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NEAL R. AMUNDSON

Transcript of an Interview
Conducted by

James J. Bohning

at

University of Houston

on

24 October 1990

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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Neal R. Amundson

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NEAL R. AMUNDSON

1916 Born in St. Paul, Minnesota on 10 January

Education

1937 B.A., chemical engineering, University of Minnesota
1941 M.S., chemical engineering, University of Minnesota
1945 Ph.D., mathematics, University of Minnesota

Professional Experience

Standard Oil Company of New Jersey
1937-1939 Process Engineer

University of Minnesota
1939-1947 T.A., Instructor, Assistant Professor, Department of Mathematics
1947-1951 Associate Professor, Department of Chemical Engineering
1949-1977 Head, Department of Chemical Engineering
1951-1967 Professor, Department of Chemical Engineering
1967-1977 Regents' Professor, Department of Chemical Engineering

University of Houston
1977-1982 Cullen Professor of Chemical Engineering
1982- Cullen Professor of Chemical Engineering and Professor of Mathematics
1987-1989 Vice President

Honors

1954-1955 Fulbright Scholar, Cambridge University, England
1955 Guggenheim Fellow, Cambridge University, England
1960 Industrial and Engineering Chemistry Award, ACS
1961 William H. Walker Award, AIChE
1969 National Academy of Engineering
1970 Vincent Bendix Award, American Society of Engineering Education
1970 Fellow, AIChE
1971 Warren K. Lewis Award, AIChE
1973 Richard H. Wilhelm Award, AIChE
1975 Guggenheim Fellow, NATO Senior Fellow
1985 Sc.D. (Honoris Causa), University of Minnesota

- 1985 Founders Award, AIChE
- 1986 Eng. D. (Honoris Causa), University of Notre Dame
- 1989 Albert Einstein Award, Computing and Modelling Association

ABSTRACT

Neal Amundson begins the interview with a discussion of his family and early years in St. Paul, Minnesota. Amundson graduated from high school at the very depth of the Depression. For the Amundson family, times were very grim, yet Amundson's parents insisted on sending their son to college. Amundson attended the University of Minnesota, where he received his B.A. in chemical engineering in 1937. Immediately after graduation, Amundson accepted a position with Exxon, then Standard Oil Company of New Jersey, as a process control engineer. There he worked on controlling phenol loss in Exxon's process for lubricating oil. After nearly two years with Standard Oil, Amundson returned to the University of Minnesota. While working toward his M.S. in chemical engineering, Amundson served as a teaching assistant in the mathematics department. After receiving his M.S. in 1941, Amundson decided to switch his educational focus and received his Ph.D. in mathematics in 1945. Amundson stayed at the University of Minnesota as an assistant professor of mathematics. In 1947, he transferred to the University's chemical engineering department and became an associate professor. In 1949, Dean Athelstan F. Spilhaus offered Amundson the position of acting chair of the chemical engineering department. That same year, Amundson became a full professor with the University. In 1951, at just age thirty-five, Amundson held the positions of department chair and professor at the University. Amundson's research work focused on heat transfer, chromatography, and adsorption. Although he was chair of chemical engineering, Amundson was first a mathematician. As a result, he structured the chemical engineering department on a more theoretical level, hiring faculty that held mathematical interests and initiating mathematical applications into a practical engineering curriculum. The strength of the faculty that Amundson assembled helped build a solid reputation for the University of Minnesota. By the late 1940s and early 1950s, Amundson introduced computers into his curriculum. In 1977, Amundson left the University of Minnesota and became the Cullen Professor of Chemical Engineering at the University of Houston, a position he holds today. Amundson concludes the interview with a discussion of his consulting work, the success of students, and thoughts on his career decisions.

INTERVIEWER

James J. Bohning is currently Visiting Research Scientist at Lehigh University. He has served as Professor of Chemistry Emeritus at Wilkes University, where he was a faculty member from 1959 to 1990. He served there as chemistry department chair from 1970 to 1986 and environmental science department chair from 1987 to 1990. He was chair of the American Chemical Society's Division of the History of Chemistry in 1986, received the Division's outstanding paper award in 1989, and presented more than twenty-five papers before the Division at national meetings of the Society. He has written for the American Chemical Society News Service, and he has been on the advisory committee of the Society's National Historic Chemical Landmarks committee since its inception in 1992. He developed the oral history program of the Chemical Heritage Foundation beginning in 1985, and was the Foundation's Director of Oral History from 1990 to 1995.

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INTERVIEWEE: Neal R. Amundson
INTERVIEWER: James J. Bohning
LOCATION: University of Houston
DATE: 24 October 1990

BOHNING: I know that you were born on the 10th of January 1916, in St. Paul, Minnesota. Could you tell me something about your parents and your family background?

AMUNDSON: My father was one of six children and he was the first one in his family born in the United States. All the others came from Norway. They lived most of the time in real central Minnesota, which was a large Norwegian community. If you've ever heard of Garrison Keillor, that's the area he came from—Lake Wobegon. And what Keillor says about that area is absolutely accurate. They lived on very poor farms in central Minnesota. The land, which is to the east of the Mississippi, is all sand and that's where they made an effort to farm. My father had one brother and four sisters. The two younger sisters were the only ones that really had any kind of an education at all. I'm not sure what kind of education they had, but they had some education. All of the others probably didn't go through more than the fifth or sixth grade.

My father was a very smart fellow, and as soon as he was able he got out of that area and came to St. Paul. My mother actually came from the Kohlers, and that was a famous old Pennsylvanian Dutch outfit. One side of the family became extremely successful, and the other side of the family didn't do very well at all. That's the side my grandmother came from. One of the Kohlers was the founder of that company that made toilet fixtures of all kinds, and you see that name Kohler. One of the others became governor of Wisconsin. My grandmother actually came from some other part of the family. They left Pennsylvania some time early in the nineteenth century and moved to La Crosse, Wisconsin. From there they went to St. Paul.

None of my mother's brothers or sisters probably got beyond the sixth grade. In those two families I probably had about thirty-five cousins and I'm the only one that ever went to a college or university of that group. Many of the others almost followed the footsteps of their mothers and fathers, without any education at all.

BOHNING: Did you have any brothers or sisters?

AMUNDSON: I don't have any brothers or sisters. I was an only child and so was my wife. If it hadn't been for my father, I probably would have ended up just like the others. But he always said that he never wanted me to have to work as hard as he had to work, so he really insisted

that I go to the university when I was able to. We lived in St. Paul, in what I think would now be called a lower middle-class area. The people were mail carriers and streetcar conductors and hand workers. My father was a hand worker. In all my early youth he was a hand worker. He was a pipe fitter or steam fitter, whatever you want to call him. Then as he got older, he became a foreman and an estimator. He spent probably the last thirty years of his life as a foreman for small construction companies and also as an estimator.

BOHNING: Was your early education all in St. Paul?

AMUNDSON: I went to Hancock School. It was a usual neighborhood school—it probably had altogether three hundred fifty students in it. I went all the way through that, and I started when I was four. Then I went to Wilson Junior High School, and I was there for three years. That was also a neighborhood junior high school. Then for high school I went to St. Paul Central High School, which was a superb high school for the times.

BOHNING: Did you go to St. Paul Central because of where you were living?

AMUNDSON: It was a long walk from our house. When I started I think all of St. Paul had only four high schools, and that would easily have been the closest one. From our house it was probably about two and a half miles, which I used to walk up and back every day.

BOHNING: Before we get into high school, did you have any teachers in your elementary education who were influential?

AMUNDSON: Oh, I can remember the names of almost all of them, as a matter of fact. I think, in retrospect, it was an awfully good grade-school education. I can remember in grades one through six the names of all the teachers except one. In junior high school I remember for arithmetic and algebra I had an exceedingly fine lady.

BOHNING: Do you think that's responsible for your beginning to develop an interest in mathematics?

AMUNDSON: No, I think it was high school. When I went to Central High School, we had extremely good teachers. I remember they were real hard, and they didn't put up with any nonsense at all. We had an awfully good chemistry teacher, and he was the one that certainly sparked the interest of all kinds of people in chemistry. His name was Mr. Bush. Then, in courses like history I had a superb woman, and also for English. They really made the students

work extremely hard. In English at this particular high school, you had to write a theme a week and you had to memorize twenty lines of poetry every week. You handed the theme in on Friday and you also had to reproduce the twenty lines of poetry that you did. So it was really a very fine high school. For years that high school produced exceedingly good students. In that high school class there were six hundred fifty students when we graduated, and I was number six in it. All but one of those ahead of me and probably three behind me eventually got doctor's degrees of some kind, either electrical engineering or chemistry or mathematics or something.

BOHNING: Was that 1933 when you graduated?

AMUNDSON: Yes, it was 1933 when I got out of there.

BOHNING: I wanted to ask you a couple of things yet, before we go on to the university. This was during the Depression.

AMUNDSON: It was the very depth of the Depression.

BOHNING: How did that have an effect on your family?

AMUNDSON: Well, my father didn't have a job. He lost his job. He came home from Chicago in about the middle of 1931 and he didn't work for about three to three and a half years. He didn't work at all. That was when I was a senior in high school. For my first and second years at the university he didn't work at all. I was called a federal student at the university when I was a freshman, and I did janitorial work for which I had to get up at half past five in the morning. I did that from five-thirty until half past eight every morning. We had a hard time in 1934. The family was on relief while I was at the university, and they used to bring boxes of food to us. I used to get jobs during vacations. I used to work for Western Union. I pedaled up hills and carried messages. Once in a while my father would get sort of an odd job and I would help him. But we lived in those on practically nothing. It was grim.

BOHNING: When did your father start saying he wanted you to go to the university?

AMUNDSON: Oh, probably some time in high school, about the end of high school.

BOHNING: The Depression had already started, yet he still wanted you to go.

AMUNDSON: But you see, you could go to the University of Minnesota at the time for the whole year for less than one hundred dollars, plus books. Books at that time were probably another one hundred dollars for the year. So for two hundred dollars, you could spend almost a year at the university. My father had some odd jobs of various kinds where he made a little, and he tried everything because he was a proud fellow. But those were very hard times. Now, people just don't understand how hard those times really were. They were grim.

BOHNING: I can partly identify with that because I was born during the Depression and my father didn't have a job. So I can identify somewhat with that, but certainly not to the extent that you can.

You mentioned Mr. Bush as the person who sparked your interest in chemistry.

AMUNDSON: He lectured like the university professors, and he didn't follow the book that he had. He had a fellow who helped him, and who ran the lab—he was a senior. They really ran a first-class operation. They did the kind of experiments in high-school chemistry that excited you, not the kind of experiments that everyone wants to do now. [laughter] It used to scare me when I think about it, because every student used to generate hydrogen sulfide at his own desk. And they handed out glacial acetic acid and fuming sulfuric acid like it was water. So once in a while when I think about it, it actually scares me a little bit, but nobody ever got hurt.

BOHNING: Now we can't do anything like that in the high school laboratory.

AMUNDSON: That's right.

BOHNING: Had you ever had any earlier thoughts about science or math?

AMUNDSON: Never. No. I would say that the earliest thing I thought I would probably do was occurred when I lived with my grandmother in the years from about 1928 to 1931, when my folks were living in Chicago. I had an uncle, and he was a draftsman for the Great Northern Railroad. He talked about civil engineering all the time. So he had me convinced that what I wanted to do was study civil engineering. Then when I got to high school I roamed with five or six high school students, all of who were interested in chemistry, and I kind of went along with that. All of those fellows ended up with doctor's degrees in chemistry. One was Robert Carlin, who was at Carnegie Tech [Carnegie Technical Schools, now Carnegie Mellon University], and I think he was department chairman there for some time. Robert Carlin. He was one of them in high school. There were Lyle Baer and Orville Beckman, and there were half a dozen or so chaps like that. We all went to the University of Minnesota to study chemistry. When I started

out, I thought I was going to study chemistry when I went to the University of Minnesota. Then, about the beginning of third year, I switched over to chemical engineering.

BOHNING: I'd like to explore why you did that, but first let's talk about your first experiences at the University of Minnesota in chemistry. What kind of courses did you have?

AMUNDSON: Let's see. How can I say this so it doesn't sound too bad. I would say that freshman chemistry at the University of Minnesota at that time was very badly taught. A lot of sophomore chemistry was not very well taught either. That kind of turned me off. That's when I decided that I would switch. I think it was in the fall of 1935 that I switched. But there was an old chap named Sneed, for example, who taught freshman chemistry—M. Cannon Sneed. He offered a substantially poorer course than I had in high school. That was discouraging. Also in the second year, we had to take analytical chemistry, and that was offered very badly by a man named [I. W.] Geiger. Then, when I got to the third year, the chemistry in the third and fourth year was offered superbly. We took organic chemistry from Lee Irvin Smith, and we took physical chemistry from Frank [H.] MacDougall. All of these were offered extremely well. I mean, they were real first-class operations.

BOHNING: Do you remember any of the texts that you used?

AMUNDSON: Smith lectured. The book was the one by [James B.] Conant, but nobody ever looked at the book (1). MacDougall used his own book (2). That was extremely well done. I would say that the freshman and sophomore works at that time were done badly. It was kind of unfortunate. But beginning in the third year, things were fine.

BOHNING: Were you developing your math background at the same time?

AMUNDSON: No. That's something that really happened in a different fashion. The undergraduate mathematics that was offered to engineers at the University of Minnesota at that time was not well done. There was a separate mathematics department within the engineering school, and it was just a teaching department. It was in what was then called the College of Engineering. That department had engineers as instructors, not mathematicians. In the fall of 1933, of the some twenty chaps who were in that department, there was not one single fellow that had a Ph.D. in mathematics. (I'll have something to say about that afterwards.) It was one hundred percent a service enterprise, with all the disadvantages that you and I understand that entails. So I didn't get interested in mathematics as an undergraduate at all. I would say in the third year—when we started to take some chemical engineering—that was offered in a better fashion. But it was spotty. It was very, very spotty in retrospect. Some of it was very good and some of it wasn't so good.

BOHNING: Were there anyone else that switched over to engineering from chemistry with you at that time?

AMUNDSON: Well, let me see. One of things about the University of Minnesota that you have to understand is at that time the department of chemical engineering was in the School of Chemistry. Chemical engineering students took almost exactly the same chemistry that chemists took. The only other thing that undergraduate chemists had to take was qualitative organic. They had to take two courses in analytical chemistry that chemical engineers didn't take. That was at the time when [Izaak] Kolthoff was riding high. Then chemists had electives that chemical engineers didn't have, and practically every chemist took advanced organic for a whole year, also from Smith. I also took that as a chemical engineer, because I liked it. Really, if you looked at the chemistry undergraduate curriculum and the chemical engineering undergraduate curriculum, you could almost say that a chemical engineer at the end of the year also should have gotten a chemistry degree. It was almost that way—not quite, but almost.

BOHNING: What kind of chemical engineering courses did you take?

AMUNDSON: Well, at that time, chemical engineering was substantially different than it is now. There were lots of descriptive things about industrial processes. How things were manufactured—how you make nitrobenzene, how you make aniline, and all that sort of thing. There were courses on equipment descriptions. What does an evaporator look like, or what does a distillation column look like, and the like. There were also labs that were routine analytical labs. There was gas and fuel analysis, where you analyzed a gas for heating value, for hydrogen sulfide, for carbon monoxide. For lubricating oils you determined viscosities. There were all kinds of routine analytical things. There was very little from an analytical mathematical point of view at that time. Very, very little. It was small, and that didn't spark anybody's interest in what you might call the mathematical side of engineering. That didn't develop for me then at all, because I didn't know any better. I was extremely clean, and very naïve in those days. All students were at that time. At that time, students didn't really complain. They didn't know any better. They though this was the way it ought to be. Now those days are over, of course.

What I'm saying about the University of Minnesota, I don't want to sound overly critical, because I don't think it was any different any place else. I think it was the same everywhere. But, usually, undergraduate engineering was taught by engineers who came back to the University for one reason or another. Most of them didn't have doctors' degrees. Most of them were not involved very much in any kind of research even when they were at the university, but a few were. There were a few in almost every department. I think the first doctor's degree in mechanical engineering at the University of Minnesota was given about 1914. In chemical engineering, University of Minnesota had chaps who got doctor's degrees in the 1920s. But it was not what you would call a scholarly endeavor.

BOHNING: Did you have any chance as an undergraduate to do any research or original work?

AMUNDSON: No.

BOHNING: It was strictly classroom work.

AMUNDSON: There wasn't any opportunity for that. You had labs where the experiments weren't precisely designed but they were rare. We had some of those in physical chemistry, under [Robert] Livingston.

BOHNING: I wanted to ask you about physical chemistry. If you had to pick something that you preferred.

AMUNDSON: That's the one I would have chosen, because it was extremely well taught at the University of Minnesota, and the lab was extremely well organized, and you knew what the experiments were all about. It was run by hard-nosed chaps who didn't put up with any nonsense. It was a highly analytical sort of activity, and a very hard run. Every Saturday morning in physical chemistry there was a quiz and every fourth Friday there was also an exam. Those guys were tough. We learned a lot from them.

BOHNING: That's an interesting comment, because today you hear students who complain about the faculty being tough; and yet what you're saying is that because they were tough you learned a lot.

AMUNDSON: I had lots of role models, and at the appropriate time I can give you all their names. I had just superb role models at all levels, starting in high school, all the way through the University, all the way through graduate work, and even after I became a department chairman I had some good role models. I think from that standpoint I was pretty lucky and I learned an awful lot from those people. And really, I learned how to behave from those people, as a matter of fact. I think there's a lot less of that now also, than there was in the old days. Lots of role models.

BOHNING: Do you want to mention whom some of them were at this point?

AMUNDSON: Well, for example, in high school, I had Mary Rosen for two solid years of English, and I had Mary Doyle the third year. These were real tough women, middle-aged—they were old at that time if they were middle-aged. In high school also, I had Molly Hyde, Mary Shoberg, and Mary Hosmer, who were very good mathematics instructors. For high school, they were just superb. They really made you work hard, and they were hard. Everybody was afraid of those women. At the university, all through freshman mathematics I had a fellow named Carl Swanson, who was just a superb teacher who knew how to challenge students. Lee Irvin Smith, for example, the organic teacher, was a good role model for everybody. Frank MacDougall, in his own crusty way, was an excellent role model. He was an old Scot and a crusty old bastard. He was tough.

[END OF TAPE, SIDE 1]

AMUNDSON: Then when I started graduate school, I had Hugh Turrittin. He was a mathematician. And Stefan Warschawski. All of these people had a profound effect on me, because I discovered more and more often that I was trying to act like them. They had extremely high standards. I had lots of chaps like that, and lots of women in high school, and even a woman teacher in junior high school that I had for three years who really knew how to teach. But the prevailing thing with all these people was they all had high standards and were good teachers. That was the prevailing thing that had an effect on me. I had some lousy ones, also. When I was in high school I had a semester that was taught by the football coach. That was an absolute disaster. We had a chap at St. Paul's Central High School who offered the physics who had just been there too long. I know we had a fellow who was in social sciences—I can't remember his name—but he was just awful. He shouldn't have been in the high school at all. But I had so many good ones that those other ones really didn't bother me. I was lucky.

BOHNING: Do you think that they are also responsible for your developing a sense of independent thinking?

AMUNDSON: Oh, yes, indeed. I can remember Molly Hyde, for example, really challenged the students. Even in high school she challenged the students. Mathematics is awfully easy to offer by rote. Getting somebody to think about something like that is a much harder job, and she was able. Mary Shoberg, and Hosmer and all of those people did the same thing. They were very good. When I had Carl Swanson as a freshman, he really forced you to think. He gave excruciatingly hard homework problems. We had to figure things out. We just couldn't fill up a paper with it. So I had a lot of very good people. I had some bad ones, but most were very good. I learned a lot from them.

BOHNING: During your undergraduate work, that was still the Depression.

AMUNDSON: 1933 to 1937.

BOHNING: Did you see any effect of that within the University? Maybe you weren't aware of it as a student. Also, what kind of facilities and equipment were prevailing in the departments?

AMUNDSON: Well, I would say that even the undergraduate students could see that in most of the engineering laboratories, the equipment was antique. I mean, even undergraduates could see that. The stuff that was in the chemical engineering labs, of which there were three that we took, on the whole was not very good. It was really old stuff, most of which not only didn't work, but it was awfully dangerous. We had to take mechanical engineering labs. Some of those old steam engines, and pumps, and things like that, that engineers were forced to use were very, very old antique stuff and the students all knew that. The chemistry labs were largely non-instrumental. You performed most of the experiments in flasks and test tubes and beakers and that sort of thing. So it was not like it is now, where all of these labs are highly instrumented, even at the undergraduate level. At that time there wasn't anything. I don't remember seeing a single pH meter as an undergraduate.

BOHNING: What about the physical chemistry lab?

AMUNDSON: Well, you had to have the Victor Meyer apparatus to determine the molecular weight of the gas. You had that. You had things like viscosimeters and the like. Many of the experiments that you had to perform—determining the solubility of potassium iodate, for example—were really things that required very little in the way of instrumentation. All of the balances were chainomatics. At the time that was the standard thing. Even that, in some cases in analytical chemistry, was kind of a luxury where the very first experiment you had to perform was to calibrate all the weights.

BOHNING: As you were approaching the end of your undergraduate career, had you given much thought as to what you wanted to do?

AMUNDSON: Oh, yes. I wanted to stay in school. That was one of the things I thought of. I had looked into going to MIT [Massachusetts Institute of Technology], I had looked into going to Purdue [University], and I had even looked in to going to Georgia Tech [Georgia Institute for Technology, for some reason or another. Something happened in the spring of that senior year that I suddenly decided I wanted to get a job. I don't know what happened. I can't think of any single incident. One of the things that actually turned me off was that all of these people were very discouraging about how long it was going to take to get a doctor's degree. They all talked about six and seven years. And at age a little over twenty, that seemed a bit much. That was

one thing that shut me off. The other thing was that after living through the Depression, suddenly getting a job for one hundred thirty dollars a month almost seemed like the end of the world. That's what I got when I started to work for Standard Oil [Exxon Corporation] in Baton Rouge. I got one hundred thirty dollars a month.

BOHNING: Did you interview with many companies?

AMUNDSON: I probably interviewed with a half dozen. I interviewed with DuPont [E. I. DuPont de Nemours and Co., Inc.] company, and somebody asked me what I thought about going back to school. In a rash moment I said it would certainly be something I would have to think about, and that killed me with respect to that job. As I said I was very green and not very sophisticated. I'd hardly been out of the city. I can remember interviewing at Allied Chemical (it was National Aniline then). I remember interviewing a sugar refinery or a starch refinery or something like that in Illinois or Iowa. I interviewed with U.S. Steel [Corporation].

BOHNING: Why Exxon?

AMUNDSON: Why did I choose Exxon? I can remember exactly why I chose Exxon. There was a fellow that was in the class of 1936 who had gone there. There was also a fellow who was in the class of 1938 who had gone there for the summer of 1936. I had spoken to them, and they all said when they got finished that's where they were going. I went there largely because I thought at least there's going to be somebody that I know. So that was why I went there.

BOHNING: And that was in Baton Rouge?

AMUNDSON: That was in Baton Rouge. I went there probably in the last week of June of 1937, and I can remember leaving St. Paul with a heavy suit on and coming into Baton Rouge when the temperature was 97 degrees, the humidity was 97 percent, and I almost expired.

BOHNING: What did you do at Exxon?

AMUNDSON: They started out everybody in kind of a three-month educational program. I think there were nine of us in the group. There were two from MIT, two from Purdue, one from Oklahoma, two from Minnesota, and one from Case [Western Reserve University]. For three months they just kind of shuffled us around, learning things. Then after the third month they assigned us. We didn't have a choice of what we wanted to do. We got assigned. I got assigned to the yard office, as it was called. It was an office that was out in the refinery, and we

were called process control engineers. Of that nine, six of us went out to the yard office, which was in the center of that big refinery. We joined four or five people who had been there from one to four or five years. We were each assigned some area of the refinery, where we were supposed to worry about being a process control engineer. It was a very ill-defined job.

You have to remember, this is 1937, really still the depth of the Depression. Within the refinery, there's very little construction of any kind, and everybody is concerned about saving money. So the object of the enterprise I was involved in was to see how you can improve operations in order to save money. It was one where you spent your time looking for steam leaks and phenol losses and wasted water and how could you make the units much more thermally efficient. You ran tests on heat exchangers to see how often they should be cleaned. It was really a job where you had to justify your existence. If you didn't save any money, and you couldn't show it, you weren't doing a very good job. I had three units that I was responsible for. One was an old Kellogg distillation unit of a very old vintage. One was a phenol extraction unit for lubricating oil that was a seven-stage unit, and the other was a phenol extraction unit for lubricating oil that was a column unit. Exxon, in its lubricating oil program, used anhydrous phenol to extract non-paraffinic things from oil. So the hafinate was much higher in paraffins than the extract was. They used anhydrous phenol for that. And what you did is you actually juggled a temperature so you were about ten degrees below miscibility. I followed engineering in those three units. The job I had was to figure out how to stop all the phenol losses because they used to lose eight thousand gallons a week. That was the sort of job it was. And it was impossible to find out where the damn phenol went. Some of it went in the air, obviously, and some went down the sewer, but you could only find about ten percent of it that way, and no one knew where the rest of it went. I had that for almost twenty-one months. At that time, the general supervision of all of us was not very good. There was really a lot of discontent. I decided to go back to the university.

BOHNING: That was in 1939?

AMUNDSON: In September of 1939 I went back to the university. In July or August I had written the University of Minnesota to see whether I could get a teaching assistantship or something. Finally, when I was about to give up, for some reason or other I got an offer to be a teaching assistant in the mathematics department.

BOHNING: Had you applied to the math department?

AMUNDSON: No, I hadn't applied any place. I just wrote and said I'd be interested in coming. All of a sudden I got this letter from the old chap who was department chairman and much to my chagrin, when I showed up there about September 28, I found I was going to teach college algebra and higher algebra. That was a shock to me, because I stuttered then, too. That was really kind of daunting.

BOHNING: I can imagine. So you were to give all the lectures and everything?

AMUNDSON: It was five hours for each of them, and you had to do all your own grading. It was slave labor raised to a very high power. That's the way they operated in that engineering department that I was talking about. That was the mathematics department, as I said. So you did all your own work, and at the same time you were supposed to be pursuing your own graduate program. As a matter of fact, I liked it.

BOHNING: I've read that you actually started taking some math courses at night while you were at Exxon. Is that true?

AMUNDSON: Yes, in the second year that I was there. It would have been the year starting the fall of 1938. There were four other fellows who were in the process-engineering department, and I. We went over to the university and said that we'd like to take something. Well, in the evening at that time at LSU [Louisiana State University], they didn't offer very much, but one of the fellows found out that there was a chap in the mathematics department who would offer something in differential equations. His name was Norman Rutt. He was a very fine chap, a very gentle man, and he offered the five of us a special course where he did it just for the fees. Of course, once again we were still in the Depression and he was happy to get the extra money. I would say that's probably the thing that sparked my interest in mathematics more than anything else. Then I got the offer to go to the mathematics department. That kind of reinforced itself. But Rutt was a fellow who was also kind of a role model. We went through the whole spring of that year. It would have been the spring of 1938.

BOHNING: You went back to Minnesota in 1939, and you got your M.S. in 1941. I'm not clear whether that was in math or chemical engineering.

AMUNDSON: Chemical engineering. When I showed up in the mathematics department as a TA [teaching assistant], there was another fellow who showed up as a young assistant professor at the same time. His name was Hugh Turriffin. He and I developed a relationship, and therefore eventually I decided to work for him for a doctor's degree. I was trying to figure out something for a thesis, and I discovered a little problem on my own. I worked it up and he helped me. I offered that to the chemical engineering department as a thesis for the master's degree. There was a hell of a big discussion because they had never had a non-experimental thesis at any level. So there was a big discussion whether it was really the appropriate thing for somebody to offer an analytic solution of a problem for a master's degree, but finally they gave me the M.S.

BOHNING: Was that responsible for your first paper on matrix methods (3)?

AMUNDSON: That's right. That's the very first one, matrix methods. I'll give you a reprint if you want one.

BOHNING: That would be great, thank you. You just said there was a tremendous debate over whether they would accept it. As I recall from looking at that paper, you actually had to explain basic matrix methods before you ever got to the application.

AMUNDSON: That's right. This was almost unknown at that time.

BOHNING: Engineers didn't have any exposure to matrix methods then?

AMUNDSON: Oh, no. You see, chemical engineering, I would say, until about the latter part of the 1940s, and longer than that in all kinds of universities, was a completely non-mathematical subject. It was much more chemical than it was mathematical. If you went to Purdue or Illinois or Minnesota, any of those schools, engineering was non-analytical. It was not an analytical subject at all. All theses were experimental theses.

When I came back to the university—I was in chemical engineering in 1939, I was in there in 1940, and most of 1941—I began to think over and over again that this was not the kind of scholarly activity that I had thought I was going to get into. I lost my interest in chemical engineering. I wasn't interested in determining heat transfer, mass transfer, and all that kind of stuff. After I got that master's, then I became a doctor's candidate in the fall of 1941 for a degree in mathematics. I was the first one who wanted to use somebody as an advisor who wasn't in the other department. I wanted to use the fellow that had shown up in the fall, Hugh Turrittin. I wanted him to serve as the advisor, and therefore that's what I did. But there was a big discussion about that, because there was a fellow who was in the pure mathematics department whose name was [Dunham] Jackson who had been a long time ago a very distinguished mathematician. He had every single doctoral student of mathematics at the University of Minnesota that had ever been given, and I was the first one to decide not to work for him. That was really a shock. It almost didn't work.

BOHNING: Whom did you have to convince the most?

AMUNDSON: You had to convince somebody over in the graduate school, and you had to convince people in the mathematics department, and even some people within your own

department. The department chairman wasn't sure that he could allow me to work for somebody there. It was a much more authoritarian operation than exists almost any place now. People could shake their head like that, then you were all done, and you didn't have any recourse.

BOHNING: I'm curious about how you managed to devise this problem for your master's thesis on your own. You had said your math background was almost limited until you took these math courses.

AMUNDSON: In the fall of 1939, I was taking some chemical engineering courses, and there's a general problem that every student in chemical engineering looked at. He always worked on distillation. There was a thing called McCabe-Thiele method, and what I discovered was that I thought I could solve that from an analytical point of view. That's the problem I solved. I discovered how to use finite difference mathematics and matrices to solve that problem. I learned most of the mathematics after I came back in the fall of 1939. So I solved a little chemical engineering problem analytically that no one had ever solved before.

[END OF TAPE, SIDE 2]

BOHNING: How did the engineering faculty respond to that when you completed it?

AMUNDSON: I don't remember that they had a real response. On the final committee there was a physical chemist and I think he thought it was kind of interesting, but I think the engineers who were on the committee really didn't understand what I was doing, because chemical engineering was very non-mathematical. It was very primitive from a mathematical point of view. That's one of the things that I changed later. [laughter]

BOHNING: Yes, you did. You started on your Ph.D. in 1941. That would have been just before the war started.

AMUNDSON: Yes, just as the war started. For years I suffered the draft. Then in the fall of 1943 I tried to get into the Navy. Hubert [H.] Humphrey and I sat side-by-side trying to get into the Navy. At the same time, he and I sat side-by-side. He was in that office the same time I was. I don't know what happened to him, but I didn't make it. Then in the winter of 1944 they called me up. They decided they didn't want me because I stuttered. At that time, it didn't hurt my feelings at all. I remember the examining officer said, "Let's suppose you were given a command. Would you be able to answer it?" I said, "I don't know." And he said, "Well, that's good enough for me." So he deferred me. I've never told that to anybody else in my whole life.

I've always stuttered. For a long time, it really didn't cause any difficulties at all, but as I get older, it seems to get worse. That's not the usual situation. Usually people get over it. But through the years, it gets progressively worse. When I was young, my mother sent me to every speech therapist that existed. Nothing seemed to work. When I lecture to students, I don't really have any difficulty at all. But when I have to speak spontaneously, without a piece of chalk in my hand, I have a hard time. It annoys a lot of people and I suspect that had I not stuttered, I probably could have done a lot of other things. Lots of times, as soon as people found that out, that was the end.

BOHNING: That's unfortunate.

AMUNDSON: Well, not necessarily. Here I was senior vice president. In jobs like that, one of things you have to be able to do is talk. If you can't, you're in trouble. But I'll live through it. I've got through it seventy-five years.

BOHNING: Certainly what you've accomplished in chemical engineering has been phenomenal. What was your Ph.D. thesis on?

AMUNDSON: I did the very first thesis at the University of Minnesota on what's called nonlinear parabolic partial differential equations. That was also a problem I discovered myself. If you talk about drying materials, that's a diffusion problem. Diffusion is described by parabolic partial differential equations. One of the fellows who was actually performing experiments in the chemical engineering department was trying to dry gels. You know what happens with a gel when you start to dry it—it shrinks. So I wanted to take into consideration what happened when you took into consideration the shrinkage. That's how the physical problem got started. I got my degree in December of 1945.

BOHNING: Did you publish that anywhere?

AMUNDSON: Just the abstract was published (4). I don't know why we didn't do something else with it, but all I published was the abstract. Once again, at that very time I was awful green and really very uninformed about what I ought to do with that sort of thing. The University of Minnesota wasn't the kind of place at that time where you could get good advice. I imagine had I stayed at Brown [University] or been at Princeton [University] or MIT or someplace like that I probably would have done it much differently.

BOHNING: Well, you were at Brown at the end of that period.

AMUNDSON: I went there in June of 1944, just the last year of the war, because I'd been deferred. All of the service programs at the University of Minnesota that they had starting about 1940, had practically all collapsed by the spring of 1944. There were things like Navy V-12 programs and ASTP programs, meteorology programs. We were all involved heavily in all those activities. It was a very hard working operation, because the Army and the Navy and those people only paid you for standing in front of the class. Some of us taught four four-hour courses. I got into that about 1942. But in the spring of 1944 those things were disappearing very fast. For example, all of the Navy people were told that the program was over now and they would have to go someplace else, and they'd ship them all out. So I wrote to Brown early in the spring of 1944 and I went there in June. That was a real eye opener. That's where I learned practically all the mathematics, or I started to learn all the mathematics that I would have to know in the future. For me, that was an extremely good thing. At Brown, they had a program for applied mechanics, which had started sometime in the early 1940s, probably 1941 or 1942.

There was a fellow there whose name was [Roland G. D.] Richardson, who was dean of the graduate school and had been, for years, executive secretary of the American Mathematical Society. As a matter of fact, the American Mathematical Society is still run from Providence. He was a very influential fellow and he persuaded the federal government to really support that operation, in that they could help with the war effort. That is, they would work on problems that anybody in the war effort could bring in to them. Because he'd been executive secretary, he knew all the refugee mathematicians who were coming from Europe. He got his hands on them and they were all there. They would come through there, stay for six months or a year, and go someplace else. So it was an opportunity really to learn what mathematics was all about. For me, that was an extremely good thing. I stayed there from June of 1944 until October of 1945. I was there fifteen or sixteen months. During the academic year I also did a small amount of teaching there. Then at the end of that, when the fall quarter started in the fall of 1945, I went back and they made me an assistant professor.

BOHNING: What was the date of your Ph.D.?

AMUNDSON: December, 1945, after I got back. That's when the degree was granted. It was all finished, really, by the time I went to Brown but then I got involved in things when I was there so I actually didn't get the degree until December 1945.

BOHNING: When you went back, was it in chemical engineering?

AMUNDSON: No, I went back into mathematics, where I thought I was going to be assistant professor. I had my whole life all figured out, that I was going to become a mathematician. Then, in the fall of 1945, all the GIs started to come back. They came back to Minnesota by the

thousands. I was in the mathematics department and we taught three five-hour courses, no TAs or anything. That was the load. There was an old fellow who had been head of the chemical engineering department for a long time named Charles A. Mann. Beginning, I would say, in the spring of 1946, I'd see him at the faculty club. He had all those students over there and it was hard to hire people, and he kept saying to me, "Neal, why don't you come over and teach in chemical engineering?" I said, "OK, Doc, that's fine," and that was the end of it. He kept on saying that all the time. So finally, in the early summer of 1947, I said to him, "Doc, if you want me to come over, make an offer and speak to the dean about it." And I let it go at that. Well, in July or so, they offered me a job for, I think, forty-three hundred dollars a year. I think I went over there for forty-three hundred dollars and I was making three thousand dollars. So old dean Lend asked me, and I said, "I'll have to think about it." I spoke to all my friends, and my wife. Everybody said, "Go ahead and go. Go ahead and go." But I had this idea I was going to be a mathematician.

I finally decided I would go, and of course it's the smartest thing I ever did. Eventually. I went over there in the fall of 1947, and then I discovered that I'd really fallen into a rat's nest. I tried to move, and I'd written to some schools saying I had a doctor's degree in mathematics, and I was prepared to go into a chemical engineering department. Because of the very nature of the business nobody would pay any attention to me at all. What's a mathematician going to do in a chemical engineering department? As a matter of fact, at Berkeley [University of California, Berkeley] I got back this very short letter from either [Joel] Hildebrand or [Wendell] Latimer (I can't remember who it was), that said, "When we need somebody with your qualifications we will write to you." [laughter] Don't call us, we'll call you. But then I decided to stay.

BOHNING: Would you describe the situation that you found yourself in?

AMUNDSON: It was a department that had a fellow who had been there for some time who was trying to administer research, and he kind of ran it. He was a fellow I wasn't very fond of. Then they'd hired another fellow and paid him a high salary, and shortly after he was there everybody saw it was a grave mistake. Then they'd had an old fellow named [Arthur E.] Stoppel in it who had been an associate professor forever. He was a little bit sensitive about the fact that I was an associate professor now and I was thirty-three and he was sixty and he was still an associate professor. I would say that in the fall of 1947 when I went in there, there were probably one hundred fifty graduate students. There were really only two people in the whole department who really should handle any doctoral students. It was really the same thing that was happening all over the U.S. All these students were showing up and they didn't have faculty to deal with them. I would say the situation was just one that was very uncomfortable, that's all.

Once again, I was still kind of green and naive and didn't know how to handle it very well, so I decided to make an effort to leave. In July of 1949, Charles A. Mann committed suicide. There was a lot of pulling and hauling there about who was going to be chairman or

acting chairman and so on. The fellow who was the dean at that time said that I should be acting chairman because this other fellow who thought he was going to be chairman was so controversial within the department that the rest of the people wouldn't have him. So he made me acting chairman, with the disclaimer that I should find somebody to be the full-time chairman. So from June of 1949 until August of 1951, I spent most of my time looking for someone to succeed me. I worked very hard at it because I knew that wasn't the job for me. That wasn't anything that ever came to my mind, as a matter of fact, and that's honest. A lot of people would say that's bullshit, but that's not true. That was honest. We offered jobs to people but we weren't able to find anybody who would satisfy everybody. Mostly, we couldn't find anybody to satisfy the dean.

BOHNING: That was [Athelstan F.] Spilhaus?

AMUNDSON: That was Spilhaus. And Spilhaus offered the job to a physical chemist who was at MIT who did all the stuff at Oak Ridge [National Laboratory] and designed the diffusion batteries there. Then they offered the job to Joe Franklin, whom you perhaps knew. He accepted, and then he kept putting off the time when he was going to come. He put it off from month to month, and finally when it had gone on from about the fall of 1950 until summer of 1951, we got together with Spilhaus and Spilhaus said, "Well, I think I'll call him up and tell him if he's not going to show up by September 15 we'll have to withdraw the offer." So he did that and Joe Franklin said, "I'm not coming." Then we didn't know what to do. That's when I said to them, finally, "Well, there are only two people in the department who can be chairman, the other fellow or me. And if you choose him, I'm leaving." So he said, "OK, you're it."

So in the fall of 1949 I became head of the department. They're not chairmen there, they're heads. That was a little shocking to me. But the only reason I said that is that I absolutely would not have worked for that fellow. That was honest. That wasn't dishonest. I really didn't think that he would accept the other option. I was really chagrined when he said, "OK, you're department head."

BOHNING: You were thirty-three?

AMUNDSON: I was thirty-three.

BOHNING: So were you the youngest person in the department at that time?

AMUNDSON: Probably.

BOHNING: How did the rest of the department react to that? Did you have their support during the search process?

AMUNDSON: Oh, I think they were all happy about it. One fellow wasn't very enthusiastic about it, but it was a relatively small operation, too. It probably only had about seven or eight people. It was way understaffed. I can almost name all of the people. I'd say we were probably eight people at the outset. I owe most of what I have to Spilhaus. Spilhaus was a very controversial fellow on our campus. I don't know if you know the stories about him.

BOHNING: No. I noted somewhere that the word controversy was associated with his name, but I don't know why.

AMUNDSON: One is that on the campus, he took very strong positions on a lot of things that a lot of people didn't like. For example, he fought tooth and nail against the College of Education. He fought the College of Education all the time. He fought with the Arts College because he had a mathematics department by that time that was getting better and the other one was getting worse. So they were now afraid of us. He was a raving alcoholic. He'd get roaring drunk. I mean, he got sloppy drunk often. We had parties with the alumni, and at those parties he would get sloppy drunk and say a lot of things in front of the alumni that were just awful. He would get so drunk that one of the department heads would have to take him home. He would have to leave his car there and we used to choose up sides as to whose turn was it to take Athel home. All the way home he would tell you what a no-good, lousy department chairman you were. All the way. So the next day when you saw him, he'd forgotten all of it and he was back to normal. The thing that really killed him was the fact that he had a bad automobile accident when he was drunk. That was all over the front page of all the newspapers.

Then he wanted very badly to be president of the university. That's a long story, and we won't go into it. He didn't get it and he essentially gave up then. But he made the College of Engineering into something it wasn't. He made the mathematics department into a very, very good mathematics department that got so strong, that as I said, the other people were afraid of them. But he changed mechanical engineering, he changed chemical engineering, he changed a lot of departments and really made the engineering school respectable. And he gave me a chance and I loved him for it. But he finally had to leave because he antagonized so many people. He was a very sweet, nice man, but just a roaring alcoholic. Then he got in trouble with his wife and that was all over the newspaper. He was a fellow who had the morals of an alley cat, between you and me.

BOHNING: Then 1949 was a turning point.

AMUNDSON: That's right. That's when I became the head and also full professor.

BOHNING: But I noticed somewhere that you never use the word “full” professor. Am I correct, that it was professor, associate professor, and assistant professor, but you didn’t use the word “full.”

AMUNDSON: Oh, yes.

BOHNING: Was the new building built or under construction at the time?

AMUNDSON: We got one million dollars from the legislature in the legislative year 1947 and 1948. This other chap who was in the department, of whom I spoke, had charge of the design of the building, and he worked with the architects. I also helped him after I got over there. That building was completed in August of 1950. We moved into it in August of 1950.

BOHNING: Were you sharing with chemistry up to that time?

AMUNDSON: Yes, that’s right. We had quarters in the basement of the chemistry building. That’s all we had. That was one of the reasons that we really had a hard time finding anybody who was interested in being head. Also, a lot of them didn’t like the idea that the building was being planned and there wasn’t anybody who was going to be there. So that was one of the difficulties. But the other thing is that there were a number of things at that time about the University of Minnesota that weren’t very attractive. I think that made it hard. There were some people who wanted the job very badly, but unfortunately the dean didn’t want them at all.

[END OF TAPE, SIDE 3]

AMUNDSON: The whole business, then, really started in 1951. That’s when I became head and that’s when we started to change the department. Although, in the beginning I made a lot of mistakes.

BOHNING: I wanted to work through two things. One is some of the work you were doing, and the other is the development of the department. I don’t know if it’s better to separate them or develop them together.

AMUNDSON: No, I think we can separate them. Let me speak about the work I was doing. I will also give you a copy of the Founder's lecture that I gave in Washington the week before last. When I was at Brown, one of the things that I got exposed to, in addition to a lot of other things, was a field at that time that was called nonlinear mechanics. In 1947 a book came out called *Introduction to Nonlinear Mechanics* by a fellow named Nicholas Minorsky (5). He had been at the model basin at ONR [Office of Naval Research] for a long time, and he wrote this book called *Introduction to Nonlinear Mechanics*. That was one thing I did at Brown. The other thing was that I learned a lot about partial differential equations, which is the way I spent the rest of my life.

So in the early 1950s, I started to look around. I was now in a chemical engineering department, I had to do research, and I wasn't a mathematician anymore. So I had to figure out something to do. What I discovered was that no one had ever used these schemes of nonlinear mechanics on chemical systems. They'd use them on mechanical systems and they'd use them on electrical systems, where they studied multiple steady states, oscillatory solutions and instabilities, resonance, bifurcations, limit cycles, and a lot of things for mechanic and electrical systems. But no one had ever made the application to systems where chemical reactions were coupled with some other physical phenomena, either heat transfer, diffusion, fluid mechanics, or anything else.

In 1952 and 1953, I worked out applications to mostly what we call "stirred pots" in chemical engineering. It's a continuous chemical reactor that looks like a batch reactor, except you have a continuous feed in and a continuous output out of it. That's the scheme, which is used for most polymerization reactors. They're called continuous stirred tank reactors, sometimes operated singly, often consecutively. So I took the very simplest one, with a very simple reaction—A going to B, exothermic—and sure enough, you could show that there could be three steady states, one of which would always be unstable. You could juggle the parameters of the system. It actually would oscillate. That's how that whole thing got started. Our first paper in that was in 1955 (6). Oleg Bilous and I wrote the first. Then, at the same time, Spilhaus got an analog computer for the University. This was called a Reeves Automatic Electronic Computer. It was called REAC.

BOHNING: Was this after you came back from Cambridge [University England]?

AMUNDSON: After I came back from Brown. I haven't gone to Cambridge yet. That first paper came out in 1955, although we submitted it before I left, before I went to Cambridge.

BOHNING: You have this earlier series on "Mathematics of Adsorption in Beds (7)."

AMUNDSON: Yes, these papers are all things I did on heat transfer. These were all what we call operations in fixed beds. These were all analytical things that I did along with some

students. These were all problems that nobody had ever set up the models before and generated solutions to them. Those really got started in 1949 and they ran through 1954, when I left. A paper showed up in *Chemical Reviews* (8) by a fellow who was at Northwestern [University] for years, Irving [M.] Klotz. The second paper I ever wrote (7) and all the papers where it says “adsorption” all followed from that paper I read by Klotz. All this work that you see in my bibliography on chromatography and adsorption all followed when I saw that article by Klotz. I saw him the year before last, when I was up there, and I promised him I would actually send him all that material, which I have never done, because he was really responsible for that. I think he worked on gas masks for ONR or for the Army during the war.

He wrote this article on adsorption on carbon. I could see there was a whole mathematical thing there, and I milked it dry. All the work on chromatography theory and adsorption that you see here, all followed from that one idea. The one idea that started all the stuff on reactor stability, multiplicity, limit cycles, that all started from that thing on nonlinear mechanics. All the stuff on chromatography and heat transfer and mass transfer and fixed beds, that all derived from that one article of Irving Klotz. All the work on distillation, which you see following that, all followed from the first thing I did after the master’s thesis. So those were the three germs that spread. Most of the work that I’ve done in the last fifteen years really started back about 1974, when I thought everybody should be working on the energy problem. So I started to work about how carbon burns and gasification of coal. All that kind of work really followed from about 1973, when I thought that a lot of us ought to be interested in the energy problem. That, I’d say, was the most unsatisfactory part of it. The other three areas were very satisfactory, but the whole business of char gasification and char burning and fluidization and so on, I’ve never been happy with that.

BOHNING: Back when you were starting these series, very early on, how readily were you able to attract graduate students?

AMUNDSON: Easily. You see, the University of Minnesota was the first chemical engineering department to exploit what you could do with mathematics. Every time somebody in Illinois or Wisconsin or Texas had a student who looked like he had mathematical inclinations, they sent him to us. So for years, starting from 1955 or 1956 through the late 1960s, I probably had four or five Ph.D. students a year. I think at one time I had thirteen doctoral students working for me, all on this analytical work. So the University of Minnesota really changed the whole mathematical outlook of chemical engineering.

It all started in Minneapolis. It’s not something I did all by myself, because in 1955 I hired Rutherford Aris, and in 1956 we hired [L. Edward] Scriven. In that same year we hired John Dahler. We hired a whole faculty whose total interest was mathematical developments in various areas. For a long time, I hired no experimentalists at all. They were all analytical theoretical types. We didn’t hire any experimentalists because we didn’t have any money. At the University of Minnesota in chemical engineering, our budget was exhausted by January of every year. From that point on, it was deficit spending. So those three areas—chromatography,

distillation, and chemical reactor development—all really started from the three germs that I mentioned. One of the things I said in the Founder’s lecture is that DuPont, Exxon, Monsanto [Company], for example, designed all kinds of chemical reactors.

But the University of Minnesota started the generic study of chemical reactors. There’s a fundamental difference in that, because in the generic study of chemical reactors, what you’re trying to find is, what is the structure of the solution space? When are there multiple steady states? When is there parametric sensitivity? When do all these things happen? For a long time DuPont would mostly design chemical reactors by starting out with a small unit and then they would build a bigger unit, and then they would build a bigger unit. They scaled up from one pilot plant to another pilot plant. That’s not what I call design. I don’t call that design.

When I gave the Founder’s lecture, Sheldon Isakoff (from DuPont) came up afterwards, and said, “Well, you know, we designed chemical reactors a long time.” But I said, “Yes, but Sheldon, you didn’t understand what I’m saying. I’m talking about the rational design from fundamental principles. Not scale up.” All that really started at the University of Minnesota.

BOHNING: What was the response of the profession when you first started the mathematical applications?

AMUNDSON: Dreadful. Absolutely dreadful. The stories I could tell you would actually be hair-raising. There was a fellow who worked for Monsanto. He was the only one who was very, very supportive, because he said that he had worked for a long time on reactors involving the hydrolysis of acrylonitrile. He said, “Those reactors are highly exothermic and lots of times they don’t work. I think you’ve discovered why.” I went to the DuPont company around 1957. I went to the Chambers Works, which really was the center of their activity, and gave a seminar. When I started to talk about multiple steady states and unstable states and parametric sensitivity, there were actually snickers. Then when the lecture was over, I was walking out for coffee and some fellow said, “You know, we have a reactor out here and sometimes it makes a product and sometimes it doesn’t.” And I said, “Well, you just discovered your first unstable reactor.” [laughter] One very prominent chemical engineer, after I gave a seminar, got up in front of all of his students and said, “God wouldn’t permit a reactor to have more than one steady state.” Now, the DuPont company, after thirty years, is sponsoring internal symposia on reactor stability and sensitivity. They’re having their own internal symposia on these. Now after thirty years, they’ve discovered it’s really important.

BOHNING: Did you have trouble getting papers published?

AMUNDSON: No. I didn’t have any real trouble, although to begin with we did, because in the early days the journals that were actually done by the American Institute of Chemical Engineers [AIChE] were very conservative. They didn’t want to publish anything at all that

wasn't experimental. They rejected the paper with [William D.] Munro (9). There are three papers that they rejected. Then I sent it off to *I&EC* [*Industrial and Engineering Chemistry*] and they accepted it. About five years after that, the AIChE had a star committee, which looked into publication policies of the AIChE journals. The fellow who was chairman of the committee asked me why we had published that paper in *I&EC* instead of an AIChE journal. Fortunately, I had the rejection letter that I had received and I sent him that. That is when AIChE changed their publication policy and started the journal called the *AIChE Journal*. That's when they started that. But we didn't have much trouble. I think a lot of people didn't understand what was going on.

BOHNING: I have a quote from someone who indicated that, essentially. The response I saw was that you didn't do any experimental work but you expected other people to test your theories and he felt that somebody should test their own theories. That was one comment. But the other was that most people didn't understand what you were doing. But he did admit later on that he appreciated that what you were doing was right. Although I guess at the time, it was not something that many engineers could follow, maybe because of the mathematics.

AMUNDSON: But a lot of it, you see, shows up in *Chemical Engineering Science*. This *Chemical Engineering Science* was a journal that got started just before the *AIChE Journal* got started. They were hungry to have papers. I think therefore they probably published a lot of stuff that they might not have published otherwise. Do you want to talk about the department now?

BOHNING: Yes.

[END OF TAPE, SIDE 4]

BOHNING: You've touched on this a little bit, but let me start by asking this. Within two years you went from a person who had his career planned as a mathematician to chairing a chemical engineering department. What was the vision you had at that point of what you wanted to do with the department?

AMUNDSON: Well, you sound like Scriven a little bit there, because that's what he always says. But you know, I was still kind of a naive fellow and I've always had the idea that I should try to make things better. It didn't make any difference what activity I was in. I guess I'm an improver. One thing that I thought very early was that when I was department chairman I probably wouldn't hire very many conventional chemical engineers for the department, because I had seen at the University of Minnesota, in those early years when I came back, what the general level of research of conventional engineers was. They were doing heat transfer and

falling film absorbers and they were doing a lot of stuff, which I thought to be extremely dull. So I kept on the lookout. I used to see [Joseph O.] Hirschfelder all the time. He often came to Minnesota and gave lectures. I took him aside one time and I said, "Look, if you ever have a student who does the kind of stuff you work on whom you think would work in the chemical engineering department, let me know." I also had good contacts with Richard Wilhelm, who's at Princeton. I didn't have the opportunity to do very much hiring.

Beginning in 1951, I really didn't have much of an opportunity for hiring before I went to Cambridge. I hired one fellow, and that turned out to be a disaster. Later on we had to fire him. Then after I came home from Cambridge, which was the fall of 1955, we started to hire. Then we started to look for lots of people. We had the opportunity for hiring, and from 1955 through about 1958, we hired most of the people who became central to the operation. We hired John Dahler, Arnie Fredrickson, Scriven, and Rutherford Aris. That's four. All those people were successful. That's luck. They all worked out extremely well in the short term. By the early 1960s, the department already had a big reputation. By 1961 or 1962, we were already getting all the best graduate students in the United States from all over. They used to come in droves. In fact, we got so many students that we took in more students than we could afford. Then, we decided it was time to hire some experimentalists. We started to look at people like Lanny Schmidt, Ted Davis, and Bob Carr. [Henry] Tsuchiya came a little bit earlier.

BOHNING: Was he the microbiologist?

AMUNDSON: Yes, he was in microbiology. He started that interest in the department. One of the things I had said is that I didn't think we would hire any conventional chemical engineers. Therefore, when we looked at chemical engineers there was nobody that ever satisfied me. Then we started to look for physical chemists. From 1958 to 1964 I hired four physical chemists in the department—Schmidt, Davis, Carr, and Dahler. Two from [University of] Chicago, one from Harvard [University], and one from [University of] Wisconsin. Then I hired Rutherford Aris. He was the fellow I met while I was in England. I hired him and he came full-time in the fall of 1957 as a mathematician.

Then we decided that we had to get into the bio area and we looked around and we couldn't hire anybody so we hired a microbiologist in the department. He sparked all of the interest in the department in that area. He was a disaster as an instructor, but he did spark all the research for a long time. We also hired Arnie Fredrickson, one of our own students who went to Wisconsin and worked for Bob [Robert Byron] Bird. He came back about 1958 as well. So all those people, with the possible exception of Henry Tsuchiya, worked out extremely well. We worked very hard to keep them.

We also hired a fellow named [William E.] Ranz, who had been at the University of Illinois and then married Roger Adams' daughter. At that time he had to leave because he didn't think Dr. Adams was going to leave. He went to Penn State [Pennsylvania State University]. After he'd been there a year, which now is about 1956, I offered him a job. He

said, "No, I'm going to stay." Seven months later he called me and wanted to know whether the job offer was still open. At that time the structure of the University of Minnesota was such that you had the department head, and you had the dean, and you had the president, and that was the chain of command. You didn't have research committees or anything else. I got on the phone to Athelstan and said, "We've got this guy, and I'd like to hire him." He said, "OK, make him an offer." And I said, "How much can I offer him?" He told me. I called Ranz and said, "This is how much we can offer." And he said, "Well, I really should have more than that. How about eleven thousand?" I said, "OK." So I called up Athel and said, "Ranz wants eleven thousand." He said, "Offer it to him." I called him back and we had a faculty member. And we hired him. Now, he had a remarkable effect on the whole operation because he revised practically all the course offerings in the department all by himself. We gave him free reign to do that. He changed all the undergraduate courses, and he changed three of the key graduate courses. All those chaps really worked out well.

BOHNING: Did your theoretical emphasis permeate into the curriculum at the lower level, too?

AMUNDSON: Probably much more than at any other school. I think that things were offered in a much more vigorous passion. Something else we started is that everything at the undergraduate level in the chemical engineering department at the University of Minnesota is team-taught. Everything. That's something I started. Take undergraduate thermodynamics, for example. You have two lectures a week and two recitation hours. The chap who gives the lectures is in charge of the whole thing. He makes up problems, quizzes, and he tells all the recitation leaders what they should do at every recitation. The recitations are handled by senior staff, not by TAs. Everything is offered that way at the University of Minnesota. So one of the things that you find is that nobody gives a bad lecture in front of his colleagues because the fellows who are handling the recitation sections come to the lectures most of the time. So you never had the difficulty there that they have in all schools, about students complaining about bad teaching. That never happens at Minnesota in chemical engineering.

BOHNING: How did the faculty respond to that?

AMUNDSON: They were all brought up that way. When a young assistant professor showed up, the very first thing he got was a recitation section. Maybe one in thermo and one in something else. That was what he had. After the third year, he suddenly says, "Gee, I'd like to give the lectures." That's the way you did it. When you start to hire full professors in the department, now you're in trouble because that's not the way they were brought up. So they buck against it. But your own people think it's the greatest system in the whole world because now you have four physical chemists and a couple of mathematicians handling recitation sections and they offer it in a much different fashion than the ordinary run of the mill fellows offer it. So I think that probably answers what you thought wasn't offered in a much more

vigorous fashion. I always had recitation sections when I was department chairman. I took recitation sections.

BOHNING: Did that make the younger faculty uncomfortable?

AMUNDSON: No, I don't think so. Not as far as I know. It was a cooperative sort of thing. Nobody tried to upstage the assistant professor. Lots of people don't understand that operation was a class operation. Nobody hung their dirty laundry outside the department. Nobody criticized anybody within the department openly. There were certainly a lot of discussions about what people did, and if one of the full professors thought that an assistant professor wasn't doing something he certainly had a discussion with him, but it was one the side.

BOHNING: To me, it's quite significant to achieve that.

AMUNDSON: You have to look at the chaps. If you look at Rutherford Aris and Scriven and Arnie Fredrickson, they're not your standard chaps. They're all tough, but they're all diplomats and very gentlemanly. Now, if you brought somebody from the outside to give a seminar in front of those people, they'd skin him. If they thought he was trying to pull the wool over them, they would crucify him. [laughter]

BOHNING: It shows something about your ability to pick people.

AMUNDSON: I've had lots of people say, "How did you get all those people?" My standard response always was that I was never afraid to hire people who were brighter than I was. That didn't bother me the least little bit, because if one starts to worry about that it's going to be chaotic. We always hired absolutely the best person. Even some people whom we hired, had they been thrust into some other situation, would have done it differently. Once they got into that kind of an operation, they decided they had better conform to that kind of an operation. There's a fellow named John Dahler, for example, from Wisconsin. Now, John Dahler, had he gone to some other department which wasn't like ours, would have been a complete pain in the butt. He saw that's not the way people in the department behave. They didn't do that. You have to know Rutherford Aris, who is a very gentle, hard-nosed man. A devout Christian gentleman. He's the kind of Christian gentleman who practices what he preaches. It's not just on Sunday. If you look at Scriven, he was a tough fellow who wouldn't think of criticizing any of his colleagues in public. That would just never occur to him to do that. Ted Davis was the same. Arnie Fredrickson was the same kind of gentle fellow. Now it's a much different situation. In the last years, they've hired three or four full professors in the department, so some of this is being chewed away up there. Some of this general attitude is being chewed away.

But the quality of the chaps they've hired is higher than it's ever been before. But that's not the way these young chaps were brought up when they were thirty years old. They showed up when they were forty-five when they had a different agenda. So it's been chewed away a little bit. Eventually it will disappear, but it's been that way now since the early 1960s. So it's been thirty years that it survived and the department just continues to get better and better and better all the time. Everybody on the University of Minnesota campus knows it. It's not one of those things that's any secret. Everybody knows it.

BOHNING: What kind of relationship did you have with the chemistry department? I'm trying to recall when chemical engineering split from chemistry and became a separate department.

AMUNDSON: That's a long story. I would say that when Doc [Charles A.] Mann was department chairman he had kind of a hard time with chemists, because most of the chemists had very low regard for the operation, and justifiably so. Lee Smith and Frank MacDougall and Kolthoff were very strong guys and very distinguished guys. Charlie Mann probably was not distinguished. There probably really wasn't anybody in the department who had the kind of class that were in some of the chemistry divisions, because if you look at the history of the school of chemistry pre-World War II and some time after that, that was an extremely good department. Pre-World War II it was probably in the top four or five or six in the United States. After the war, they fell on hard times. Charlie Mann survived but I think he suffered. Then, I would say beginning in 1949, after he died, I didn't have any difficulty with those people at all. We didn't have any difficulties in 1947 and 1948. Then Spilhaus showed up in the fall of 1948. In January of 1949, he made chemical engineering a department in the school of chemistry. Before that, the chemistry school had five divisions—organic, inorganic, analytical, physical, and chemical engineering. Then he arbitrarily on his own made a department of chemistry and a department of chemical engineering.

The department of chemical engineering now had its own budget. It never had its own budget before. So that gave the chemical engineering department a little more strength. Then this chemistry school, which was headed by the head of the chemistry department, went through several changes. It went through Lloyd Raderson, Dick Arnold, Bryce Crawford, Stuart Fenton, and I'm not sure how many more. We got along fine. I would say there weren't any difficulties. We had some difficulties over some minor things, because engineering for a long time at the University of Minnesota was a five-year undergraduate program. Chemistry also had a five year-program, which they didn't exercise. We had discussions over if chemistry was going to have a four-year program, why couldn't chemical engineering have a four-year program? These are trivial things, which in retrospect are kind of unimportant. They used to use our graduate students in their chemistry labs as TAs. They did that for years. Then, in the early 1960s, Spilhaus was gone and Warren Cheston was now dean. He arbitrarily withdrew us from the school of chemistry and put us in the College of Engineering, without really consulting us. I was angry about it, and I suppose if had I been older and felt my oats I would have fought about it. But I personally think that was a fundamental error. I thought we had a respectable relationship with the chemists. We had two on our faculty who had joint appointments with the

chemistry department. I thought that was a mistake, and as a matter of fact, I still think it was a mistake.

BOHNING: I was going to ask if you had any cooperative research programs.

AMUNDSON: Oh, there are lots of them now. Then, John Dahler worked with two or three people over there and Ted Davis worked with somebody. I would say there were some, but I wouldn't say it was a lot. I think the thing that really changed was the spirit, because when we associated with the chemists I thought that something always came out of that which didn't come out when we were out of that atmosphere.

BOHNING: Much later on there was a change again when material science was integrated.

AMUNDSON: The University of Minnesota had what was called the School of Mines and Metallurgy. Through the years, those operations around the United States have been disappearing one by one until in 1968 or so. I think that there were only fourteen of them left in the U.S. So here was an operation that had a faculty of probably twelve to fifteen in a school. The number of students was falling off, the research programs were zilch, and the dean got approval from the Regents to abolish it. What do you do? You had tenured faculty. So most of the people who were the miners and rock-breakers went into the civil engineering department. That left a group of about five who were really not material scientists yet, but were metallurgists. Spilhaus decided that they'd either go into chemical engineering or they'd go into mechanical engineering. Those were the options. He was persuaded—I don't know by whom—but he was persuaded by somebody that they should go into the chemical engineering department. He finally called me over and said that's what he wanted to do. I said, "That's fine, but if you're going to put them in the chemical engineering department, you have to carve in stone someplace that you won't start the materials science department someplace else." He said, "Okay, that's a deal. You take 'em. I won't. That's so stipulated." At the time, a lot of people thought that was stupid. Even some people in our own department. We even changed the name of the department from the Department of Chemical Engineering to the Department of Chemical Engineering and Materials Sciences. There were jillions of people who thought that was stupid. Now it looks like a stroke of genius, in fact, because that's where the action is in chemical engineering. In chemical engineering, the action is in materials. So now, materials science makes up essentially half the department. Half of those thirty-four are essentially material scientists. It's a very successful operation.

BOHNING: Has much of that had to do with polymers?

AMUNDSON: Oh, yes, a lot of it's got to do with polymers. There must be four polymer people. They've got [Matthew V.] Tirrell, [Christopher W.] Macosko, and Frank Bates, all really good people. I forget who the fourth one is. There's another one in polymers. I hired Macosko. He was one of the last people I hired when I left. So that was very successful and it's really surprising. I think there's only one other school in the United States to have done that, and I think they did that at Syracuse [University]. I think they have Chemical Engineering and Materials Science now. It's a logical development, because most of materials science is chemistry. [laughter]

BOHNING: And polymer people are having more trouble finding a home these days.

AMUNDSON: That's right.

BOHNING: You talked about REAC earlier. When did you first start introducing computers into your work and then also into the curriculum?

AMUNDSON: We had REAC, which we got about 1948, and that stayed active for four to five years and then it became obsolete. Then the University started down the IBM [International Business Machines] route and we had an IBM 602A calculating punch, and then an IBM 605 and then we had an IBM 650 computer. Those were all small, and that took us up to the summer of 1956. I was a consultant for Remington Rand Univac. I discovered that they had an ERA 1103 that they'd be willing to sell, and I convinced the University that University of Minnesota should get this ERA 1103.

[END OF TAPE, SIDE 5]

AMUNDSON: The University paid for this Univac 1103. My students and a fellow in the physics department were the major users at the University. We had that until about 1960 or 1961 when Control Data [Corporation] started. I convinced the University that they should get a CDC 1604 computer, which was model one when Control Data started. We had that 1604 for about four or five or six years. I think we probably got that 1604 about 1960. By about 1964, they were looking for something else. Then I think we got a CDC 6000 or 6400 or something like that. By that time, computers were established so well in the University that there was a University committee doing everything. But I got the ERA 1103 for the University and I also got the CDC 1604 for the University. My students were the major users, along with a couple of fellows from the physics department.

One of the fellows from the physics department was William Lipscomb, who got the Nobel Prize for the stuff he was doing at that time, which he never says was all done at the

University of Minnesota. All that stuff on the boron hydrides that he did was all done in Minneapolis on the old 1103. That's right. All of that work on the boron hydrides was on the 1103 and then the 1604. I remember talking to Crawford one time, and I said, "Bryce, it really irritates me that Willie never says anything, that all that was done at the University of Minnesota." He said, "What do you expect, gratitude?" [laughter] Willie Lipscomb did all of that at Minnesota. He Fourier transformed things to death.

BOHNING: But the mathematicians weren't using it.

AMUNDSON: No, no. Mathematicians didn't use the computer at all. At the University of Minnesota, that's in very recent years that the mathematicians are picking up on it. In fact, all of that stuff at the University of Minnesota happened after I left Minneapolis in 1977. That all happened after that. That's when the governor decided that Minnesota was going to be the center of the supercomputer industry. He gave the university extra funds to form an institute of computer-something-or-other, and it's really something that's in the university and it isn't in the university. It's one of those things that kind of hangs on the edge.

BOHNING: Is there anything else you'd like to add before we get to why you left Minnesota?

AMUNDSON: No, that was a very happy time for me. Spilhaus and I, when he was there, had a very nice arrangement. Once he found out that I wasn't going to make a lot of mistakes, he just let me do anything I wanted to do. The dean of the graduate school thought the same thing. For example, there were always university fellowships of certain kinds that were available. The dean of the graduate school used to have the standard thing that he would say when they were having a disagreement over who ought to get the fellowships. He would say, "Well, why don't we give them to chemical engineering, because we know they won't get wasted." It was one of those situations where we were successful. Everybody knew it. I had really done so much within the university. I was central to so many activities. For example, we had two mathematics departments and I was the one who actually got them combined.

BOHNING: Didn't you once head that?

AMUNDSON: I headed it. What happened was that the president of the university promised Steve Warschawski, who was head of the engineering mathematics department, that he would combine the two under him. Then Warschawski, three days after that, started to really look at what the fine print said. And although the president had said one thing, the fine print didn't say that. So Warschawski had an offer for a long time to go to La Jolla. He accepted it that day, as soon as he saw what the fine print said. So I wrote a long letter to Spilhaus and sent a copy to the University. I deplored it, not only because they lost Warschawski, but also they really

disappointed all mathematicians practically. Even the ones in the other department. Not all of them, but almost all of them. So Spilhaus called me up and said, "Come on over. I'd like to discuss it with you." As I walked in the door, he said, "You are now the head of both of these departments." I was absolutely dumbfounded. I was in a state of shock. I went home and came back the next morning and went over to see Anne Puzak, the dean's secretary. I said, "Look, I have to see Athel because I can't do this." She said, Athel is gone for two months. [laughter] It was in the summertime. "Athel is gone for two months." So I spent from June until Christmas week trying to combine the departments in the engineering school so that everybody knew where the budget was, and I finally got everybody to agree with it. Finally the University sent it the last week of school before the holidays and said, "OK, now they're combined." Then Athel said, "Now you're chairman of the committee to find somebody to replace you." I'd heard that one before, of course. [laughter] I spent a long time and we finally found a fellow internal to the department who would accept. So that's the way that got solved.

I've served on every substantial committee. The fellow who was the academic vice president for a long time was the former head of the electrical engineering department. He and I were friends, so whenever he wanted to have a discussion, I was sort of in his kitchen cabinet. He used to have me come over on Saturdays when he had a hard problem and needed some help. He would have me come over and I would work with him. So I was kind of central to the whole operation for a long time. Then in about 1972 or so, I wanted to resign as chairman. The department wouldn't hear of it, and they convinced me I really ought to stay. By the spring of 1974 I had made up my mind. In these years the university changed a lot. That old structure we had before of head and dean and president was now gone. Now there was a president, there was an academic vice president, several associate vice presidents, a dean, several associate deans, and they all had to make a job. I finally wrote a long letter to the president deploring that structure and said, "When I started out, Athel Spilhaus and the president of the University and I could settle all of our problems over the phone. Now, we have all of these people in the administration building, all generating forms, papers, committees, and here I sit as department head with exactly the same structure in the department I always had at the end of that goddamn funnel. It's all coming to me, and I've had it." So I resigned. I didn't tell anybody I was going to resign, I just resigned.

As I was moving out of the office on July 1 of 1974, exactly twenty-five years after I'd moved into the office, I knew I had made a mistake, because you really can't be so central to an operation and all of a sudden make yourself un-central. You have what I call a replacement problem. For some reason or another, I decided to move my office way up on the third floor, as far from the main office as I could. It was lonesome. I used to hope to get a wrong number, as a matter fact, if just the phone would ring. I handled it very, very badly. Everybody says, "Now you can do whatever you want." That's a siren song, which is not valid. People say, "Now you can go back and do all that research that you wanted to do before that you didn't do, and do all those things that you wanted to do." I said, "But that's not what I want to do."

So I had a hard time and I didn't handle it well. I kind of made a nuisance of myself in the department. I should have gone on sabbatical that first year, in the fall of 1974, but I put it off until the fall of 1975, when I went to Germany and I came here for a while. I came here for

one month in November. The chap who was actually the chairman here was my student. He said, "Look, we'd like to have you come every year for some period." I kind of jokingly said, "It would be easier to come permanently than it would be to come for a few months every year." I really wasn't serious about that. It was just kind of a way of turning him off. Well, two weeks after that, when I was just getting set to go back to Minneapolis because I was only going to stay here for a month, they made me an offer of a regular job with a chair. But I went back to Minneapolis anyway and I came in the fall of 1976 for the fall semester. I was trying to decide whether I should say "yes" for sure or "no" for sure.

After I went back to Minneapolis in December of 1976 I finally decided that I would come. The main reason being that I had observed people in Minneapolis who had been at the university when they retired and hung around the university. They were fifth wheels. They couldn't understand what their function was, the university couldn't understand, and their colleagues couldn't really figure out how to treat them either. So I said I didn't want to put myself in that kind of position. Then I decided to come. When I came here, they didn't buy me by far. A lot of people in Minneapolis think they probably gave me an oil well, or something. But they didn't buy me. They didn't really do anything special for me at all. As a matter of fact, in retrospect I was kind of stupid. I should have bargained with them. But I think that really for me it was a good thing. Not only financially, because they now treat me extremely well, but I think I probably stayed alive longer than I would have had I stayed in Minneapolis. I think had I stayed in Minneapolis, I would have either made a damn nuisance of myself or I would have gone to sleep. I don't think there would have been anything in between. Here, as soon as I showed up, I got a half a dozen graduate students. I got enough money one year, and I had three postdoctoral fellows. So I stayed awake. When I came I was just sixty-one or sixty-two. I'd say that I got eight or nine more good years out of it that I probably wouldn't have gotten in Minneapolis.

Now, next Thursday, a week from tomorrow, my last student will get his degree. He's going to stay as a postdoctoral fellow for the rest of this academic year and I won't take more students. For a long time I never thought this would be home. As a matter of fact, my wife and I used to make jokes about it—that this is where we live, it's not home. Minneapolis is always going to be home. For a long time my wife really was not very fond of it here, although she's not a complainer so she didn't complain about it. One time, about six or seven years ago, I was getting a little bit ticked off here. You know, you go through these things. I was a little bit ticked off, and I went home one night and said, "I'll tell you what we'll do. Let's go back up to Minnesota and we'll build a solar house on the east bank of the St. Croix. We'll be able to spend all summer there, and we'll be able to spend all but the coldest part of the winter when we can go someplace else." She said, "No, I'm not going. It's too cold." Before that, people used to ask me, "What does Shirley think of it here?" I said, "Shirley has her bags packed all the time, and if I went home and said, 'Let's go,' she'd go out the front door with her bags." But then she surprised me and said, "No, I'm not going. It's too cold." [laughter]

But I would say that the fundamental difference between here and there is in what you'd call the subculture. Minneapolis is a very reliable, honest, clean, and reputable operation. The government is honest, and there are no scandals in the police department. For a long time here

in Houston that was a problem. It's the fourth largest city, so it has all the disadvantages of being the fourth largest city. It has lots of low-income people, so there's lots of crime. Honesty with respect to government and things like that are not given a high priority. For a long time here, just the general services that you get that you normally expect in a reputable city, you don't have. The police department, for example, has over a fifteen-minute response time if you call, if they respond at all. There's no such thing as cleaning the streets. There are lots of places in the city where there aren't any sidewalks. After a while a lot of these things start to bother you a little bit. They're not serious, but they're bothersome. That's why I say it's the subculture I'm talking about. Local pride here disappears at the lot lines. You have a big pride in having a big fancy house with maids and a Cadillac in the driveway and so on, but once you go off the lot there's no local pride in anything. What I'm really saying is there aren't a lot of good citizens. Whereas you go to Minneapolis and they do things just because that's the thing to do. Here, people don't understand that concept at all. Here, and it's really kind of an exaggeration to say it, everybody's trying to be a millionaire. And a lot of them are making it. [laughter] Lots of them are making it.

We've decided to stay, and we started to remodel our house. We made that decision this summer whether we should move someplace else or not. I prevailed on my wife that I didn't want to move anymore. I'm tired of moving. So we've started to remodel our house, and I guess we're here for the duration, as they used to say. I don't mind it now, really. It has a lot of good things. If you like opera and ballet and good symphony and lots of drama and so on, it's a very good place to be. Certainly people here are very, very friendly compared to Minneapolis. Here, when you shop, you have to have a conversation with the clerk. In Minneapolis, the Norwegian who was behind the counter had the general attitude—state your business and be off. [laughter]

BOHNING: Is there anything else you'd like to add?

AMUNDSON: One of the things that I kind of said earlier is that you said something that reminded me of Scriven. I've really had the kind of career that young people can only dream about. I say that purely from the idea that I think I've been extremely lucky. Scriven always thinks, "Well, you had the grand plan. Maybe you never said it or verbalized it, but you certainly had the grand plan." My own feeling is that I wanted to change chemical engineering so that it would be different. I didn't stop to think that when I was doing mathematics at an early time when nobody was doing that in chemical engineering, that it was going to have a big effect. That wasn't something that ever entered my mind. Ever. It just turned out that I happened to be, luckily, at the right place at the right time.

BOHNING: There was one other thing I did want to ask you about, and that was consulting work.

AMUNDSON: I consulted for about three years with Univac up in St. Paul (Remington ran Univac) on computations and process control and a lot of things like that. That ended because they changed their whole structure and essentially fired everybody that I consulted with. Then I consulted off and on for Exxon. I consulted for about five years running, and then I got tired of that, because usually consultantships only last for a certain time anyway. Then five years ago I consulted with them again for a couple of years and I was out of that. I consulted for Phillips [Consumer Electronics] before I came here. I was with Phillips for three or four years as a consultant. After I came here, I consulted for Shell [Chemicals Ltd.] from when I came in 1977 until about 1982. That I finally got out of because it was not working for me. It wasn't something very satisfying. I've not consulted with any industrial operation for a long time now. I don't miss it, as a matter of fact.

BOHNING: As you were building the department in Minnesota, how did you feel about faculty being heavily involved in consulting?

AMUNDSON: They didn't do it. Nobody consulted very much at that time. There was very little consulting.

BOHNING: Was that by your own wish?

AMUNDSON: No, it was something that we almost never discussed, but it was not really a problem. It wasn't a problem at all.

BOHNING: We talked briefly at lunch about funding and support. You said that when you started you had few students and little money and you were theoretical because it was the cheapest way to go at that time. How did you change that? How did you work to get that industrial support that wasn't forthcoming?

AMUNDSON: Well, I would say that when we started to get funding, we didn't get anything from industry at all. It all came from the National Science Foundation [NSF] and NDEA [National Defense Education Act]. In the year 1968, one third of our one hundred fifteen graduate students were supported by the federal government—NDEA fellowships and NSF fellowships and that whole gamut of fellowships that we had. Then within the department, we always had about fifteen TAs. We always had a substantial number of TAs who worked over in the chemistry department. Then we used to get more than our share of fellowships from the graduate school from the excess funds that those other fellowships brought in. Those were also distributed as fellowships. Then very early, these young hotshots were able to get money from the National Science Foundation. From departmental funds we supported most of the students for the first year. Then when they became useful we'd switch them off onto other ones.

[END OF TAPE, SIDE 6]

BOHNING: You mentioned that somewhere along the line 3M [Company] started putting money into that much later.

AMUNDSON: Well, that happened after I left. Now, 3M and I always had a problem with each other. I think that's one of the reasons why they never gave us money earlier. 3M treated our department very badly. 3M was the most surprised operation in the world when the first departmental ratings came up and we were so high. They couldn't understand that at all, because they never liked this whole theoretical approach. 3M is an entrepreneurial operation that for a long time did Edisonian research. They just had no understanding at all of what the theoretical aspect of it was. 3M was not very friendly toward us. After I left, 3M suddenly discovered that people like Davis and Scriven and the polymer people were doing things they really should be interested in. So now they have a very friendly relationship and some of that is because I'm gone. As a matter of fact, I think 3M has even given them some funds for an endowed chair within the department.

BOHNING: Your students are responsible for spreading the word, as it were, throughout the engineering profession. How did your early students fare when they went out? Did most of them go into academic positions or did they go into industry?

AMUNDSON: Now, let's see. Let's put it this way. I would say the very earliest ones, say in the early 1950s even up to 1960 or so, most of those people went into industry. Then I would say, starting with the group in the early 1960s, a substantial number went into academic jobs. There is [Dan] Luss, who is here; Roger Schmitz, who is vice president at [University of] Notre Dame; Leon Lapidus was department chairman at Princeton; Andy Acrivos is the Einstein professor at City College of New York. So lots of those chaps went into those kinds of jobs. But on the other hand, Lee Raymond is now president of Exxon. He was my student. Ken [Kenneth J.] Valentas is director of Biological Process Technology Institute. [W. Richard] Schmeal, is a department manager over here at Shell. So I would say that there was a fairly even split, although lots of people think they all went into academic jobs, but that's not true. There's a fairly even split. If you look at the general percentage of chemical engineers who were students of the chemical engineering faculty at the University of Minnesota, it's larger than any other university in the United States right now. Rutherford Aris had thirty Ph.D.'s. Scriven has had thirty-five or forty. The department chairman at MIT now is one of Scriven's students. A fellow who's head of the University of Washington is one of Scriven's students. George Gavalas at Cal Tech [California Institute of Technology], Morton Denn is at Berkeley, and Bob [Robert S.] Schechter is here at UH [University of Houston]. They're just all over the place.

So in general, the University of Minnesota Ph.D.'s—I'm not just talking about the ones I had, but all the others—are really very well represented. And it's still going on. Northwestern has two, Texas A&M [University] has two, The University of Massachusetts has two, Princeton has one, Purdue has three—you just go from department to department and you find there's hardly a department in the United States that really doesn't have at least one on its faculty. I probably have had sixty. I would guess that Aris has probably had forty or forty-five Ph.D.'s. Certainly Scriven is in that general region now. Arnie Fredrickson has probably had twenty-five. You could easily name two hundred without any difficulty, half of whom are probably in academic jobs around the country. So there are a lot of them.

BOHNING: What are the positive and negative changes you've seen in the profession in your career?

AMUNDSON: Let's talk about students first. I would say that there was a time certainly up until maybe eight years ago when chemical engineering got some of the very best students. In the years from 1958 through 1972 or 1973, there were some superb graduate students that came up, not only at Minnesota, but all over. They got very, very good students. Also at that time, I think chemical engineering was held in very high regard by a lot of people. I spent roughly three and a half years from January of 1984 until almost 1988 in Washington, at least once a month and sometimes every week on end. Now the situation has changed. My general view at the moment is that chemical engineering is not held in high regard by anybody anymore. It's really considered to be kind of an old conservative bunch of old-fogey types who don't want to change anything. That's my perception. There will be a lot of people who would argue about that, but that's what I see.

Chemical engineers have become very hidebound, and one of the things I said in 1985 was that if we're not careful, we're going to disappear like metallurgy disappeared. We're going to disappear the same way. There's still going to be somebody doing chemical engineering, but it's not going to be very exciting stuff. Chemical engineers were always very, very happy to prepare students for the general chemical industry. DuPont, Monsanto, Allied [Chemical Company], you name it. And they did a hell of a job at that. If you look at those operations, those huge complex units, we did that. I know that chemists got the idea of what you can do with what, but chemical engineers are the one who put that damn thing together so that the chemical industry is one of the few industries that shows a positive balance of payments. Now that's something we can be very, very proud of. But the general view I have, and the view in the report, is that's going to be short lived. Everybody, the Saudis, the Mexicans, and everybody, is going to be able to make all those fancy products with our expertise. We're going to help them do it, as a matter of fact. And that is going to really raise hell because they don't have to pay anything for the raw materials. A chap here says that Saudi oil costs Saudis about fifteen cents a barrel to get out of the ground.

We have to change. If you look at our report (9), it says that we have to get into high tech areas. We have to get into high performance materials. We have to get into all kinds of

things where the Saudis and the Mexicans and the Taiwanese and the Iraqis and everybody else are not going to be competing with us. Otherwise, we're not going to survive. Now, the difficulty is, you can't convince academic chemical engineering departments that that's what they ought to do. It's not working, see? It's very depressing, as a matter of fact. The way to find out is to write Berkeley and Minnesota and Wisconsin and so on and say, "Do you have some young faculty?" There's nobody who's really working in polymers, no one's working in composites, nobody's working in high performance materials. Now when I say there's nobody, what I mean is there are very, very few of them. And that's the business of the future that we have to get into. I worry about that.

My general view also is that the whole chemical engineering profession is in kind of hard times. The number of students is small. When numbers fall off, quality falls off much more. You don't get the same distribution. You get a much bigger distribution down at the bottom. If you look at what has happened with all of these takeovers, when Chevron [Corporation] takes over Gulf [Oil], a lot of research gets stopped. When the Germans take over a chemical company here, that research all disappears. It's all being done in Germany. My general view is that I think that our profession has to wake up. I don't know what you think about it or not. But I think we have to wake up. I think we're in for some hard times. Now, chemical profits are high. At least, they were high this last year. Most people think that's not going to last. And I've never, in all of my consulting with Exxon, Phillips, and Shell, have been impressed with the general quality of engineering research in those outfits. It's never impressed me at all. I always had the feeling from the standpoint of engineering science that they were many, many years out of date. I've always had that feeling. It's very discouraging. Maybe it's always been that way. I hope I'm wrong, but I don't think so.

BOHNING: Well, I don't want to close on a negative note, but I will say that I really enjoyed this and I want to thank you for taking the time to spend the afternoon with me.

AMUNDSON: Oh, absolutely. That's no problem. In this whole effort I've had a really good time. I don't regret anything that happened at all. As I said, I always thought I was extremely lucky. I've had the kind of career that most people can't even think about. It seems to me that it was all a big accident.

BOHNING: No, I think there are others in print who have said it's more than that.

AMUNDSON: [laughter] Well, now I have a hard time figuring out what I should do. I think about that almost every day. If I could figure out some way how I could spend my time, I'd probably retire at the end of the year. I can't figure out anything that I really want to do. And I know my wife doesn't want me hanging around the house, and I don't want to hang around the house, because I know what happens.

BOHNING: Are you still teaching here?

AMUNDSON: Oh, yes, I teach. I taught this morning from 8:00 until 10:00. I lecture Mondays and Wednesdays from 8:00 to 10:00. I have one doctoral student who will be finishing, but then I serve on lots of thesis committees. I serve on search committees, and I'm on the University Senate, and I get involved in lots of activities here. I'm on the promotion and tenure committee.

BOHNING: It must be quite a contrast being on a promotion and tenure committee, considering what you were able to do back in the early days at Minnesota.

AMUNDSON: It's really one of those things that can only make you unhappy. It's not one of those things that makes you happy because the fundamental decision you have to make every time you consider a promotion and tenure is, if you let this chap go, can you get somebody better? And that ought to be what determines it. But it's not what determines it.

BOHNING: It's the other side of the coin.

AMUNDSON: Yes, that's right. Before I came here in the fall of 1977, promotion and tenure was done by each dean on his own. I convinced the then chancellor that there should be a University promotion and tenure committee and I served on that the first year they had it for the whole University. But, see, this is a very primitive place. It's very, very primitive. It's nice from a certain standpoint because it's not bound by a lot of rules, regulations and traditions of various kinds. So here you can do a lot of things that you couldn't think of doing at the University of Minnesota. I always say that the University of Minnesota has a rule and a regulation and a form and a committee for every possible contingency. And if they don't have one, they're perfectly ready to make another one.

BOHNING: Is that one of the reasons you were attracted here at that time?

AMUNDSON: No, what I thought was the following. What went through my mind in 1974, 1975, 1976 was the energy problem. I thought the northern universities were going to have an extremely severe problem. The legislature in Minnesota, I think, became unfriendly to the University. That's what I thought. I thought that northern universities were going to have a hard time. I also thought that there were two universities in the South that had lots of potential. One was the University of Florida at Gainesville, and the other one was here. I looked at the University of Houston, and I said, "Here's a city that's probably very shortly going to have four

or five or six million people in it, and it ought to have a first class university.” And I thought the same thing about the University of Florida. What I didn’t understand here was that UH is always going to be number three after the other two. That I hadn’t considered and that was a fundamental error in my thinking. Those two places do everything they possibly can to hold us back. They work very hard at it. Texas A&M would give half of its own campus away if it could establish something within the city. Two years ago, A&M made a power play to take us over. It could only happen in Texas. They had some supporters on our Regents. But when the Regents found out that A&M wasn’t going to give us anything out of the permanent University fund, they lost interest. But A&M made a big power play in 1987 and 1988

Now, I think it’s going to be harder for this university to develop, because it really doesn’t have any kind of base of support from the city. And the city is having its own financial problems. So therefore, the idea of our getting something from the city is kind of remote. The Harris county delegation in the legislature is a divided group. If we could actually get the Harris county delegation all to pull in the same direction, we could get a lot more. But they don’t do that.

The other thing that’s happened here, which has been discouraging, is that the whole central administration since I’ve been here has been extremely unstable. Since I’ve been here now, we’ve had four presidents, four academic vice presidents, and three vice presidents for finance. So it’s been a very unstable operation. There’s been a little chicanery also, which has kind upset things. But now we have a lady that showed up on September 1st. She’s a black lady—Marguerite Ross Barnett. She makes sounds as if she’s going to stir things up. We’re hoping and waiting. She’s only been here for six weeks. So we’ll see what happens.

That’s been a lot. I’m a non-stop talker.

BOHNING: I’ve enjoyed it. Thank you again very much for spending the time.

AMUNDSON: I have some things I’ll send you that will tell you a little more.

BOHNING: I’d appreciate that.

[END OF TAPE, SIDE 7]

[END OF INTERVIEW]

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