gasoline department, that was F. E. Rice; I got to know them and their assistants well. I worked for them also, in a sense, and there people would call me and ask me questions, and it was wonderful. For example, I had been studying the Fitts pool, and they were up to about a gas/oil ratio of 700 [cubic feet of gas per barrel of oil]. One day the chief assistant in the gasoline department, Ed Buddrus, called me and said, "Say, my man out in the field there says it's 2700 now." I said, "I don't believe it." He said, "He's reporting it," so I said, "Tell him I'll be there tomorrow morning at nine o'clock, and we'll go test wells." I drove the two hundred miles down there early the next morning, we tested wells all day, and it was 750 or something; he had gone out and asked some questions of the operator, not taking any measurements himself. So they knew that I was checking experimentally and that it was an ongoing concern.

BOHNING: You made the comment that your math background was poor. Could we go back and explore the reason for that?

KATZ: Yes, I took the four semesters of calculus, and a little bit of differential equations. My differential equations instructor was a graduate student and he wasn't very clear in explaining things. I went to math teachers when I was doing my

thesis, trying to solve this trial and error equation, and I never got the help I needed from them. In the graduate course they had a man who went to MIT to get the math for chemical engineering, and he came back and presented his new course on graphical methods. Of course it was advanced descriptive geometry, if you want to put it that way; it was very bad. None of our teachers, excepting McCabe, were what you would call reasonable mathematicians. Badger knew very little math, Brown was only so-so; he did learn a reasonable amount in thermodynamics, but he wasn't a good mathematician in a general sense. I never really learned it, I had to pick it up the hard way.

BOHNING: There wasn't a real math component built into the curriculum?

KATZ: Only this special course on graphical methods. That was the only course required, and I didn't take anymore. The poor teaching that last semester was such that I didn't have the hankering for math that I should have. When I started college I knew algebra, trigonometry, and that sort of thing, pretty perfect. But it was a real handicap. Later on I went into fluid mechanics and heat transfer; over the years it was kind of a handicap.

BOHNING: It was before you left Phillips that you began to put together this concept of reservoir engineering; is that correct?

KATZ: Yes. When I left Phillips I wrote them a report called reservoir engineering. A copy of the front page is in my papers (16). I tried to explain to them what was coming in the future, and what I had done. When I went back to the university I took on the job of filling in the gaps of knowledge. For example, retrograde condensation; they asked me what it was in my doctorate exam. I explained it to them as best I could, but as I look back on it I see that I didn't really know, and my teachers knew even less. I left Phillips to come back to the university because my family was here, I admired the opportunities university teachers had in consulting, the academic freedom, and the right to investigate, the teaching of young students, for doing what you wanted to in research. There was a harshness to company management people towards young folks who didn't agree with them. The harshness never hit me, but I saw it with other people, let's say who were less in charge of what they were doing than I was. You see, my bosses didn't understand what I was doing as well as I did, whereas in other cases they did, and there was a difference in their behavior.

BOHNING: So in your case they left you more alone, as long as you were coming up with results?

KATZ: I had wonderful people to work for. Ted Legatski was my supervisor, and [Richard C.] Alden was director of research and his superior. Their offices were aside the entrance to the room where I had my desk, and if I was doing something new, or changing a plan, I'd stick my head in the door. If they were not engaged I would tell them in a few minutes what was on my mind, what I planned to do. If they had no comments, they would thank me, and I'd walk on; if not we would sit down and discuss it for another ten minutes. When I saw Ted Legatski about two months ago he recalled that the energy that I had was unusual; that's what they thought of what I did. When I left Phillips, Don Knowlton, the chief engineer said to Alden, the director of research, "There's no reason Don can't take his reports for teaching, is there?" So I had thirty-five or forty memos, maybe six or seven reports, papers I'd written, like the one on equilibrium constants which I published after I went to the university, the material balance one which I published while I was at Phillips, and another couple of papers.

BOHNING: When during that three-year period at Phillips did you come to the realization that you wanted to go back to academic life?

KATZ: Brown came to the Natural Gasoline Association meetings in Tulsa and visited with me. Then in February 1936 I went to New York to work with Kellogg on vapor/liquid equilibria because they were starting a program. On the way home I went to Penn State to interview people to be working for me. And I went through Ann Arbor to help someone from Bartell's group to come into the chemistry group. I interviewed him on the train between Ann Arbor and Niles and he did get a job offer and came to work for us. Brown was the one who kept inviting me to come back. McCabe left to go to Carnegie Tech and when he left they had a vacancy and they were going to hire two people. They hired me and Henry Rushton. He became a president of AIChE, and he was at Purdue for a long time; he's gone now.

BOHNING: So you moved back to Ann Arbor in

KATZ: September 1936.

BOHNING: And what was the situation when you arrived there? Had many changes happened?

KATZ: Not many. McCabe was gone. They had a teaching fellow, Jess Walton, so there were three new teachers, and the rest of the faculty was the same. I got to teach materials, with Brier giving the lectures. I had a hundred students in three classes.

A section of unit operations, I guess with Baker, and a section of organic chemical technology with A. H. White. In the second semester I had seniors who had to have problems, and a couple started on those. By 1938 or 1939 I began having large numbers of people working with me, got up to seventeen once. This plan of having every student do a research project before he left, meant that the faculty was forced to come up with ideas. Whether they were any good or not was another matter.

These students did all kinds of things. The first Engineering Research Institute was organized in 1920 for doing work for industry. A. E. White, who was in our department in the metallurgical group, was the director. In April 1937, the Eaton Manufacturing Company in Ohio asked us to come down as they had a problem. Little capsules full of carbon dioxide were sold for seltzer bottles, and they were filling them in a welding operation. The critical temperature for carbon dioxide is 90°F. They had a high pressure cylinder from which they were trying to fill them but had 30% rejects because they were so bad. I asked them what they really wanted, and they replied that they just wanted to fill them. I suggested to get some dry ice pellets and drop them in, and then you can weld the capsule in a few seconds. I took on the job and within six weeks I made them a pellet machine, producing something like fifteen a minute. Put the dry ice under the plunger, rotating it, cutting it off, knocking it out. You could get the help you wanted in 1939. You could get the machine shop to work overtime with gusto. When you asked for air cylinders to run things, they'd ship them the next morning. We did this project and we had it running in six weeks.

I learned that dry ice was extruded at 20,000 psi. So I got the idea to give it to a student. Get a special steel and make a very high pressure cylinder with an eighth-inch plunger on it, pack it with any solid you can get off the shelf, and find its extrusion pressure. They ran maybe forty substances for me. As I recall, sulfur extruded almost explosively. But this would have been one of ten students working with me. I had one putting various organics on water to see how you can change the rate of evaporation. I did all kind of things, in addition to surface tension and viscosity; things I needed to know about hydrocarbons, vapor/liquid equilibria, and phase diagrams.

The next outside job I got was with Brown, studying the storing of gasoline, making propane and butane and how to handle it in marketing. That was with Brown. The third job I got myself. Because I had done work in phase behavior, the Southern Minerals Corporation in Corpus Christi asked me to help them on a gas well that was producing condensate; they knew I'd published on vapor/liquid equilibria under high pressures. Southern Minerals Corporation was a Gulf Oil subsidiary. In the Fall of 1938 I started with this consulting, and I invited Brown to come and join me on it, because it was a Federal Court case and I was not yet quite dry behind the ears. I thought I'd better have a man who understands thermodynamics as well. So we made these glass-windowed gauges, and did tests up to 3000 pounds in

constant temperature air baths, and we could see the critical, we could see the condensation, the retrograde condensation. I had two students who took these gauges and made the first phase diagram; the first phase diagram run was by a couple of students. And, of course, it was published.

I had some friends in court, so to speak. The chief production engineer in Chevron gave me a fellowship for a graduate student under my direction in 1938. Harrison Howell of Industrial Engineering Chemistry wrote to ask me to submit an article on retrograde condensation in 1939. Fred Kurata was by that time doing his doctorate on critical phenomena. In the meantime I'd gotten the graduate school to give me \$3000 to run a literature search on pressure phenomena and hydrocarbon data in the literature from 1860 from 1910, and thereafter through Chemical Abstracts. Prior to the Chemical Abstracts, 1908 or whenever it was, there were no indices to the European literature, Comptes Rendus or whatever, you had to thumb through those pages. So I had two students at \$1000 a year apiece, and they went to the literature and started from 1860, and it was eventually published (19).

[END OF TAPE, SIDE 4]

I myself was studying this stuff when I found Kuenen's articles and Kurata digested them (20). My competitors in the retrograde condensation paper (21) were Sage and Lacey at Caltech. I got to know them very well; I visited them first in 1936 on a trip to California. They provided me with data, the methane/propane data they sent me was very helpful. So I was a supporter of theirs, but they never ran visual experiments. I took the methane/propane blind cell data and drew the phase diagram. What a particular methane/propane system would look like; pressure/volume, pressure/temperature, percent vapor and liquid. I drew a diagram and I sent it to them and asked if it showed what they thought the system to be. They said, "Well, you plotted it from our data; that's good enough for us." I published it but then I thought we must run one, and that's when we did it.

These were the things that helped me. I also found in the literature something that no one had ever commented upon. As of 1910 the physicists and physical chemists who had worked on phase behavior dropped the subject like a hot potato. They may have written summary articles. Sydney Young had one in 1910 and he ran a retake in 1929 on vapor pressure and critical phenomena (22). But all the rest dropped out, so to speak. They dropped out so well, that the physical chemist and the physicist teaching phase behavior and thermodynamics of applied systems didn't even know the subject properly. Ben Lindsly saying methane wouldn't go in the liquid state. I had to relearn it all, and I found it was in the old literature. I found Villard, a Frenchman, who ran hydrates. Gas hydrates is one of my fields, and I started while I was at Phillips. Hammerschmidt had his paper on gas hydrates (23), but he didn't find this literature, he only found a review

article in German, around 1928. I happened to get well acquainted with Hammerschmidt later, because I worked as a consultant for his company.

I found Villard had run the methane hydrate to 4000 pounds; this was in 1895 (24). He had run the solubility of solids and liquids in gases up to 4000 pounds in 1895. He took iodine crystals and put gas over it (methane or some other gas) so that if you pressurized you could see the violet vapors come off the crystals. He put paraffin wax to see it disappear, and then he would let the pressure down and see these things precipitate on the walls. He understood pressure phenomena.

One way of saying it today; in refining you use temperature to vaporize, in petroleum production we use gas pressure. You can take any oil or liquid, put it in a chamber, put gas above it put the piston and compress it; by compressing it you can make anything vaporize except asphalt. I had a student who was going to study asphalt; we had a thirty-thousand pound cell but he never got to do it because the windows didn't hold well enough to do it, so he ran an experiment that was good up to 14,000 psi.

I found all this material in reviewing the literature; I found Andrews' article on continuity of the gas system in liquid states where the PVT data for carbon dioxide was given. I reproduced the Joule and Thomson article. I put my retrograde condensation paper in this bibliography (19). A summary of all the papers Sage and Lacey had published, of Sydney Young's papers, of my papers with Brown. I put in the original article on the discovery of critical conditions. I put in Faraday's discovery of hydrates working with Sir Humphrey Davy. The hydrate material is all in here. I was the person who published this, with a local company in Ann Arbor, and it really didn't get a very wide distribution. It's out of print now except through University Microfilms.

Another study I made at that time also gave me a reputation. You know what cycling is? A high pressure gas field, with liquids dissolved in it, will make you a couple of hundred barrels of liquid in a separator when you produce the gas. Humble Oil sold gas fields like that to Lone Star. Lone Star took the field, drilled the wells, produced the liquid out of it for gas, took the gas from the separator compressed it, put it back and displaced it. That is called cycling. Of course, this was preventing retrograde condensation, you see. By 1940, the operators that had these plants in Texas had me retained by mail although I never visited. I worked on the case where they sued Lone Star for cycling. Humble along with Standard of Indiana and Tidewater Seaboard organized a company (Distilling Engineering Processes Company) that exploited these patents. They had these patents and they sued Lone Star. In one week of deposition from the inventor we caused the company to disappear in 1943 and the subject to fade away. So I had an early experience with patents and with these companies. In 1941 when I worked with Phillips, I spent the summer, half time, studying the literature of high

pressure phase behavior in the field.

BOHNING: I've wanted to ask you, since you bought that up, why didn't you stay in Ann Arbor during the summer?

KATZ: Well, partly money, partly because I liked to work in industry. Phillips wrote and asked me to come back in the summer of 1940, so I agreed to come for twelve weeks or so. When I went there they had decided that I was not going to work in Bartlesville but to take charge of a group of engineers who were studying the Oklahoma City field. They were men loaned by the oil companies who had an office, a couple of secretaries and a draftsman, but the man in charge had done something bad with the funds. This was the Secondary Recovery Association and I was asked to take charge. However, because I was not a registered engineer then, they had to have another man to officially run the engineering group. He was the official engineer because he was the only one in our group who was registered. Of course, I went home and got registered myself, the next year.

Within twelve weeks that summer we made a study of the Oklahoma City secondary recovery. That was when I made the wettability study. I showed the mechanism of the oil drainage out of the Oklahoma City fields was gravity. We wrote a report for the Recovery Association. I knew Dean McGee, he's the one who introduced me to geology at Phillips. At intervals of three or four years I'd see him somewhere. He was in the same office when I worked at the Secondary Recovery Association. Gravity drainage and wettability were the two things that came out of that summer.

Phillips asked me to come back in the summer of 1941 and then I went to the Bartlesville laboratory. One thing I did was a study of distillate retrograde condensate. The patent was still in doubt, you see. The other thing I did was to help them run higher pressure vapor/liquid equilibria. We ran up to 3200 pounds when I worked there, and they got it up to 9200 when I was there that summer. I directed that work, and I also directed a study of compressibility factors. I had a paper with the Gas Processing Association in 1942 that showed that gas gravity was a good variable to represent the composition of a natural gas that didn't have a lot of non-hydrocarbons in it (25). That was a good experience too, and I wasn't gone long enough to hurt my graduate students.

BOHNING: How many graduates students did you have by 1940?

KATZ: I probably had two, three or four at a time. I had a very bad experience. It was partly my fault and partly the unreliability of a man who should have known better. Willard Wilcox was a teaching fellow and my first doctorate student. He

took a problem on gas hydrates. We had these glass-windowed cells and instead of having them in air baths, we had them in water baths with an ordinary plate glass window, no mirrors. I had worked on them in the room at 3000 pounds. Brown and I worked on them with a half-inch clear glass in front and we knew what we were doing. Willy was running the hydrate formation condition for natural gases. One day he came in and said to me, "I've gone to 4000 pounds," "Whoa!" I said, "Don't ever," and I made him promise me never to go above 3000 again. I said, "We just don't know." He promised me he would never go over three thousand again.

Now Villard said ethane had a critical in the hydrate, so Wilcox was looking for a high pressure critical, probably not understanding it, of course. It turned out that ethane liquefied and therefore stopped going up in pressure and temperature abruptly. Fred Kurata, my other student, shared the lab and he was in there about two weeks after this incident with me. Wilcox said, "I got her to four, and I'm going to five." Fred said, "I'm leaving," and had gone through the door and was walking down the hall when it went bang. The glass cut Wilcox so badly that he died on the way to the hospital. Awful; he had been two years with Dow Chemical in pilot plant work before becoming a teaching fellow.

That started me on the road to an interest in safety. But the project was finished. I went on with hydrate work, but with Master's students only. Wilcox got a posthumous doctorate.

BOHNING: Did you continue the development of the reservoir engineering concept?

KATZ: Yes, I did, in a sense. Now, when I look back, I and many people did not use the phrase "reservoir engineering", but we all understood it that way as a concept. We called it other things along the way. John Calhoun was the first, I believe, who began using it as a title of a series of articles which later became a book, Reservoir Engineering. But I thought of it as that way.

BOHNING: What were the other terms that you used?

KATZ: We would describe what we were doing; flow through porous media. We would talk about the dew point of a gas, the retrograde behavior of fluids, the effect of pressure on viscosity. We would describe the physical phenomenon itself, rather than use the term "reservoir engineering." I don't even have it as an index item in the Handbook. For example, one of the things that we did was to show that if you know the composition of a gas and you know the composition of a liquid you could tell if it is from the same reservoir or not. I did that on Rodessa Field in 1936, I guess it was, for Phillips. They had

a gas field producing gas and in the same zone, down structure, they owned an oil well. The Bureau of Mines ran the analysis and I showed my colleagues that the gas field was over the oil. They were blowing the gas off and selling it, and they shouldn't have. So Phillips management knew that, you see. That was a reservoir engineering thing, a new concept.

I had the phase behavior, Standing ran the vapor equilibria at 8000 pounds for me, and he measured the density of the vapor and the liquid with steel pycnometers. He is retired now; he went through the grades at Chevron, running the lab at La Habre, and then became their reservoir engineering teacher. He worked in Norway for two years after he retired from Chevron, and then he taught at Stanford for a while. Now he's at a retirement village south of Los Angeles.

Next was Fred Kurata; he got his doctorate with me. He was at [the University of] Kansas but he's gone now. He worked on phase behavior in the laboratory all his life, he measured criticals. He did one of the retrograde condensation articles (21). Charlie Weinaug did the surface tension (27), the formula that he came up with still holds. We're now looking at some parachors with a student from Iran for a meeting in September; looking at the parachors for the heavy constituents.

A group in SPE [Society of Petroleum Engineers] put together the best papers on phase behavior; I chaired a session and had presentations from some of the good men from the oil and gas companies, as well as from Bill Swift from the University of Kansas. On it went: viscosity was Leo Bicher (28). We had the rolling ball viscosimeter but this had a turbulence problem at high rolling rates. Actually Sage and Lacey did the rolling ball viscosity data on methane (29). A preprint was distributed. About three months later you got a new one; please throw the old one away and use this one. I knew them well enough to have them tell me what the problem was at the next meeting we went to. It turned out that, at high rates, turbulence in the crescent where the fluid flows by the rolling ball was such that in the low density region you didn't have a correct value and you couldn't calibrate it well. We didn't do that; I ran the methane/propane system, and at low density some of our data was not too good. The Reynolds method for viscosity uses only a small head of hydrostatic pressure to push the liquid through an orifice in a cell under pressure. I'm told that Ed Cummings had done it with ethylene at Purdue, and the people at the Illinois Institute of Technology in Chicago also did viscosities with the new type of instrument. What it amounts to is that it flows through a capillary more slowly under a controlled pressure.

For operating purposes in the reservoir work my data showed that the gas gravity could be used instead of molecular weight; this was accepted as a good contribution. I did work with the API [American Petroleum Institute] on prediction. If you know the composition of an oil, you can predict the shrinkage and the solubility. I got the industry to send me oil samples and their

results of solubility and shrinkage, and I came up with methods that from the gas oil/ratio and the gas and oil gravities, I could tell them a lot about the reservoir. I did this in 1938 or 1939. Dr. Standing made essentially a full-time career expanding that, plus phase equilibria that he did in his thesis. He has a book, Volumetric Properties of the Hydrocarbons (30). It was an expansion of this work I did on vapor equilibria, and very well done. Standing received the Lucas Medal from the SPE before I did. He's now a member of the National Academy of Engineering for his work. It was an extension of the kind of work that I had done, but done so well for practicing engineers that Norway wanted him to come and teach there; he taught engineers at Chevron, worldwide.

So I did have that interest. I dropped out of reservoir engineering for oil fields at that time. Well, I had several cases, one with Cities Service and others. But I turned some of my attention to heat transfer. I did that in the forties when people went to war. I took up their problems and did Foust's work when he was gone, and got into heat transfer with finned tubes. I started and Knudsen finished a book on fluid mechanics and heat transfer (31). I went into gas storage starting about 1949.

BOHNING: Could we talk a little bit about the war years?

KATZ: Well, we had a large group to teach from the Navy, on a three-semester year basis. I taught nearly all the time. I got two months off one year. One month I spent in Calgary working with the government of Alberta on the Turner Valley field. I was the government's witness on how to handle gas and oil production in the Turner Valley field. It was quite a thrill to be the government's witness, and then to go to lunch with the man in charge. They were fine people to work for, and I met some of the other people, Ralph Davis, for example, in gas engineering. Both of us got introduced to different methods of looking at things. The second month I went to Erath, Louisiana. I was there in April, near the end of the war. Roosevelt died while I was there. The work was on two-phase samplings with pilot tubes. We did a major project for the gas processor people, because they had all these condensate wells and they had to know how much condensate came from each because of the royalty. The plant ran the wells for many people. They knew what each well was supposed to do, and the yield of liquid for a unit of gas. They knew what the plant made, and they divided the plant product according to these individual well tests. It was a nuisance and very difficult, so we collaborated and showed how these things work.

The people in Ann Arbor did not get much into war research. Rushton came to Michigan to teach with me. He got into oxygen production during the war. I didn't get into war production work, neither did Brown. He was with National Dairy, I believe, or something of this kind. We did not get a major project like

most schools did. Brown later became Chief Engineer of the Atomic Energy Commission.

I did a lot of little problems. For example in 1943, Kellex Corporation, a subsidiary of Kellogg, who ran the Oak Ridge diffusion plant, wanted some tests run on heat transfer through finned tubes. Chrysler Corporation was making special finned tubes, and they wanted to place banks of these thin tubes to find the heat transfer coefficient on the outside of the tubes. George Cooper from Kellex was sitting with us and he was calculating the data as we ran it and reported it to Manson Benedict, who was doing the design. We didn't know what it was for at the time, but we were told that Chrysler had made a million feet of these intercoolers for the gaseous diffusion plant at Oak Ridge.

I was doing little things like that. Of course I was teaching all the time until 1946. I had a project with API on the design and construction of pressure relieving systems. This was started during the war, but ended afterwards. This was safety again. We visited thirty refineries or high pressure gas plants to see how they handled safety valves. What incidents they had with functioning or improper use; fires, of course, were one problem. I had an assistant, Nels Sylvander, who left there and went with the Library, Pennsylvania coal project, then to Pennwalt, where he became a director, and he's retired now. I called him the other day, on St. Patrick's Day, and I asked him where he was forty years ago today. He didn't know. I said, "Well, I'll tell you. You were on the top of the RCA building watching the St. Patrick's Day Parade down Fifth Avenue. I sat with you." We had been to the API headquarters that morning and had nothing to do in the afternoon. So we went up there and watched it.

This was another safety project you see, we have a booklet called Design and Construction of Pressure Relieving Systems (32), an Engineering Research Department Project. We actually built a small fire vessel and measured heat transfer. This was another safety project. We found many problems and I've done a little bit of consulting. Eastman Kodak had some problems with blowouts under shock waves and so on.

[END OF TAPE, SIDE 5]

BOHNING: What effect did the war have on student enrollments? You had the Navy people, but the regular graduate students?

KATZ: We had enough; the halt and the lame. Mike Rzasa did this [Katz displays book]. This was done during the war (19), he finished in 1946. I started it way back in the thirties, but I didn't get it completed. His wife typed it. Mike had poor eyesight, he had to get a draft deferment. He tried from Yale and from Connecticut. He wrote and told them that he was my assistant. "What was my job?" "An assistant professor." "A

fellow who is an assistant in the first place can't amount to much." But they didn't dare draft him because he couldn't see without glasses. Mike did a good thesis with me; he did the design problem with me for the gas processing industry absorber, a new method of handling gas absorbers. I have it in the handbook.

I had a round table of first generation immigrant engineers. I was the German, Mike Rzasa was Polish, Cheddie Sliepcevich was Serbian, Fred Kurata was Japanese, George Preckshot was from Latvia; we all were there. But George Preckshot got drafted and went to China. He wrote back and said he was on the Burma Pipeline Road, where the jetfuel was taken overland for Chiang Kai Chek in a pipeline, and they lost half of it in route. He was one of the patrollers. He wrote me a card and said that there was nothing wrong with China but the Chinese.

After the war we got up to 235 graduate students. We had a policy of not passing very many though for the doctorate, so we could have mostly Master's students. We had a large number of Navy, as I mentioned. We had an influx of a large number of Indians. We had as many as fifty Indian graduate students or advanced seniors. One of them was Hori Sethna. Sethna was a Parcee and returned to India. We had visited him in Bombay in 1968 and he's been here since. He had been designated as one of the outstanding one-hundred and twenty-five engineers, at the 125th anniversary of our University. He became the head of the Indian atomic work. He sat by Mrs. Gandhi, when she announced their bomb. By then he was head of their atomic energy agency. When I went there, I visited him in Bombay, then we went out to Tromby. I gave a lecture to his people out there. We've been in communication since about 1946.

We had fifty graduate students, one of them was [G.] Tripathi, who went back to Benares, put up a school there. He was Brown's student but he worked with me on a paper too (33).

BOHNING: What about your other colleagues in the department during the war?

KATZ: We had several of them who were reserve officers who got called up. Foust was one. I took over his heat transfer course, then I took over Brier's. Foust was in charge of the test center in Arkansas. I think Brier was at another test center in charge of explosives. [Elmore S.] Pettijohn went to the Navy and he was the executive officer on the ship that went into Guadacanal.

A. H. White had been a reserve officer in World War I. We had four of out of eighteen of us go. That's what gave us the effort to teach. I had children, and I was at the interim age. The question never really arose about going. By 1941 my children were about five and seven. I was always teaching students, doing things of this kind. If the war had gone on longer, I would have

been taken on as the technical director of a synthetic rubber plant for Russia, but it never got off the ground.

BOHNING: How did that association come about?

KATZ: Somebody was looking and came to me. We had a couple of meetings about who would be in charge if we were going to build it in Russia. But I didn't become further involved because the war was too close to its end.

BOHNING: Was that from the Office of Rubber Reserve?

KATZ: I think so, yes. I did something for the Reconstruction Finance Corporation; a study of making natural gas into liquids. The Carthage plant in Texas. If Gulf built the refinery using one of their gas fields, and it didn't work too well, could they salvage the refinery as an oil refinery? I looked at that problem for them.

BOHNING: I think you mentioned the Wolverine Tube Company somewhere along the line. You had a long association with them.

KATZ: Yes, I did. Ed Young took it over when I became chairman of the department. I passed it to him to share the responsibilities. My son actually worked with him on it along with another student. Ed had it until about 1980. A long association and a very good experience. We were their technical heat transfer people.

I would go and see the field problems. We went to places in nine states and gave two days of lectures on design using finned tubes. Had booklets and so on with it. Ed Young went into the insurance consulting part. It was a nice experience. We almost had a book on Finned-Tube Heat Transfer, but I had two books going and we didn't complete it. But we published some 25 papers on this topic.

Condensation we knew very well. Ken Beatty did a thesis on that, a lot of good work. Jim Knudsen did his thesis on it as did Don Robinson. Don Robinson went into phase behavior thereafter.

BOHNING: Was it at this time that you became involved in professional organizations?

KATZ: No, I did that early. I was very much impressed with the AIChE, although I didn't join them until 1937, when I got back as

a teacher, but I did get their annual volumes. Some of my friends at Phillips would buy the annual volumes and sell them to me. I belonged to the Society of Petroleum Engineers. I'm a fifty-year member there. I was active as a student chapter member. George Holbrook and I were on a student committee for revising the departmental curriculum. Get rid of a couple of courses and expand a couple of others. When I came back to Ann Arbor it had happened. You know we had the first student chapter of the AIChE in Ann Arbor; A. H. White did that. I became secretary of the student chapter committee in the forties, and I published the solutions of the student chapter problems. I did that first thing. I was on the accrediting committee. Then I got to run for council, had to run a couple of times to get elected.

I started the Nuclear Division. We put on the first large-scale meeting on the peaceful uses of atomic energy in Ann Arbor. We had 1200 people, over one hundred from other countries. I initiated that meeting in 1954 and was the program chairman. At that time, the American Nuclear Society hadn't yet been organized. Then I did a joint meeting through the Engineers Joint Council, seventeen professional and technical societies joined together, for a congress on nuclear energy in Cleveland, in December 1955. I was chairman of the meeting where there were 205 papers. The American Chemical Society was involved, amongst a lot of others. I worked at Oak Ridge in the summer of 1951.

BOHNING: I wanted to ask you about how you got involved in nuclear work?

KATZ: Carbide [Union Carbide Company] was getting professors to come down to Oak Ridge and become involved. I took the job in 1951 for eight weeks. Anyway I went there, I worked on homogeneous nuclear reactions and the physical behavior of fluids. Joe Smith and I wrote a bulletin from Oak Ridge (34); I think it was on water/nitrogen relationships and phase behavior. One of the students did his doctorate with me on heat transfer down there. I had worked on liquid metals earlier, with the Mine Safety Appliances Company and I wrote a chapter of the liquid metals handbook for industry (35). I met Admiral Rickover there. I keep a record of famous people I've met. I mean to write a page or two about them when I think of it. I met Rickover at his office when I was doing work with sodium, potassium and lithium at Mine Safety Appliances Company. I went to GE and saw their work on sodium. They asked, "Didn't I know I was talking about sodium in the nuclear reactors of submarines?" and I said, "No, I didn't know it before."

In 1951 there was a man from Carbide in charge at Oak Ridge. We went down at spring vacation for interviews, and I took the job there for the summer. It was a very nice experience. When I came back I helped our school get a nuclear reactor by taking one of our men, Henry Gomberg, down there. I got to understand this

area. I was on the visiting committee at Oak Ridge, and later at Argonne. Floyd Culler was there. They had me chairman of the visiting team, with nuclear people.

At the 25th anniversary of the formation of the Nuclear Society there was not one word in their article about the Ann Arbor meeting. I'm going to get someone to help me write an article [about it]. I've talked to one of their people and they said that if you send us something we'll publish it. There was a mixture of people from chemistry, mechanical and other engineering involved in our meeting.

I organized the nuclear division for the AIChE, I became a director, and of course I ran for vice-president, and made it, against Walter Lobo. I did Walter Lobo badly. I've known him since he was with Kellogg. When he ran a second time, I started a petition to get Stu Churchill on the list, and Stu beat him. But I hired him on an EPRI [Electric Power Research Institute] study of coal conversion. I have a high regard for Walter. His wife was not in very good shape, the last time I saw him, a couple of years ago.

BOHNING: You came back from Oak Ridge as chairman of the chemical engineering department?

KATZ: That's right.

BOHNING: How did that come about? Brown was chairman before you, is that correct?

KATZ: Yes. He became Dean. I was never anxious to be chairman, but I didn't want some people to be my chairman. Selection of a chairman was usually after canvassing the views of the faculty. I took the job but I was never particularly good in getting new people in, because I never had a budget when I wanted to go and hire somebody. We hired some very bad people, Brown and myself. The department was not as good as it ought to have been. That was my weakness.

My strength was to have an interest in all the faculty. I'd tell them that everyone should want to see every other person to succeed, try to help them, and never to throw stones in their paths to stop them. That was something that isn't always true. When there are difficulties at schools, it is usually that the people aren't willing to work together with each other, harmoniously, and help each other, but where there is an unhealthy rivalry.

I gave up the chairmanship voluntarily. I had a disagreement with our dean; I had several disagreements with Brown before he passed away. Brown was a very capable leader but

he made decisions by arguments. He'd argue with you. He and I had adjoining suites of four offices with our students. We argued in my office a lot. The library was across there; after a couple of years they closed the door to the library.

Brown was a very good man, but after he became chairman he wasn't anxious to have people dispute with him or talk against some proposal of his. Finally, when there was something he thought highly of, I was the only one left who would explain the reason why some of our people weren't in favor, but didn't care to say it. When he became Dean, he had one policy that I just couldn't accept. That was to plan new buildings but not to decide what the building was to be used for until it was built. He had an associate dean who had a policy of not putting any offices in a new building. I said that every man would want an office out of a laboratory, you've got to build those offices or nobody will want to go there. He did build offices but he didn't build enough and it's been all carved up since.

Brown and I did some things together; we had a book together (36). It was very bad to see him go. He never admitted what he had, his family never acted as if he had cancer. He was a great leader in many ways, especially in the field of thermodynamics, doing good engineering work for companies, teaching the concept of thermodynamics, and understanding what he was doing. Very good.

BOHNING: What changes occurred when you became chairman? Did you have any specific goals that you set for the department?

KATZ: Well, I don't think so. Brown in his last days as chairman put through the book on unit operations. Each of us helped. I was always his underling, so to speak. I knew things he wanted and would want it done, so I would do them for him. I helped to finish the book, although I didn't write too much of the text.

In those days that book seemed fresh, and this brought us a generous supply of students. We gave something like 10% of the chemical engineering Ph.D.'s in the nation during the decade when I was chairman. We were paralleling MIT, probably 90 or 95 % of their output. Our people were engineering-oriented. The current President of the AIChE was one of our students, but who didn't go on to a doctorate program. I wrote papers on such subjects as making a professional engineer (37); without having all their effort going into research, but getting some knowledge about management. Getting something of communication, and even making a doctorate in engineering. I made a proposal like that for the ASEE [American Society of Engineering Education].

I felt that our second-year graduate courses that some of the Master's men could take, really made them. I'll put it this way. We had a man call us once, and he had one of our Master's

graduates, not an outstanding student, but a good one. He wrote that he had hired lots of engineers, but never anybody who landed on his feet, who could be of use to the company in the first two or three weeks, like our man did. We had design problems for them. For example, design for taking the propane and butane fractions out of natural gas after it came to Michigan from the Gulf coast. I knew the two people who were designing for this. The losing bidder was a friend of mine, he gave me his design. We went to see the plant as it was being built, and watched the operation, and I gave them a course. The design course was to select the process design, and I would present the two alternatives and let them choose how they were going to do it. We had that kind of second-level design course. I stepped down as chairman in 1962 when we weren't yet geared into computing, but I did get into it. This is a subject we should discuss sometime.

I was really thinking of making each faculty member do well. Be successful, do research, do engineering practice, do good teaching, and prove his appointment to full professor after he'd done a lot of good work.

[END OF TAPE, SIDE 6]

BOHNING: We want to talk about the advent of computers and how that was introduced into the curriculum.

KATZ: Cheddie Sliepcevich had used the computer at Johns Hopkins or somewhere for getting some numerical functions for his light scattering phenomena (38). He was interested in computing, and some people in research were using the computer. I believe it was the 650 we had and I became aware that it was an upcoming thing.

Once, a man from Standard of New Jersey came and sat in my office when I was departmental chairman. I asked him, "How long would you say it would be for one of our new graduates before he was using the computer? Would it be two years, three years; how long?" He said, "Why don't you say two or three months?" I had to say to him, "You know, we aren't preparing our students for that sort of thing; it's time we did." So it was Sliepcevich and Joe Martin; he was a mathematical person too, who taught a math course. I suggested they get an example problem and run it through, then arrange lectures on programming for our faculty and graduate students. We held the first lecture in February, must have been 1958 or 1959. An icy evening in an old building in the University, and two hundred students and faculty showed up. That was intense interest. We put out an example problem, and we wrote a proposal to the Ford Foundation, who then financed a project for us at the University, for, I don't know, \$250,000 or \$300,000.

A fellow by the name of Woody Craig, I believe, was deputy to the head man of the Ford Foundation when they were going into

engineering. They gave MIT a million dollars then, or maybe more, and some, I believe, to Caltech and other schools. Our project was put together to give us funds to teach our faculty and graduate students well enough so that they would know how to put computers into the teaching process; for engineering itself not just for research. The Ford Foundation came back shortly after with a project, with a \$900,000 budget, provided we took it on as a national project, which we did.

I think we got it approved in 1959. I visited ten selected schools; MIT, Pennsylvania, Ohio State, Illinois, Wisconsin, Minnesota, University of Texas, Georgia Tech, and I suppose, Berkeley; anyway I visited ten schools. In all ten schools, I found only one problem that had been given to undergraduate students. A statement of a problem and its solution was available from the head of the computer center at Georgia Tech, who was also a professor of engineering. The only problem. I knew Gordon Brown at MIT through a committee, the Commission on Engineering Education, and I knew a lot of the other leading figures. The head of civil engineering at Illinois. So I knew some of the computing people. That was through EDUCOM [an inter-university communication council], I guess. Anyway, I went to Gordon Brown and he assembled some of his people because they had not yet addressed the subject. Always there were some older people who said that this would not amount to much, a mere flash in the pan.

The MIT man who was head of naval engineering said, "Folks, you're just off the beam, the most a student will ever do is this. You'll say to them, see this pack of cards, I'll put it in here and I'll take the print-out and I'll say, 'Now there's the solution to your problem.' That's as close as a man will ever come to the computer." But Brian, the committee man from chemical engineering said, "Oh no, you don't understand, the computer uses a new thought process, you have to organize yourself. The organization you have to do to put it on the computer will give you a new way of looking at things; that's the value of the computer. And, of course, the rapidity with which you can do it." The man from civil engineering was working on a program himself, calculating how much earth is required to make a new road, and that sort of thing. The young people, they just clobbered the old folks. I sat there and just listened. I remember the same thing at the University of Pennsylvania. Everywhere I went I found two or three keen, younger faculty. When I came back I selected 150 people over a three-year period. I got three years from the Ford Foundation, and two years from the National Science Foundation for the project on the use of computers in engineering education. We put on designs, on-line designs, from 1954 to 1965. Each person who came to us for their summer semester had to do some example problems; we published them with their solutions. Then we sent the volumes to every engineering school we knew of in the free world.

I went to Brazil to teach for a semester in 1963. That was at the end of the Ford Project, and the beginning of the design

one, and while I was there I visited Argentina. I went into a Dean's office to make myself known, and he said, "You must meet the young man who runs our Computing Center." When I met him, I saw that he had our reports and was using them. It was worldwide. At the end of our project we issued reports for the various divisions of engineering, chemical, civil and so on. And we had a big composite volume, with everything in it (39). We really started the thing, but it was very slow in coming. People who had not had it as a young person, did not learn enough to synthesize problems. I was one of those, you see. I taught a course of introductory computing, and I did the recitation, but not give any lectures. Now it's here, but it was slow in coming.

BOHNING: What kind of monetary support developed to provide the new equipment?

KATZ: Well, you understand the \$900,000 was used to help the Computing Center, but we tested small computers, the Bendix 15 or whatever it was, and another one. We tried the small computer in our lab, where people could come and use it or the bigger one, the 704 afterwards, finally the 360. What we found was that people would be happy to use the larger computer, because it would do extensive programs more quickly, but they had to stand in line two hours. So they'd come and do it themselves on the smaller computer. That's what we learned. It was really the discipline. We brought in a man from Bell Labs to lecture on computer appreciation, a course which should be given to every person that goes to a university.

We put on lectures. We had lectures on medical diagnosis with the computer, on synthesizing music. We tried to make an influence on the University. After we got this project, our math department started a course on the basics of computing, through the Computing Center. It was a cooperative project with the computing center. I organized a committee at our college, a man from each department and myself. I got zero cooperation out of the Dean's office, you see. That was another thing with the Dean; he wasn't going to help me. He appointed a new committee, and I already had one, didn't even know we had ours going. The Ford Foundation had one question. "Was I going to be in charge, if they gave us the money?" See, they knew the situation. Anyway, I was only the manager or the promoter, pushing the right people, I guess.

BOHNING: When computer science developed, did it end up residing in the College of Engineering?

KATZ: No, they had them both. But they've since merged, I believe. It's in electrical engineering now. Then they were the biggest foot draggers. They weren't into it yet. They were hardware oriented, not program oriented.

Elliott Organick was one of our graduate students in phase behavior, who went to the University of Houston as head of their Computing Center. I got him to come to be the technical director. He wrote the Fortran Primer, but he also wrote the MAD Primer, and we are using the MAD language, which is a superior system, at the time ahead of the recent Fortran 4. He died last December; he went to Utah to head up a computing science department, not an engineering but a science department at Utah. Before he went there he had been to MIT to consult with them for a year. They had him come to help them set up the educational program for MIT. He was our first man. I had another man from Kentucky, and the third man was Brice Carnahan, who's still with us.

BOHNING: How did the AIChE respond to the advent of computers in the education of chemical engineers?

KATZ: Very well, they have CACHE. It's now a private corporation run by chemical engineers for chemical engineering faculties. Monsanto gave them their process design programs, some ten or fifteen years ago. Our people were involved, and there is a division now in the Institute of Chemical Engineers. I've forgotten the exact name of it, but it has to do with computers in mathematics. Carnahan received one of the awards there. It's a national thing. Are you familiar with EDUCOM?

BOHNING: Only through something that I have from you.

KATZ: EDUCOM is an inter-university consortium. I was active with it because, in 1966, Jim Miller, who organized EDUCOM in the first place, asked the University to have me go to Boulder. I'd started the overview committee for university computing. So I went to Boulder for three months and helped them write a book called EDUNET (40). This was supposed to be a communication network, between universities with computers and for all uses of technology in helping universities function, both educationally and in business aspects. Miller went on to be president of the University of Louisville. He is a psychologist, and was head of Mental Health Research Institute in Ann Arbor. He's kind of a think-tank person.

I got on the Board of EDUCOM as a consequence and, the first thing I knew, they had me chairman of the Board. They had a man as president who was just ill-fitted for the job. Hired him without realizing that he knew what the universities wanted. He would not visit any. He said, "I don't need to go, I know what they need." For example, bibliographical data banks. How to find things. I had them all organized, ready to have a meeting on data banks, what they were, where they were, and he let it drop like a hot potato. I said we should get rid of him, and we

hired Henry Chauncey, who had just retired from the Educational Testing Service, as our chairman. We moved our offices from Boston to Princeton. He was an associate dean at Harvard, I think, when he went there in the first place. He's got Harvard, Stanford, MIT, and these schools going, and it's a growing institution now; 160 schools are in it. Our CACHE would be one of the ideal ones. I don't know how they handle the telephone lines, whatever they use for communication, but they do have a series of programs for chemical engineering, that's among their best. The best example for EDUCOM, showing how a profession should do this kind of thing.

We also have a Michigan State/Wayne State Computer Program, Merit, I believe it's called. I was aboard on that. I presented the budget to the joint committee of the legislature, to get this thing going as a line item in the state budget. We have Chemical Abstracts on line, and we share the cost between the three schools. I've been promoting things like that. But I'm not really a mathematician. I did take a look at informational science as it was coming along. In fact, I wrote a report for our college. I chaired a committee for the Engineering Foundation in New York in EJC, the three engineering organizations in the Engineering Center in New York. I was president of the AIChE, when we dedicated the building. I went there and met Herbert Hoover when he turned the sod. Chemical engineering has done pretty well in computing.

BOHNING: What's the situation in Michigan in terms of building it into the curriculum?

KATZ: It's coming close. I'm not really involved in it because I've been out of teaching for ten years. Essentially, computing is available to everyone, almost as much as they want. The teachers are using computers in their instruction pretty thoroughly. Textbooks are now being published along with a book of disks. In the book that I'm now working on with Lee I'm suggesting that he prepares a set of disks for problem solutions and essentially get those royalties.

BOHNING: I couldn't help but think, as I was looking through the Handbook of Natural Gas Engineering (2), how much help a computer would have been when you were doing a lot of those diagrams and calculations.

KATZ: Yes, it's true. For example, on the vapor/liquid equilibria, I've run at least one thousand calculations by hand. Slide rule and then a small calculator. When I gave lectures at Maracaibo at the Universidad Del Zulia for students of gas engineering, I came back one day and I said to my wife, "You know, they've got little desk computers, and they get the answers before I do on my slide rule." She said, "Well, it's time you

got a computer for yourself." And, of course, I have.

I had an American Gas Association project on retrograde condensation in natural gas pipelines (41), and of course I went back and renewed my work in the field of phase behavior. With a young Iranian student, Abbas Firoozabadi, and another young man, Hekim, I set up a program for depletion calculations (42). We used extended analysis. We'd go up to C15, C27 even. We'd even had generalized properties to C45. Take a reservoir and apply the computer program, and it will run off a dewpoint, then it will run off a depletion at specified pressure drops, and give you the composition at the well head. The composition of the vapors and liquids in the three separators in a row. It will compute all three. I'd judge it would take someone a full year to calculate what this thing does in two minutes. It's just astounding. Because, you see, often there are thirty constituents and simultaneous equations. It's just impossible by trial and error. So I did get into computing with that a little bit.

BOHNING: Since we've mentioned the Handbook -- I think what you gave me is the Russian title page. Could you tell me more about that book and how it originated?

KATZ: It is one of the things that Brown and I did for the University so long ago. A summer session Dean, Harold Dorr, wanted to start a continuing education program for industry, outside of the university's extension division, but it got down to teaching real estate and a few other things. Not much pay for the man who did it, and he only did it because he couldn't get a better job. It wasn't very good that way.

So we started this in 1953. The concept was that, first, we would make fresh course notes. Second, the teacher would be paid like he was doing consulting work, or something comparable, so that he could do it without financial sacrifice. Finally, it would encourage him to get the notes into textbook form. It started in 1953 with phase behavior, and at the end of the two-week course, we had a two-day symposium on phase behavior to which several of my former students came and asked, "Why didn't we write a book on this subject?" There are six co-authors, five of them actual students at the time and the sixth was a person I had worked with about twenty years earlier. They wrote certain chapters, and I wrote others. Of course I was responsible for everything in the end. I was going to call it a Textbook of Natural Gas Engineering. When McGraw-Hill got it ready to sell, it was so expensive; eight hundred pages, eight and half by eleven inches, twelve hundred references. Of the six hundred figures in there, two hundred and fifty were from my papers. Many quantitative figures are used in engineering. They said, "It's so expensive," and I said, "It's like a handbook." "Oh, we'll call it a Handbook of Engineering."

There was one little flaw that wasn't appreciated that I still have trouble with. The change in title immediately took it out of the college division and put it into the industrial division. They didn't even put it in the college book catalogue. I'd fight with them and they'd put it back in, and then later leave it out. I'm still having that trouble with the new book. It should be half and half; college division and the industrial. About fifteen years ago, someone from McGraw-Hill wrote to me to say that the sales were down a little bit, and that my royalties would be halved from now on. I wrote to the company president, "Look at the kind of letter I'm getting from your outfit." In the first place, the sales weren't down. But he fixed it up for me. In any event, it has sold now for twenty-six years. In the last six years, until maybe last year, it has sold better than since its second year. Half the sales are international. An Iranian that I got to know well is using my book. I visited him in Scotland two years ago -- he had to leave Iran, when the revolution came. He was telling his daughters about me, and I said, "Well you're really talking about my Handbook. Tell me, what's good about it?" "When I pick it up and read it, I feel like the person who wrote it, that I actually did this and understood it. Feel I didn't read it out of a book or an article somewhere." In a sense that's what made it sell. It's really a compendium of the phase behavior, reservoir engineering type material; not oil recovery, but for handling gas, that was known as of 1957.

BOHNING: Had you updated it at all?

KATZ: That's what I'm doing right now. But it can't be a full updating of everything. Some chapters are going to have to be left out, because I'm writing it as the production and storage of gas, hardly any oil recovery. I've got to keep the size down and make this edition saleable.

BOHNING: You did a lot of consulting work on pipelines.

KATZ: I did work on the natural gas pipeline. You have to get engineering studies of the reservoirs that supply the gas. Then you have to get storage places near the market, because you can't afford to bring all the gas needed on a cold winter day from the source a thousand miles away. So I worked on both ends. I started working on expanding the Michigan-Wisconsin Pipeline, which came from Texas to Detroit and Milwaukee. I studied the Hugoton reservoir in Kansas, and went on down into Oklahoma and Texas.

I did a little problem where I said I calibrated the geologist. That was a bad thing to say, because it really gave them an inferiority complex. Geologists are quite often qualitative although they're getting more quantitative all the

time. They looked at the core cuttings cut with a drill. They were going to look at twenty bags of cuttings for seven hundred wells. Twenty bags of cuttings each well, and seven hundred wells, to find what kind of porosity the cutting showed. They had a classification system from zero to seven, and I inquired what porosity is represented by number five. "Oh, we don't know, we are going to have to find some way of judging it." I told them that I would find out for them. I gave the same team of geologists, five of them, these two thousand cores, that had already been tested. They classified them and I took the data and showed what the grades really meant in terms of porosity and permeability. It proved that the eye of the geologist was very good. The data made a beautiful curve.

[END OF TAPE, SIDE 7]

That was the calibration of the porosity and the permeability values. It was very helpful and we won the case. From there I went on to finding the deliverability and so on of two other pipeline cases. One was the American/Louisiana, going from Louisiana to Detroit. The other one was from British Columbia and Alberta into the West Coast, the West Coast line.

BOHNING: Did you do anything on the Alaskan Pipeline?

KATZ: Yes, on the oil line. I reviewed a bit of this for a consulting firm. If the line stopped for a period of time, say three weeks, in the arctic winter when it was forty below. How long would it take for the oil to congeal so much that flow couldn't be re-started? The pour point on that oil is thirty-four degrees. We made calculations and looked like in three weeks they would still have some movable oil in the middle of the pipe head which was 48 inches in diameter. I'd been there before the field was developed fully, when just fifteen or twenty wells had been drilled.

I was the process design consultant on the proposed gas line. The chief engineer of Detroit American Natural Systems said to me one day, about mid-December, "Why not build a LNG [liquefied natural gas] line?" and I said, "Oh, it's too cold." I let it go but I went home and designed what I call a dense phase line. A dense phase is a natural gas pipe line, say, six to eight pounds per cubic foot, whereas a normal line is at 1000 pounds. Liquefied natural gas at -259°F is 27 psi. If we make it about nineteen pounds, and be just below the critical of methane, close to -120°F , and keep the pressure always up at five or six hundred pounds, never let it drop below that, and you can carry it through. I made a hand calculation design for the line, and, instead of 48 inches, I could use 36 inches pipe for the same flow. Questions came up such as nickel steel versus ordinary steel. Another question that came up was that state regulations were not available that applied to that kind of pipeline. Of course you'd only take a dense phase line through

permafrost, I believe. This concept is still being looked at. My son was at Arco, he's taken an early retirement because the whole group of the research people that he was involved with were let go when they had this consolidation last fall. They were looking at it for bringing it to, say, Cook Inlet, for Japan. As they couldn't take the gas from crude to Cook Inlet, which can be made year round shipping, they might consider a dense phase pipeline.

I also got into a similar area. Columbia Gas was trying to sell Esso an ocean shipment process; they wanted Esso to sign a secrecy agreement but they wouldn't. Finally they agreed to hire someone to arbitrate. I was selected. It wasn't a very hard job, just to interview both companies. It was this dense phase; they were going to build high pressure tubes and fill them with this high dense phase. They would not liquefy, would only cool it to -90°F and save on a lot of refrigeration. The only trouble was that they did not have refrigeration aboard, and they could not have it boil off. It was only for a short haul, across the Mediterranean really, but Esso decided that they couldn't buy it.

So I was involved in the gas line, but it never was built. Federal Power Commission required two applicants, and, of course, a second one came in and, through some environmental activity, they got the bid. They changed it from being the best hauler of gas, to going along the oil pipe line and the Alaskan Highway, regardless of the fact that you really didn't want to go that way. But the environmental people won, you see. But we did compare a dense phase and an LNG and it showed conventional. The people of the Alaskan line that I worked with, Williams Brothers, used a chilled conventional with twelve pounds per cubic foot of gas. But they had to put tires on it, because the pipe would have a long-distance ductile fracture if it hyperfractured.

BOHNING: I have a list of other things that you have been involved with, not in any particular order. The first one you had mentioned earlier, and that was that trip to Brazil. Were you on a sabbatical when you did that?

KATZ: No, I just took time off for a semester. I'd given up the chairmanship the previous year. I'd always wanted to do something of this kind, to be away. So I took a job of teaching one semester in Rio in the first half of 1963. Started a fulltime teaching program where the students studied during the day, not part-time. They had only nine students at their Master's level. They were all subsidized, people who were instructors elsewhere. I taught thermodynamics and heat transfer. I tried to look for a source of textbooks; the National Research Council, people like that, the library system. If you wanted to get the ASME [American Society of Mechanical Engineers] transactions, you had to call Sao José dos Campos, the school for the military there [Instituto Tecnológico de Aeronauticá], and have it sent because there wasn't a copy in

Rio, even though they have a school of mechanical engineering.

Last week I had a visitor who teaches there. He's trying to start a laboratory of phase behavior, especially of gas hydrates, with which I'm very familiar. He invited me to come down next year for a short period. Anyway, in 1963 it was time to renew my studies and get back into research, after I'd been department chairman for eleven years.

BOHNING: Did you do any advising on their curriculum?

KATZ: I visited Bello Horizonte and Sao Paulo to see the deans. Their university people were not really able to do good work because they all had other interests. Their pay was not very good so they had to make an outside living as an engineer and that is difficult. What they paid me, and it was considered quite high, paid my rent only, period. I also had a fellowship from the Organization of American States. But even with that, I figured it cost me \$10,000 to go there for that semester.

BOHNING: Did that develop any relationships with the people there?

KATZ: Several people followed me -- from Florida, somebody from Minnesota, and other places. It is now a going concern; the school has fifteen faculty, with forty graduate students. They're going strong right now so it did really initiate a program that Latin America had not had previously. I visited the University of Mexico in 1960. Chemical engineering or oil engineering, I don't remember which, had two hundred and ten faculty, but only ten on site, something like that.

BOHNING: We've touched on safety before, and your interest in safety developing out of that unfortunate incident in the laboratory, but you've been involved in a number of other aspects of safety as well. Could you discuss that briefly?

KATZ: Yes. I gave a paper for the National Academy of Engineering, about 1970 on safety in the petroleum industry (43.). They were getting ready to have a meeting on the topic of safety, and I brought together the senior safety person from Du Pont, Dow Chemical, Sun Oil, Natural Gas Pipeline of America, and I guess one other. I asked them, "What would you do with a ten million dollar budget to improve safety?" Sol Queener of Du Pont said, "We had eleven people killed at Louisville, where they were using acetylene to make synthetic rubber. For each plant, I got together the people who designed it, the safety division, the fire division, management and the operating division, even to the shift operator level." He explained how he required them to

exchange ideas and problems, to keep minutes and record the near misses, to make recommendations and to make annual checks on the action taken, to compare intentions with action. He said that over the next few years they had the finest safety record ever. This was part of my paper, and of course other people chipped in. As you know, the petroleum industry has a relatively good safety record. At that time, this would have been the divisions of the oil business; refinery, chemical, pipeline, and production. It did not include the auto accidents of employees traveling to and from work, but on-the-job accidents. There were seventy-five fatalities per year, for that whole big industry, which was relatively low. I tried to say this, I tried to work with American Standards Association, but they brushed it off, they didn't understand. I read every case of the seventy-five fatalities, to the extent that there were records in the API. In a few cases the causes were materials; materials failed or corroded. Most of them were management systems problems; somebody didn't know about this, or that, what to do. I suggested this systems review concept, and I told the Standards Association that they should have a model systems review for safety procedures, without being specific about any particular type of plant. Have this kind of policy that Queener described, with an input from your committees, just like you do with any other standard. Pipes don't rupture, because you have a good standard, you've got a boiler code, you've got all these other codes. That is what I recommended, but they gave me the brush off. It was about the time they were going through a change of name and organization. I don't even know what the name is now, it used to be the ASA, American Standard Association, its not that now. [American National Standards Institute; ed.] That's one of the things I was doing.

Another one was in 1964. The Research Council of the Academy of Sciences asked me to organize and chair a committee on Hazardous Materials, advisory to the U.S. Coast Guard. I did this from 1964 to 1972. We set up a wonderful group of people. For example, an organic chemist from the research department of Phillips Petroleum who was not at all involved in regulations. He was a wonderful person to come and classify chemicals; the hazards in fire, odor, inhalation, and skin absorption, auto-ignition. We got into pressure relief, the ability to ignite by electrical sparks and so on. We had a team of people who really worked. Someone expert in fire codes, people from the insurance companies, as well as chemical company people. The Coast Guard people were always sitting with us. I met Garnett who was the Coast Guard Admiral in charge of merchant marine safety. There was a barge in the middle of Cincinnati with chlorine on it, about to turn over. I was sitting with him discussing what he should do and what advice he should give the local people.

So I did this up to 1972. Oh, we had a meeting in 1969 in Rotterdam. Went to Rotterdam and met with the European community, and I chaired the meeting there. Rotterdam, which is not really a port but more of an industrial complex, particularly those involved in marine shipments. They had strict methods of

handling hazardous material. When you came in to port you have to let them know what's in your cargo, and they make you go certain routes if you have hazardous materials. There are certain rules you have to follow, and certain places you have to stop and unload. That sort of a thing. Barge traffic was important as well as ocean vessels.

I got involved a little bit in the very large crude tankers. One blew up and sank on the way from Africa to England or somewhere. The crude tankers going back empty would hose out their tanks rather than fill them with inert gas. When you spray water or any fluid but, I guess, water particularly, you can set off a spark. Just like a rainstorm and lightning, and that's what caused the explosion. Mobil built a new carrier, and they got the best experts but failed with the inerting. On the first trip they blew out hole in the deck that was as big as a football field. That's the kind of problem we were looking at. We had a man here in Ann Arbor, Arthur D. Moore, who was interested in static electricity, particles and that sort of thing. There are a lot of potential problems of that kind. I was trying to direct them into these matters but I thought that after about eight years I should back off and let somebody else do it. Then it deteriorated somewhat; the executive at the Academy and the Admiral's assistant at the Coast Guard didn't get along, and they called it all off. About three years later, they asked me to come back, which I did for a couple of years to try to start it up again.

BOHNING: Were you successful?

KATZ: Well, I never did know; it seemed to be going along. We were looking at medical problems as well. If a person was handling, say, five cargoes on a given day and one was benzene, and one of them was something else, would the interaction of substances from the various cargoes in the human body be more hazardous than the individual ones? We were still looking at LNG. We had this problem with LNG that I mentioned earlier. I wrote the report for the Coast Guard on the result of the superheat limit explosion, how it would happen.

I also analyzed the S.S. Marine Sulfur Queen. This was a ship from Houston to the Philadelphia area carrying molten sulfur. I believe the melting point of sulfur is about 240°F. It's much better to carry it molten. Molten sulfur above the melting point generates sulfur gases, some of which are autocatalytic to ignition. The ships have a ventilation system to take care of normal ventilation. The Sulfur Queen was off Florida in a very bad storm, and the last signal was that they were in a very violent storm. A couple of lifeguard rings were found, that's all: thirty-three men lost. My analysis showed that the compartments had a 20 foot head of molten sulfur, and that, with a density of 1.8 or whatever it is, is quite a hydrostatic pressure; about eighteen pounds. At 18 psi the

sulfur would absorb the generated gas. Then, if you had a violent storm, the rocking of the ship would cause the molten sulfur at the bottom of the bin to come to the top where it would belch, and the ventilation would not be able to handle it. They must have had an explosion in the layer between the molten sulfur and the bottom of the deck and it just blew the ship apart. That's the kind of thing I did for the Coast Guard. Various reports were published, especially the list of 200 chemicals and their hazards (44).

BOHNING: While we're talking about safety, we mentioned this at lunchtime ...

KATZ: Dikes around the tanks. That's the system, again the system. Some Phillips people came to our meeting when I was speaking about what caused the explosion in the Bureau of Mines test. They presented a movie and somebody asked a question; the speaker said that methane is normally a vapor, so if it spills it will vaporize automatically. I said that I hoped that no one would ever say that again after Cleveland, because it doesn't. It was simply that the LNG got into both the front door of the laboratory, and got down into the sewers, and came up in these homes. Within the first two weeks they settled something like eight million claims. It was the largest industrial disaster before Texas City.

BOHNING: I didn't realize that so many people in the laboratory were killed.

KATZ: Ninety, I think, out of the hundred and twenty in the laboratory were killed, including the man who was the head of the design team.

[END OF TAPE, SIDE 8]

I'm told a person was driving around and this LNG was in the atmosphere, where it would ignite and the paint would curl off the car from the heat radiation from the fire up above. Again it was the safety of the system. The particular tank was a watermelon-shaped vessel that the Pittsburgh Des Moines Steel Company [now Pitt-Des Moines Inc.] made. It wasn't a copper sphere. Two of the copper spheres were full and I think survived the whole affair, without rupturing or anything. They were intact at the end. Of course they had a cork insulation, a copper inside jacket, and outer steel shell.

BOHNING: You also did some work on compressed air storage.

KATZ: I should talk about underground storage; natural gas

first.

BOHNING: We haven't really talked about that at all.

KATZ: I told you about the continuing engineer education in the summertime. I started a course in underground storage in 1959, and every two years I gave a two-week course, and, as time went on, we cut it down to a one-week course every year. Dr. Tek was my associate until 1981, at which time we separated ways, and now I give it myself. I gave it last June, and I'm talking about doing it again next year.

I consulted with five companies in the last year. I've been with one since 1949 or 1950. East Ohio and Southern California Gas Company for about fourteen years, and a lot of little companies for short periods.

So I've studied gas storage. By the time I got to 1966 my notes were big enough to be called a book, so I published it myself here, Underground Storage of Fluids, with Keith Coats (45). Keith Coats was one of our students who taught a while with us before he went to the University of Texas. He left that to organize InterComp, a simulation company. Various people have come in to help me, but I essentially give the course myself. It is really a handbook; my present notes are the draft of the new book. I had twenty-one attend this past year; probably half of the people with technical functions in reservoirs and gas storage have been at one of my courses. Of course, a lot of them have gone up into management. One of the fellows who came to the 1959 course became chairman of Peoples Gas in Chicago, Cliff Davis. It's been helpful in relationships with gas companies.

BOHNING: Where did the early works start?

KATZ: In places like Pittsburgh, where they would take out from an old gas field in the winter and let it rest in the summer. Then they put the summer excess in and soon they were putting it in during the summer and taking it out in the winter. But the storage fields I'm talking about are ones with a field designed to fit the reservoir, with new piping systems, extra wells, even new wells, and which go to a higher pressure, even to a discovery pressure.

We've had monographs on the movement of water (46). I was involved with the aquifer storage fields, where a gas bubble is created by pushing the water out. Illinois and Iowa have most of the aquifers. The companies I've worked with have aquifers there; Northern Illinois Gas Company, that I still work with, have the largest aquifers and have got one field with 160 billion cubic feet; 1.9 billion cubic feet of water displaced to create a gas bubble.

It's a tremendous thing; they can put into the Hersher Field, the first one in that area. Thirty miles from the gas, companies in the center of Chicago can turn in more than one billion in an hour's notice, at any time. They can get a billion feet per day in the Detroit system, out of free flow from a field in one of the reefs. Consumers Power has a similar one; Northern Detroit is Consumers Power territory, you know, up to Bay City and beyond. They started with Royal Oak up north. They can get a billion out of one of their reservoirs. On a cold day about two years ago, the companies in Detroit took out 4.8 billion in one day from their twenty fields. They took out 4.8 billion and were shipping gas south, yet none was coming in through their two big pipelines. We have the best storage system in the world here in Michigan. There is a company that only does fee storage; they store for other companies for a fee, and they do it by interchange. If United Gas in Louisiana wants storage service up here, they contract for it, send the gas up the pipeline in the summer when it's not too full, and it is put in the ground for the winter when it is needed. I've been called to several special cases of storage systems. The Leidy field operated by three of the largest companies in Pennsylvania were having losses greater than they thought, and I was brought in to review the situation.

BOHNING: Where in Pennsylvania?

KATZ: In the north, near New York state, it's in a state park on the western edge.

BOHNING: Is compressed air storage a natural outgrowth of that?

KATZ: Yes. Electric power people have a greater peakload problem, both weekly and daily, than the gas company. In a pipeline at night when not quite as much gas is needed, the lines may be pressured up so that in the morning when it is needed, the line may be dropped from eight hundred pounds to six hundred. In other words, you get some storage in the pipeline; line pack, we call it. But the line pack on an electrical system is about one millisecond, not very long. The electrical company carries as much as 25 percent as a spinning reserve. They must have something. If the wheels aren't going when the need arises, it's too late. One method is pumped hydro, that is when you pump water up.

Over at Ludington, the Consumers Power pumped hydro has been going some twenty years. They built an 800-acre artificial lake 275 ft. above Lake Michigan, had to put asphalt-covered sideboards on it, but the bottom was lined with clay. They have six 27 ft. diameter flumes to bring the water up. Large pumps that will pump it up such that they can raise the lake seventy-five feet in

ten hours. Pump it up at night, let it out on demand in the daytime, and generate 1800 megawatts at five minutes' notice. They can turn their turbines on in five minutes, but they take fifteen minutes to a half hour to get them going fully. They're so big that a man could go through the valves and, if he didn't bump his head, he'd come out alright. Fish go through it; twenty percent of them get killed, so that's one objection.

They think that the environmental people are going to prevent them from building any more, so, rather than doing that, they use peakload turbine. A jet fuel turbine driving a generator for the peakload. When a jet fuel turbine is going, two thirds of its power is taken to compress the air it burns. The concept of compressed air storage is to compress the air and pump it into the ground at night, using cheap electricity at fifty cents a thousand BTU. At a daytime peak, when it's worth six dollars a thousand, the compressed air is taken out, some fuel added to it, and is used to run the turbine. The advantage, of course, is that you don't have to buy any more base load or expensive power plant.

There's one in Europe, in Germany, using a salt cavity. When this hit the energy industry, the people of the Department of Energy were not very knowledgeable. They called a meeting to have folks discuss it, and I was asked to come to talk about the environmental issues. Although I've been on the advisory board of the EPA, I didn't know enough about the environmental aspect, but I offered to talk about the design principles. When I came back home from that meeting, I decided to write it up as a book. I tried to get the Electric Power Research Institute to fund a project for writing this but they and the University couldn't agree on terms, so I got ten or twelve companies to subsidize me twelve hundred dollars apiece. For this I would write the book, give them twenty copies, and give a one-week course for two people. And that's what I did, about 1978. It's called Compressed Air Storage for the Electric Power Industry with Ed Lady as co-author. But none of the companies built one in the States.

In September we're going to have a meeting; ten companies will come and look at what we know and how we do it. I'll give a two-hour lecture about the reservoir and how it operates. The Electric Power Research Institute is pushing hard to make sure that people know about it. There's a big communication gap between engineers used to surface construction and operation and those working underground, as you probably know. Of course, underground geology is unpredictable. So the compressed air storage was never taken up. Of course, the minor depression we had in the early eighties didn't help.

I was a chairman of a committee of the Academy of Sciences where we looked at stationery power sources and their effluents. One problem was that electric utility people could not predict what their use would be, ten years hence. To build a plant they have to have a ten-year lease. With compressed air, they could

do it with a three-year lease. Eddie Kahn, the man who was trying to control prices for Jimmy Carter, was our economist on this committee. He said that when you have step function, like the jump in the price of oil, there's no opportunity for extrapolation. They couldn't say within twenty-five or thirty percent what the increase in electrical power usage would be ten years later. They just couldn't predict. In fact, the rate of increase went down. Electric power need today is not anywhere near what they thought it would be twenty years ago, when they had to make decisions about adding new power plants. Of course, there is the extra cost of controlling the effluent; acid rain, you know. We knew all about acid rain in 1975 when I chaired this committee. A man from the Audubon Society was on my committee. It's the sulfur in the air that comes from fuel combustion which, when it interacts over a city with automobile exhausts, really generates the acid rain. It makes SO_2 , which goes to SO_3 . When a cloud of vapor from Ohio with a lot of sulfur gets over Pittsburg and gets a lot of automotive exhaust, with all the hydrocarbons and free radicals running around, why it ends up as sulfate, which then comes down as acid rain.

BOHNING: The last map that I saw of the area that I live in had contour lines of SO_2 concentration. One of the worst areas in the Northeast is exactly in the county where I live.

KATZ: And you don't necessarily generate the sulfur there, it comes cross-country.

BOHNING: No, we're getting it from Ohio, and perhaps Western Pennsylvania. But for some reason, that certain spot in the mountains is the place where it's accumulating and has reached very high levels. A lot of damage to the woodlands is occurring, especially in the higher altitudes.

KATZ: The Black Forest in Germany is dying rapidly, so it is claimed. Somebody was there just a year ago; the trees are dying by the millions. I'm not positive, but they think it's all acid rain.

Well, our environment; for two or three years I was on a National Advisory Board panel on power plant emissions. But these boards are composed of unpaid, capable people who are so busy. We even had our chairman not come to several of our meetings; then the meeting date would be changed at short notice. Finally I decided that I could not function like that, so I got off the board after three years.

BOHNING: I've reached the end of my list of questions. We have been going now for almost five hours; is there anything else? I have a couple of things I wanted you to tie up, but let me ask

you are there any other areas that I haven't touched on?

KATZ: I think not. I have had a lot of little experiences, like this one I just told you about. By and large, I think I've given you a pretty good view of what I've done. I feel fortunate that my health has been maintained. I did have my operation, like the President's a couple of years ahead of him. I also know that one should not hope to carry on in the same fullness of activity at my age. Next year, when I'm eighty, I'm going to try to have only a few technical activities, and look at historical things.

BOHNING: Well, we certainly look forward to seeing what you are going to do when you turn your attention to history, if it's as thorough as your other work has been. Let me close by asking you what changes you've seen in the profession during your career.

KATZ: The change in the areas in which chemical engineers work is that, instead of there being a lot of voids in knowledge, we've gained a lot of information. That has been very helpful but, on the other hand, we've reached a state where the information is so voluminous, and so scattered, and so ill organized, that people don't find it. For example, this man from Brazil, who says he's working in the field of gas hydrates. I asked him if he'd looked at the chapter in the Handbook, but he'd never seen it. It had all that we knew back in 1957, and it's still pretty good even today. I ran across a project from the University of California on condensing butane or pentane on finned tubes; this was sometime in the seventies. I sent them a reprint from 1945 where we had solved a formula for predicting the condensation of organic materials on all finned tubes using the Beatty equation (48). A straight-forward calculation, good within a couple of percent, maybe three, four percent at the most. I said that I didn't understand what they were doing, reinventing the wheel. The word that came back was that they needed the money, and the sponsors were willing to support that kind of research, so they would just do it over. That's the sort of thing that was happening in Berkeley, California, Department of Mechanical Engineering.

I'm a great believer in summaries, compilations, accesses to sources of data; I feel that every professor who does something new and different is obliged to put it in a form where his colleagues in the years ahead can find it in the archives. Now we're getting to the point where we can put it on computer systems. These are the main things. No one works as hard, I must be honest. In my day, if you came at night to the university, maybe a third of the professors would be there until ten o'clock at night. Saturday mornings; until the later years that I was chairman, we functioned Saturday morning, classes and all. I would judge, with few exceptions, that the average person today puts no more than 60% of the effort that we did in 1933. That's the way it is. A few; our present chairman [H. Scott

Fogler] is an eager beaver; you go there on a Saturday and he will be there working. But that's not the lifestyle anymore. One of the things that helps to save time is the computer, and it has been very helpful. Affluence doesn't necessarily bring forth extra productivity, the reverse is more likely.

BOHNING: I'm glad you made that comment, I was going to ask you what would you attribute this decline in productivity effort.

KATZ: Our people are better informed in general knowledge, but the students aren't any better at basic problems, probably not as good as thirty or forty years ago. They just aren't getting better. I just hope they can hold their own, in terms of discipline and knowledge. We have shed many things that we didn't really need, and we've got more to shed, I think. I'm talking about all fields now, not just engineering. Medical people, I've had enough experience with hospitals. The medical people are trying to shed things that don't count. Specialization, over-specialization. I don't think chemical engineers have over-specialized to an extent that I would feel concerned about.

BOHNING: The product that's coming out the universities today; how are they in terms of moving into industry?

KATZ: I'm not on the industry side. The young people that come to us as professors after their doctorate degree are fish out of water. They are going to take a long time to really be useful, but this may be the price you have to pay for having fresh ideas, not following the old tracks. I am not one who believes in that process, the way that some folks do. Would you like me to send you a copy of a talk on the experiences of an industrial-related engineering educational program?

BOHNING: At this point I'm going to close by thanking you for spending so much time with me. I've enjoyed it, I've learned a great deal. I appreciate your sharing all your experiences with me. Thank you very much.

KATZ: You're welcome.

[END OF INTERVIEW]

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