THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

STUART WINSTON CHURCHILL

Transcript of an Interview
Conducted by

Joseph C. Marchese and Jeffrey L. Sturchio

at the

University of Pennsylvania

on

21 March and 28 March 1985

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
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STUART WINSTON CHURCHILL

1920	Born in Imlay City, Michigan on 13 June

Education

	University of Michigan
1942	B.S.E., chemical engineering
1942	B.S.E., engineering mathematics
1948	M.S.E., chemical engineering
1952	Ph.D., chemical engineering

Professional Experience

1942-1946	Technologist, Shell Oil Company
1946-1947	Technical Supervisor, Frontier Chemical Company
	Department of Chemical and Metallurgical Engineering, University of Michigan
1948-1949	Research Assistant
1949-1950	Research Associate
1950-1952	Instructor
1952-1955	Assistant Professor
1955-1957	Associate Professor
1957-1967	Professor
1961-1967	Chairman
	University of Pennsylvania
1967-1990	Carl V. S. Patterson Professor of Chemical Engineering
1990-	Carl V. S. Patterson Professor Emeritus of Chemical Engineering

Honors

1961	Phi Lambda Upsilon Award for Outstanding Teaching and Leadership, University of Michigan
1961	Citation for Research Contributions, Air Force Aeronautical Systems Division
1964	Professional Progress Award, American Institute of Chemical Engineers
1966	Honorary Fellow, Chemical Institute of Canada
1966	President, American Institute of Chemical Engineers
1969	William H. Walker Award, American Institute of Chemical Engineers
1971	Elected Fellow, American Institute of Chemical Engineers
1974	Elected Member, National Academy of Engineering
1977	S. Reid Warren, Jr., Award for Excellence in Teaching, University of Pennsylvania

1977 Visiting Researcher Award, Japan Society for the Promotion of Science

1978 Warren K. Lewis Award, American Institute of Chemical Engineers

1979 Max Jakob Award in Heat Transfer, American Society of Mechanical Engineers and American Institute of Chemical Engineers

1980 Founders Award, American Institute of Chemical Engineers

1981 Special Honorary Issue, Chemical Engineering Communications

1983 Diamond Jubilee Medallion, American Institute of Chemical Engineers Heat Transfer and Energy Conversion Division

1983 Eminent Chemical Engineer, Diamond Jubilee of the American Institute for Chemical Engineers

1983 Elected Corresponding Member, Verein Deutscher Ingenieure

1987 Featured Engineer, Chemical Engineering Progress

ABSTRACT

Stuart Churchill begins with background information about his family and early education. He then describes his undergraduate years at the University of Michigan, where he was quite active in the mathematics department as well as in chemical engineering. After working in industry for five years, at Shell Oil and Frontier Chemical, he returned to Michigan for graduate school. There, he began both his extensive research on heat transfer, natural convection, and combustion, as well as his career in teaching. After earning his Ph.D. and a position on Michigan's faculty, he began work on several military projects in the nuclear field. In addition, he served on the National Council of and as president of the American Institute of Chemical Engineers. He was also active in industrial consultation. After acquiring increasing administrative responsibilities as chairman of the department, he chose to move to the University of Pennsylvania to return his focus to research and teaching. His students were always a top priority, and throughout the interview he frequently alludes to his close, continuing relationships with them. He also stresses the dramatic impact of increased use of applied mathematics and improved computer technology on chemical engineering. Churchill concludes the interview with a brief discussion of his current work, his family life, and his leisure activities.

INTERVIEWERS

Jeffrey L. Sturchio received an A.B. in history from Princeton University and a Ph.D. in the history and sociology of science from the University of Pennsylvania. He was Associate Director of the Beckman Center for the History of Chemistry from 1984 to 1988, and has held teaching appointments at the New Jersey Institute of Technology, Rutgers University, and the University of Pennsylvania as well as a fellowship at the Smithsonian Institution's National Museum of American History. After a sojourn on the senior staff of the AT&T Archives, Dr. Sturchio joined Merck & Co., Inc. as Corporate Archivist in June 1989.

Joseph C. Marchese received a B.S. in physics from St. John's University, an M.A. in physics from Columbia University, an M.A. in history of science from the University of Notre Dame, and a Ph.D. in history of science from Princeton University. He has taught high school physics, mathematics, and chemistry, and held positions at Visual Education Corporation and Mathematical Policy Research, Inc. Dr. Marchese was a consultant to the Beckman Center for the History of Chemistry in 1984-1985.

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INTERVIEWERS: Joseph C. Marchese and Jeffrey L. Sturchio

LOCATION: University of Pennsylvania

DATE: 21 March 1985

MARCHESE: Let's begin with a few questions about your family background and early education. When and where were you born?

CHURCHILL: I was born in Imlay City, Michigan on June 13, 1920.

MARCHESE: Could you give us the names of your parents and their occupations?

CHURCHILL: My father's name was Howard Heenan Churchill. He worked as a bank clerk and later as an insurance agent and independent businessman. My mother's name was Faye Irma Shurte Churchill. She was a high school teacher of history and English before they were married.

MARCHESE: Do you have any siblings?

CHURCHILL: I have two brothers. The older is Robert Howard Churchill. He was also an engineer but eventually took over my father's business and is now retired. My younger brother, James Paul Churchill, is a federal judge in Michigan.

STURCHIO: Did you say that Robert was older than you?

CHURCHILL: No, they're both younger.

STURCHIO: Is there anything in particular in your early schooling that led to your interest in engineering?

CHURCHILL: I had never thought about the possibility of taking engineering in college until a career guidance counselor looked at a test that I had taken and said that's what I should do. I had really intended to study chemistry or physics or mathematics up to that point. Indeed, I applied at Harvard in chemistry and at Michigan in chemical engineering. I received a scholarship at Michigan and not at Harvard, and that resulted in my choice of a career.

STURCHIO: Who was it that you were hoping to study with at Harvard?

CHURCHILL: I did not have anyone in particular in mind.

STURCHIO: Was that the same for Michigan, or was it just the idea of going to Michigan for chemical engineering?

CHURCHILL: My mother was a graduate of the University of Michigan, and we grew up hearing a great deal about it. I had visited Ann Arbor many times, and I was very predisposed to go there.

There was actually a third choice. One of my high school teachers tried very hard to persuade me to go to Hillsdale, where he was an athletic coach. I actually would have had a better financial arrangement there, but they didn't have engineering.

STURCHIO: What was your high school experience like? Were you slated for a technical goal?

CHURCHILL: I don't think so. I went to a small high school, so there was very little choice in courses. I enjoyed mathematics and sciences, perhaps the most, but I had equal interest in languages, in English and in other subjects. I think the mathematics teaching had some persuasive effect. Looking back, I believe this environment was a very fortunate one for me. I was the best student in my class and thereby gained confidence. I was able to compete on the baseball, basketball and football teams, to play in the band and orchestra, to perform in the theatre, etc., a gamut of activities which would not have been possible in a large high school. I had enough time beyond these activities to read extensively and to work at a bakery during the study hall periods.

STURCHIO: So you had at least some background in math, chemistry and physics when you went to Michigan?

CHURCHILL: Very elementary.

MARCHESE: Did your parents influence the orientation of your career?

CHURCHILL: My father was fundamentally opposed to my studying engineering. He really wanted me to go into business with him.

My other two brothers also started engineering, and these were consecutive bitter defeats in that regard. I should note that my father was supportive once we had made our choice of a field of study. One brother eventually gave up engineering to work with him. My mother was permissive in this sense. She very strongly urged me to go to college, but I don't believe that she had a predisposed idea of what I should take.

STURCHIO: When you graduated from high school it wasn't exactly a buoyant time for business. Did the general economic climate have any impact on your decision?

CHURCHILL: The economic climate made it difficult to go to college. There may have been only two or three out of my high school class who did so, partly because it was an agricultural community and their parents didn't want them to leave the farm. We were just coming out of the Great Depression and my going to college posed a problem for my family as well. Obtaining scholarship help was a big factor. Between my own savings and scholarships, it did not cost my family much for the first two or three years of college, but somehow they would have scrounged so that I could have gone anyway.

STURCHIO: You said that only a few people out of your class went to college. What eventually did most of the people who were your cohorts in high school do? Did they stay in the area?

CHURCHILL: Curiously, I do not know. The reason is that at that exact time World War II broke out and almost all of them went into military service. When I returned to my hometown (since my parents were still there) very few of the young people I had known were still living there. I have lost touch with every single person in my high school class, and I do not know where any of them are today.

MARCHESE: Were there any other teachers or books or clubs that you belonged to that helped to shape your decision to take courses in chemical engineering?

CHURCHILL: I find it hard to remember any because when I went to the University of Michigan to take chemical engineering, I didn't have the vaguest idea what it was about. I'm afraid that I was somewhat undiscriminating in what I read in grade school and high school. I remember starting at the top shelf and reading every book in the high school library, which indicates a voracious desire to learn but not much of a focus. I don't believe my interests in a career were very focused either. I was also interested in athletics and music, and I remember being interested in science fiction. That may have had some impact. I certainly did not study any particular books by or about engineers. I remember going to talk to the one person in my hometown who was an engineer and that he tried to persuade me not to become one because he was out of work. That was rather disheartening.

STURCHIO: That was the time when Du Pont was using its new slogan, "Better things for better living through chemistry." Did that make its way to Michigan? What about a few years earlier when Chicago had a World's Fair?

CHURCHILL: I don't recall hearing of that slogan until later, but I was very impressed with the Chicago World's Fair. We spent a week there and that may have been an influence on my choice of a career. I remember being overwhelmed by the Hall of Science. That formed very deep memories. I can still recall many things from that trip, when I must have been thirteen.

STURCHIO: You applied to Michigan and received a scholarship. I recall reading in Varma's article (1) that you said your experience with mathematics early in your college career was one of the things that shaped your later use of mathematics in engineering. What other influences at Michigan do you recall?

CHURCHILL: I was almost not a chemical engineer at Michigan. I became very close to Clyde Love, my professor in mathematics my freshman year, and I actually switched to mathematics. Love had written two very well-known books on calculus and analytical geometry (2). Michigan had a remarkable applied math department then. As a consequence, he convinced me that I could study both mathematics and chemical engineering, and that if I enrolled as a student in mathematics instead of in chemical engineering, I would have my choice of courses in the university because there were very few students in the applied math department and I could avoid the long lines at the time of registration. I took his advice and did not formally register in chemical engineering until I was a senior. I then confronted the department with the fact that I had taken all of their courses; they were quite grim about this. I remember a confrontation with G. G. Brown who said, "Merely taking some of our courses does not guarantee that you are going to get a degree in chemical engineering." But in the long run they were reasonable.

I played in the University of Michigan Marching Band for four years, and that was a very demanding experience. I probably spent more time doing that than I did in class. We practiced perhaps sixteen hours a week. I remember one of my freshman teachers telling me that I could not go on a trip to Boston with the band because I would miss his test. I said, "But it is an official University of Michigan Band trip." He said, "You'll have to make a choice whether you are going to be an engineer or a musician." I said, "I've already made that choice, but that doesn't mean I can't do other things." He gave me a very difficult time, and in fact, a zero on the test. The first time I ever came to the University of Pennsylvania was in 1939 as a member of the Marching Band when Michigan played Pennsylvania in football. We joked that the band was more heavily recruited and practiced more than the football team. I formed most of my college friendships with bandsmen rather than chemical engineers and joined Acacia Fraternity on their suggestion.

STURCHIO: You were close to Clyde Love as one of your math professors. Were there other professors early in your Michigan career?

CHURCHILL: I remember an English professor that I also had in the first semester. I won't say that I thought about switching to English, but I did very well in his course and subsequently took several of my electives in English literature.

STURCHIO: From what you say, most of the engineering students did not have quite the flexibility in their curriculum as you did.

CHURCHILL: Nominally they did, but perhaps they had a different focus, and therefore no reason to select offbeat courses. They just did what the students do here--picking up courses cafeteria-style. As a consequence of doing both mathematics and chemical engineering, I actually had little flexibility. And there was the band, which meant that I could never take laboratories in the fall. Hence I had a very peculiar schedule as well. I took courses out of order, and for that reason I did not know many of the other students in my own class in chemical engineering very well.

STURCHIO: That was going to be one of my next questions. Do you recall other students who later went on to chemical engineering careers?

CHURCHILL: I have lost touch with almost all of them, again for the same reason--the war. One exception is Worthy Boyd, whom I met again in graduate school and who worked for Exxon. Curiously, I've kept in touch with a few whom I knew in the mathematics program, but very few of the chemical engineering undergraduates. Gordon Van Wylen, later Chairman of Mechanical Engineering and Dean of Engineering at Michigan, and now President of Hope College, where my daughter is a student, and I were nominally classmates for four years, but we don't remember seeing each other then.

MARCHESE: Perhaps you might name a few professors from your chemical engineering courses and say what influence they had upon you. What was your relationship with A. H. White, for example, while you were an undergraduate student?

CHURCHILL: I had A. H. White for a course as a freshman and again as a senior. At the latter time he was then commuting to Washington because of the war. He had a very profound effect upon me as a person and gave me a sense of the history of chemical engineering. He founded chemical engineering at the University of Michigan and was the first professor there. He went to Zürich to study, and came back to advocate that chemical engineering should be a separate curriculum.

STURCHIO: He had been chairman for quite a while.

CHURCHILL: He must have been chairman for many years, but about that time he stepped down and G. G. Brown became chairman.

MARCHESE: White was chairman between 1914 and 1942.

CHURCHILL: It is 1942 I'm speaking of, and that confirms 1942 as the year Brown became chairman.

MARCHESE: White was also a pioneer in formal contract research for industry and government. In 1920, he got the University of Michigan to enter into some sort of an agreement.

CHURCHILL: I think that involved Professor [Walter] Badger and was with the Swenson Evaporator Company. Some say that was the first industrial research contract of any kind at the University of Michigan and possibly in chemical engineering anywhere.

STURCHIO: Did that close connection with industry characterize the way that most Michigan faculty in chemical engineering structured their time, or was White unique?

CHURCHILL: They all had a very close involvement with industry as consultants. Many were doing research, for industry, rather than for the federal government. I didn't know there was any other way to do things. That was the accepted mode.

STURCHIO: Did undergraduate students get involved in research projects?

CHURCHILL: I did. We had the choice of doing so, and during my senior year Bill Collamore and I worked with Don Katz. We did a senior research project that Don later submitted for publication (3). I got to know Don Katz quite well this way. That experience was a persuasive factor in my returning to graduate school.

STURCHIO: Two things about that article intrigued me. One was the obvious relevance that it had to the growing petrochemical industry at the time, since understanding the phase behavior of these different mixtures of acetylene, ethylene, ethane, and the other binary mixtures would help companies to process refinery gases more efficiently. The second thing was that it was based in part on work that was done in the 1890s by a German. It struck me as intriguing that you should pick up something that old to do further work on.

CHURCHILL: Apparently very little had been done in that field in all those years. Katz and [Fred] Kurata had either discovered or rediscovered the phenomena of retrograde condensation. It turned out to have a very important application in the ownership of gas above oil wells. Katz was involved as a consultant in a lawsuit over this in California--whether or not somebody pumping gas was getting someone else's oil. That may have inspired his interest in retrograde condensation--which goes in the opposite direction from what common sense would tell you. Katz thought that perhaps the ethylene-acetylene system might behave the same way and indeed it did. Our paper was published in a second-rate journal because in the limited time we had, we didn't measure compositions, and Don Katz was a little nervous as to whether or not we might have made some systematic error in computing compositions. It turned out that the computed compositions were all right because another man repeated our work as part of his Ph.D. thesis.

MARCHESE: Who was W. G. Collamore?

CHURCHILL: W. G. Collamore was one of the top two or three students in my class in college, and went to work for Exxon upon graduation. I heard that he had a mental breakdown, tried to kill himself, and was in an asylum for some time. I have no idea what has since happened to him. He did not give any indication of such behavior when I knew him.

STURCHIO: J. L. McCurdy is the one who did the work later. Did Katz have a large group of graduate and undergraduate students working on problems like this or was there just one project that you were enlisted to help with?

CHURCHILL: He had a large number of graduate students during that period, but this was 1942, and many of them soon went away for one reason or another. I remember Fred Kurata, Cheddy Sliepcevich, and Harry Drickamer from that time but the latter two did not work for Katz.

STURCHIO: I'd like to come back to that particular piece of research when we start to talk about the work that you did after joining Shell Oil. If we could go back for a minute to A. H. White and the other faculty. You said that you had White as a senior. Was that a research course or a lecture course?

CHURCHILL: It was a lecture course in which he described the processes then being used in the chemical industry. I remember being impressed because he knew the details of all of these off-the-cuff. This was not information you could find in a book anywhere.

STURCHIO: That would be an older style, even then.

CHURCHILL: He had a completely different style from the other faculty members. The undergraduate curriculum was then quite different from the present. It was somewhat mathematical, but only in an algebraic sense. This posed somewhat of a conflict because I was at the same time studying mathematics at a much higher level than they were using in chemical engineering. It may have been fifteen years before I found any use for this advanced mathematics. Chemical engineering work at that time was on a completely different mathematical level, and I was not yet capable of associating the two.

STURCHIO: Did you have courses with Katz, other than the research that you did?

CHURCHILL: I never had a course directly from him. However, he occasionally substituted in a few classes of the courses that I had.

MARCHESE: How about George Brown? Did you have courses with him?

CHURCHILL: Only later as a graduate student.

STURCHIO: Which of the other chemical engineering faculty did you study with as an undergraduate?

CHURCHILL: Offhand I remember [Alan S.] Foust, [John C.] Brier, [Richard E.] Townsend, [Edwin] Baker, [Donald W.] McCready, [J. Louis] York and [J. T.] Banchero.

STURCHIO: It sounds as if you must have taken every course that they offered.

CHURCHILL: Banchero and York were graduate students and instructors (known as "Brown's brats") who ran the laboratories, so I knew them in that category. I probably had some contact with almost every member of the faculty at one point or another. A. H. White nominally taught the freshman course on materials, but many of the metallurgists in the department also participated in the recitations and laboratories. It was then a joint department of chemical and metallurgical engineering. In the freshman and sophomore years, most of the courses were not taught by members of the chemical engineering faculty. We studied things like welding and forging which are long since gone from the curriculum. The first real course in chemical engineering was, however, much the same as it is now. It was a course in stoichiometry taught by McCready which I studied from Hougen and Watson's book (4). Thermodynamics was not then taught in chemical engineering, but I took two courses in physical chemistry from [Lee O.] Case and [R. H.] Gillette which included thermodynamics, and then unit operations. I had Foust one semester and Brier the other. I had a course in inorganic technology from Baker.

STURCHIO: Was the unit operations text by Walker, Lewis, and McAdams being used at that time (5)?

CHURCHILL: No. We used Badger and McCabe because they were Michigan people (6). I was aware of the other book, and I looked things up in it occasionally.

STURCHIO: For the course in inorganic technology, didn't Badger and McCabe do a book on that?

CHURCHILL: It was written by Badger and Baker (7). Curiously, Badger later asked me to revise Badger and McCabe with him, but I declined because I wanted to write my own book.

STURCHIO: What other texts do you recall using at that time?

CHURCHILL: Let's see. I recall some that are still in my library. White used only lecture notes in his course. In physical chemistry, I know we used Getman and Daniels's book (8), and Case's notes on the phase rule.

MARCHESE: What courses in mathematics did you take, and do you remember what texts you may have used?

CHURCHILL: I used Love's books in analytical geometry and calculus. I used Robinson's book on differential equations in chemical engineering (9). I can't remember who taught that course, but it was not Love. I used a book in advanced calculus by Woods (10). In Love's courses in advanced calculus, we depended almost entirely on notes from his lectures.

STURCHIO: Did you come across Sherwood and Reed's book on applied mathematics in chemical engineering (11) while you were an undergraduate?

CHURCHILL: No, that occurred in my first semester as a graduate student.

STURCHIO: Did any of the other chemical engineering students combine as much interest in studying mathematics with chemical engineering as you did?

CHURCHILL: I believe Robert Wallace did. The other students in engineering mathematics were from some other branch of engineering.

STURCHIO: On a spectrum from highly theoretical to strictly practical, where was the focus of the department's work and teaching?

CHURCHILL: I suppose it lay in between. The faculty were all people with practical experience, but we studied the existing theory of all these processes, which was not very advanced at the time.

STURCHIO: Did the course you took in physical chemistry help with the research project you did as a senior? You mentioned that Case used his notes on the phase rule, and you were of course doing work with phase diagrams.

CHURCHILL: Undoubtedly it was connected. That was by all means the most difficult course in chemical engineering, if not in the University of Michigan, at the time. It was universally conceded that no one understood Case completely! As a graduate student I also took courses from him and again found him challenging. He was a recognized genius. He was not being obscure, it was just that he had a little trouble dealing with ordinary humans.

STURCHIO: Some theoretical scientists are like that.

CHURCHILL: I might also say that I took physical chemistry at the same time I took another course because I needed extra courses for the joint degree. That semester I took seven courses. The only way I could do that was to take two courses on top of one another. I just happened to be lucky that they did not then give tests on the same day. I only got caught that way once and managed a makeup. When I showed up for one final exam the instructor asked who I was. But that semester I was cited as the only person in the university to obtain seven A's.

STURCHIO: Your senior year was the year that Pearl Harbor occurred. You've already mentioned a couple of instances where people did leave and things were disrupted. What effect did that have on Michigan and on the chemical engineering department while you were there?

CHURCHILL: No one left that year except some of the graduate students and perhaps Brier. Foust told us we were the worst class he had had in his entire history of teaching, and he attributed this to the fact that everybody was distracted by what was going to happen as a result of Pearl Harbor in December.

[END OF TAPE 1, SIDE 1]

CHURCHILL: The faculty members of the chemical engineering department were very abrasive at that time, and I think that affected everyone. They regularly insulted you in class. The remarks that have recently caused discussion here [referring to Dorfman controversy at Penn] are mild compared to what was said in our classes.

STURCHIO: Was that unique to Michigan at that time or was it something that was part of getting an engineering education?

CHURCHILL: That I do not know. It may have been part of chemical engineering at that time, but I think it had more to do with the personalities of that particular group of faculty. I have no idea if the same thing was true at MIT or elsewhere. I have a suspicion it was.

STURCHIO: When you came back as a graduate student were they still like that, or had they mellowed?

CHURCHILL: The turnover had resulted in some but not complete mellowing. Badger, who had been most famous for his insulting remarks to everyone, was gone, but Foust and others who used that style were back.

STURCHIO: Did they ever turn their ire on you? Are there any incidents you care to relate?

CHURCHILL: Not on me particularly. If they called on you, and you didn't know the answer, you simply had to accept a lot of abuse.

STURCHIO: Was it couched in terms of, "What if you design a plant and you don't know the answer?"

CHURCHILL: Well, they said (in expurgated terms), "You poor fool, what are you doing here?" And, "If you're not going to do this work right, why are you wasting my time?" This was fairly typical of the group of people who were there, Brown among them, but not Katz or A. H. White. When I returned as a graduate student I entered Brown's class about two minutes late the first day because I had been in a line to buy concert tickets. There were one hundred people in the room. He went through a stack of cards and said, "Churchill." I said, "Here." He said, "What's the second law of thermodynamics?" He did not sit down for the entire hour. He and I had a dialogue. When I started to take off my coat, he said, "Don't take off your coat because if you don't answer these questions correctly, you can leave permanently." I just stood and answered his questions on thermodynamics the entire hour. Fortunately, I succeeded in luring him into mathematical aspects where I was more secure than he was. I came out of it okay. He never called on me again the entire year. He did this again the next day with somebody else. You never knew whom it would be. That was his style, and I think many other people in the department had copied it.

STURCHIO: How large was the department in terms of faculty and students?

CHURCHILL: There were roughly 100 students in my undergraduate class. That included metallurgical engineers who were not distinctly identifiable. There were perhaps twenty-five faculty members including those in metallurgy and materials processing.

STURCHIO: Michigan must have been one of the largest departments at the time.

CHURCHILL: I suspect so.

MARCHESE: Did it have a distinguished reputation?

CHURCHILL: They thought they were one of the top two or three schools in the country. As an undergraduate you don't have much perspective on this, but I believe that was a fair rating at that time.

STURCHIO: You mentioned that White was spending some time in Washington doing war-related work. Did the other faculty get involved?

CHURCHILL: Many of them soon left, Foust first. I think he left at the end of the spring semester of that year. Brier, with whom I later did my doctorate, also left that year and soon became commander of the Southwestern Proving Ground. Brown was away off and on during that year. I believe that almost all of the senior faculty were soon gone or partially so. A large core of young people who were graduate students sort of maintained the fort during that period.

STURCHIO: Which of them came back? Presumably a number of them did stay on after the war.

CHURCHILL: I believe they all returned.

MARCHESE: Did the publication of your paper (3) help you in getting your position at Shell?

CHURCHILL: It did not. In fact it may have had a negative influence. When I went to Shell and asked permission, which Katz said I should do, to say at the bottom of the first page, "presently with Shell Oil," they refused unless they reviewed the paper. I wrote to Katz and informed him. He said, "Nonsense. It's none of their business." So, it did not say Shell Oil on it.

STURCHIO: So you were at Shell when that paper came out. Did you start at Shell immediately after graduation?

CHURCHILL: Yes, in May, one step ahead of "the draft".

STURCHIO: Was it difficult to find a position at that point, or were there a lot of openings?

CHURCHILL: Just at that point the job market went from strongly negative to strongly positive. In every one of my interviews I was offered a job on the spot. Almost all the students had a similar experience. The problem was to assess the probability of being deferred. It was very hard to get an honest and reliable answer.

MARCHESE: Was this job at Shell considered to be war related?

CHURCHILL: The position did not absolutely assure me of a deferment, but there was a high probability that it would, and it turned out that way.

STURCHIO: Where else did you interview?

CHURCHILL: I didn't interview any place. All the interviewers came to Michigan. I only had interviews with four or five companies. I remember that I talked to Wyandotte Chemical, Upjohn, and Allied Chemical. Shell was the first and I was immediately attracted to that offer. Katz, with whom I was doing my senior project, thought that would be a very good place to work, although he thought the company was dominated by chemists rather than chemical engineers at the top.

STURCHIO: I assume that it had something to do with the fact that you had been working on subjects related to the sort of thing you would be doing at Shell.

CHURCHILL: Most of the Michigan faculty were working on something related to the petroleum industry, particularly Katz and Brown. So, you almost gained the impression that that's what chemical engineering was about.

When I first went to Shell at their Wood River, Illinois, refinery, I was assigned to the experimental laboratory which sometimes did hurry-up research, even over a weekend. Something strange was happening in the refinery and you must find out why. We either went out and took samples or set up experiments and tried to duplicate the plant behavior. I worked on a wide variety of jobs the first year. Usually these tasks were of a very short duration. Shell was building the second-ever catalytic cracker. It was decided that I and three or four others would operate a pilot plant. For training we were sent down to Houston where they already had such a pilot plant. I worked there for several months, then came back and started up the pilot plant at Wood River, and then worked with the commercial plant which started up almost immediately.

STURCHIO: Was that right after you started?

CHURCHILL: No, a year and a half later.

STURCHIO: 1943?

CHURCHILL: Perhaps March 1944.

STURCHIO: Was there a lot of pressure to get the plant running?

CHURCHILL: There was tremendous pressure because it was to produce aviation gasoline which was needed by the Air Force. That was an exciting time. Sometimes the pressure wore on you. Everything had a great priority and urgency. And you felt patriotic as well. It wasn't just some commercial venture with many alternatives.

MARCHESE: Considering the circumstances and pressure, did your education at Michigan give you the proper training for this sort of endeavor, or did you feel that you were learning on the job?

CHURCHILL: Of course, we were learning on the job. I was as well or better prepared than my compatriots, but even so I usually felt a deficiency of preparation for the specific problems that I faced. That was a persuasive factor in my ultimately deciding to go to graduate school. I didn't know enough about many things and no one else seemed to either, except for a few Ph.D.s who obviously understood a great deal more.

STURCHIO: How many people were in the group that you were working with at Wood River and what kinds of backgrounds did they have? What kind of team did Shell Oil have to do this work on catalytic cracking?

CHURCHILL: There were dozens of young chemical engineers at the same level as myself. One felt in many ways competitive with the others. We realized we were in both training and observation. Shell even gave us written tests periodically. It was quite clear that a rating process was going on but you never quite knew where you stood. From your assignments you obtained some clue as to whether or not you had done the right thing or the wrong thing on the last job.

MARCHESE: Who was supervising your work? You mentioned there were one hundred or so people with you.

CHURCHILL: My supervisor changed frequently. I remember some of them. My first supervisor was John Harkness, who was a Ph.D. chemist from Harvard. Then Larry Lovell, a B.S. chemical engineer from Mississippi. Then I worked for Stan Meisenberg, a Ph.D. chemical engineer from Michigan who was the assistant superintendent in charge of cracking. He succeeded Dale Loeb, perhaps because of a controversy with me. When I was at the Gas Plant I was working for a very capable man, Peter Malson, whom I discovered only had a bachelor of arts. He had never formally studied chemical engineering. I was impressed because of his effective use of self-study. I should also mention George Lorenz, a Ph.D. from Illinois, whom I greatly admired, and again Larry Lovell, who later became director of research at Houston and died prematurely.

This was a very peculiar time. There were almost no chemical engineers at Shell three years before I went there. They had not hired anybody for ten years and suddenly acquired dozens of chemical engineers fresh out of school. Nobody was in front of you. There were only two or three Ph.D.s who were hired in the mid-1930s and who had first been forced to work as technicians or the like.

STURCHIO: Was Shell competitive as far as salaries were concerned? Do you remember how much you were paid at the time?

CHURCHILL: I started working at $155 per month and a number of us were furious to learn that they hired people at $170 six months later and did not give us a raise. Their argument was that wages were frozen because of the war. Wages covered a wide range because of the shortage of people, the draft and federal regulations.

STURCHIO: Did the tests and the competition eventually lead to regular raises? Did technical achievement help you to advance?

CHURCHILL: Oh, yes. I kept getting better jobs and a slightly better salary. Although we were not overwhelmingly paid, that was not a big factor in any of our lives. When I speak of this competitive feeling, it's clear that Shell created it, but we were all good friends outside of work. I drank beer and played golf, etc., with these other young engineers. During the pilot plant days, we worked on shift work. It was an unusual time socially because in the whole area we were the only young men around, and therefore people said angrily, "Why aren't you in the army?" You attracted a great deal of attention if you played golf or did anything recreationally or socially in public. We had gasoline when few others did because we drove back and forth to work at odd hours.

STURCHIO: Who else worked with you on the pilot plant project? There must have been a smaller group who worked closely together.

CHURCHILL: Yes, I know them all to this day. Most of them stayed at Shell until they retired. One became a vice president of Shell. Another was superintendent of a refinery. They all did very well.

STURCHIO: What were their names, just for the record?

CHURCHILL: Ray Schneider, Donald Miller, Walter Kress, Ed Hanudel, Emil Nasser, Hank Hendricks, Al Garner, and Harry Walker are a few.

STURCHIO: You mentioned that while working on that project you felt that you could have known more and nobody else seemed to know what was going on. You also mentioned that the supervisor had a Ph.D. in chemical engineering. At that time Ph.D.s in chemical engineering must have been relatively scarce. Do you have any further comments about that?

CHURCHILL: I was very inspired by these few people who obviously knew so much about things that I hardly knew existed. I didn't feel jealous of them. I just wanted to emulate them.

MARCHESE: In addition to the inspiration that you felt (which may have influenced you to go to Michigan to get a Ph.D. degree), what effect did your job have upon your later vocation as a chemical engineer?

CHURCHILL: I think it had a very profound and lasting effect to this day. I had a wide variety of experiences and a great deal more responsibility than an engineer might have in more normal times. They were forced to give us responsibilities that in normal times would have been given to people with ten or fifteen years' experience. If you're thrown into that kind of fray, you benefit. I think this sense of urgency, and being pushed rapidly into responsible jobs, influences my teaching and my self-confidence to this day. The joke was that every time you did something well at Shell and just got on top of things, they switched you to a completely different area of the refinery or plant, and threw you in over your head again. I don't think that that goes on now, simply because there are more capable people around and less urgencies.

MARCHESE: Did you find the atmosphere at Shell different than that at Michigan?

CHURCHILL: Not really.

STURCHIO: There was quite a bit of that there, also.

CHURCHILL: The atmosphere was a product of the time. I can remember having only been at Shell a year and being in a meeting with two vice presidents and the refinery superintendent. Dale Loeb, who was the assistant superintendent in charge of cat cracking, started to abuse me over some measurements on the cat cracker. I was right, and some of the people recognized it and said, "Do not let him intimidate you. He is no brighter than you are. You're right and he is wrong." That was a lucky event which greatly enhanced my reputation at Shell. I have had this feeling all my life of being lucky under such circumstances.

STURCHIO: How large was the pilot plant, and, eventually, how large was the plant that went on-stream?

CHURCHILL: The pilot plant processed perhaps three barrels of oil per day, and the plant consisted of two units, each for 15,000 barrels per day. That must have been one of the most abrupt scale-ups in history.

STURCHIO: There must have been a few problems connected with that scale-up.

CHURCHILL: There were nightmares, but miraculously the units soon ran at capacity and as needed. They were underinstrumented because the government was trying to minimize the use of valves, meters, etc. Those two cat crackers are still running today.

MARCHESE: Do you know if Shell was working in conjunction with other oil companies? Standard Oil, for example, did a lot of work in catalytic cracking.

CHURCHILL: Oh, yes. That project was joint with a group called the CRR or something, that shared knowledge on catalytic cracking. Shell basically adapted the Exxon process. Exxon had a one-hundred-barrel-per-day semi-works plant and had started one full-scale catalytic cracker before Shell. Although we cooperated, there was also a great incentive to do it our own way and develop patents on improvements.

MARCHESE: Did you interact with people from that company?

CHURCHILL: I did not, although one of my friends was our liaison and went to meetings with Exxon and the others.

STURCHIO: You suggested that Shell was interested in developing some proprietary knowledge. Were there any guidelines as to what would happen after the war, when this kind of cooperation was relaxed?

CHURCHILL: I think so. Shell was always looking at how we could take Exxon's process and improve it so that we would have a better operation than they after the war. We were, however, legally forced to share all that information. There were also construction companies that were intermediaries and an attempt was definitely made not to let them know how we were improving the process so that they would not carry that information to our competitors and build in our improvements in the next plant.

MARCHESE: Did the federal government have a say in the work that Shell did, or more specifically in the catalytic cracking, via funding?

CHURCHILL: As far as I know, there was no funding by the federal government except for the purchase of the aviation gasoline. The federal government acted as a central clearing house for petroleum production, and encouraged everyone to share this knowledge. In so doing, we were protected from anti-trust action. We had other governmental interactions. I worked shortly, perhaps even in my first year, in developing an anti-rust compound which went into turbine oil. This material was said to be a decisive factor in World War II since it allowed our ships to stay at sea indefinitely. If salt water gets in the fuel oil, it eventually corrodes the bearings in the turbines, and the ship has to come in and be repaired. Somebody at Shell discovered a compound which prevented this. So Shell got a contract with the government to make this material. All they had was a laboratory result. Before I arrived, they had started to build a pilot plant. I was told to try to improve the process. I found a different method of separation which increased the productivity about one hundred times. So the pilot plant itself, as redesigned, supplied all the fleets of the free world. Shell never needed to build the plant itself. However, they had great problems with the government because they had contracted to produce so many barrels a day of the additive and it was clear to us that one part in a thousand of the purified material did the same thing. Shell struggled throughout the war trying to get that specification changed to one of performance, but never succeeded. So we put a few drops in the barrel of oil and no one could tell the difference. Shell was not trying to cheat anyone out of money. It was just nonsense to make a thousand parts of something when you only needed one.

STURCHIO: What was the compound?

CHURCHILL: It was a condensation product of a C_{18}-alcohol and maleic anhydride which was hydrophobic on one end and hydrophilic on the other. It made a film around all the water droplets in the oil so they do not contact the surfaces of the bearings.

STURCHIO: Did you have experience at Michigan to prepare you for that sort of work?

CHURCHILL: Not directly. The scope of our education was limited and a good part of it involved reinforcement. We learned by doing. Even the poorest student in the class could design a distillation column which would work. We were thrown into the laboratory with pieces of machinery and told to get some data. Nobody helped, so we became adept at putting things together with baling wire. Although that was very frustrating, it also built a confidence that we could do things. We did not run canned experiments. I think these experiences helped in the long run.

STURCHIO: Did your colleagues from other programs have different experiences?

CHURCHILL: Yes. I don't know specifically how their education differed, but my coworkers had had quite different backgrounds in chemical engineering. They knew more about some things and less about others. Generally, I found their chemistry was better but that I knew more mathematics. Most of them had not had that "in the laboratory, on your own" type of experience. Those were helpful things, but I think most of my associates had an adequate preparation.

MARCHESE: I believe you started as a technical assistant for the petroleum refinery portion of Shell Oil. Did you have any other titles as time went on?

CHURCHILL: During this period, they first called us junior technologists, and then they decided it would sound better to the federal government when they were trying to get deferments to call us technologists. That caused some internal problems because that had previously been a very senior title in Shell. None of us were overly impressed to have this title since we knew it was only for external purposes. Titles were not a big thing at Shell.

MARCHESE: What led you to leave Shell in 1946?

CHURCHILL: I had been thinking for some time that I did not want to spend the rest of my life doing what I had been doing there. I didn't see anybody at Shell with my background doing anything I wanted to do. The ultimate was to spend the rest of one's life in that refinery or another refinery. Shell was a very paternalistic company. If you became the superintendent of the refinery, you lived inside the refinery gates. Every time an alarm went off, you got out of bed and ran out to see what was happening. To me, this involved an over-commitment of one's life. I saw people who had been there for twenty years, who were in the highest positions I might aspire to. They were spending sixteen hours a day, seven days a week either working for Shell or on call. I decided that wasn't for me.

MARCHESE: How did you end up with the Frontier Chemical Company?

CHURCHILL: One of my colleagues at Shell was the liaison for cat cracking with the other oil companies. His name was Curtis Cannon, and in conjunction with two of his college classmates he started a new chemical company. The war had ended and therefore I was free to go. I had been thinking about going back to graduate school, when he called and persuaded me with quite a raise in salary to go to work on this new enterprise. He promised me that after a year they would make me a stockholder in the company and I would become part of management. He described the prospects for the company, and made it sound challenging. I liked him very well and respected him very much. Also, I was primed for such an adventure, so I joined him.

MARCHESE: What did you do at Frontier Chemical?

CHURCHILL: It was in Denver City, Texas, which is at the end of the earth. [laughter] There was not a natural tree to be seen for two hundred miles. The high plains of Texas are almost a desert. We started an electrochemical plant. These people, the friends of my friend, were in the oil-well acidizing business. They put hydrochloric acid down oil wells in order to improve the flow of oil. They bought the acid from Dow, who was their competitor in providing acidizing service. Not surprisingly, they occasionally had trouble obtaining delivery of the acid, so they decided that they should either manufacture the acid themselves or threaten to. They persuaded Cannon to leave Shell to look into this possibility. He concluded it would be economical for them to make their own acid. He then went down to West Texas to start the plant. He had ordered all the equipment for the plant but nothing had been installed when I arrived. The buildings were there, but that's all.

STURCHIO: Had the plant been designed?

CHURCHILL: The plant had been designed and a lot of it was being delivered. I was the only engineer besides Cannon but he also hired an experienced maintenance man from Shell whom I had known slightly. We built and operated that plant with former cowboys. Literally. All the employees had been cotton pickers or cowboys. They had never been near a chemical plant. The cotton farming was in such bad shape that they were looking for any kind of job. This was not textbook engineering. I could not have done that job without having been at Shell. I spent the four years at Shell with other very good engineers. I was continually learning by helping people do things in a company that was very high-tech. Shell was as good as any other company in the country at that time in the use of advanced technology. Before arriving in West Texas, I had never looked at an instrument, except to take a reading. When the instruments arrived, there was no one around to ask for help. I had to get the instruments out of the boxes, read the instructions, and teach some cowboy how to assemble and get them working. Those men were, however, very good with their hands and hard working.

STURCHIO: What kind of process were you using to make the hydro- chloric acid?

CHURCHILL: It was a unique process. We drilled a water well. We pumped the water from that well back down another well into a salt bed 2000 feet below the ground. The water dissolved the salt and made brine. We treated the brine with calcium carbonate and caustic soda, both of which we made. The treated brine went into Hooker Type-S cells. An electric current was passed through the cells, making chlorine and hydrogen and caustic soda. Everyone else sells the chlorine and hydrogen. We burned the hydrogen and chlorine and made hydrochloric acid. We also evaporated the water out of the caustic soda. We used a small fraction of the caustic soda for treating the brine. We took CO_2 off our stack gas and used that with caustic soda to make the calcium carbonate which we also needed for treating the brine. We were in the middle of a natural gas field, and we could buy offgas for almost nothing. We ran gas-driven compressors to generate electricity, and that's what we ran the cells on.

So we had no raw material entering our plants at all, except the cells, which we bought from Hooker. We trucked and sold all of the products within a fifty-mile range of our plant, so this operation was phenomenally profitable. The nearest caustic soda plant was at Freeport, Texas, which was eight hundred miles away. Their freight was more than our cost of manufacture. The nearest hydrochloric acid plant was in Dallas, four hundred miles away, and that product is eighteen percent water. So, these competitors just gave up, and we suddenly had the whole of West Texas as our exclusive market. We could not make enough product to meet the demand. We totally underestimated the market.

MARCHESE: How large was it?

CHURCHILL: I cannot remember in tons. We had originally sixteen and eventually thirty-two cells. The wisdom from Hooker was that one had to have six hundred cells to be minimally profitable, but our operation was different in every respect and very profitable even on such a small scale. On paper, our plant was supposed to pay out in six months, but because of some delays in startup, it took eleven months.

STURCHIO: It's still phenomenal.

CHURCHILL: That is absolutely phenomenal. In addition, we sold raw brine. Actually, we could have shut the plant down, and sold all of the brine for regenerating the zeolite treaters everyone used for softening the water. Culligan picked up our brine in trucks. They could not afford the cost of drilling a well in comparison to our charges for the brine.

STURCHIO: So that explains why Frontier Chemical was in the middle of nowhere.

CHURCHILL: The location was the key to its success.

MARCHESE: It sounds like a smashing success. Did Curtis Cannon live up to his promise and make you a partner?

CHURCHILL: No, but I have no hard feelings in that respect. After I had been there less than a year he and his two partners sold out and became instant millionaires. They had invested $25,000 and sold out for several million. Curtis Cannon retired at about forty. One of the partners, Eddie Childs, now owns the Houston Astros, among other things. The other is an executive of the Republic National Bank in Texas. Actually, they made millions independently of the Frontier operation. Because by acidizing oil wells, they knew where all the successful wells were and leased promising land in advance of drilling. They ended up owning many acres adjacent to successful drillings. They became two of the richest men in West Texas.

MARCHESE: What did you get from them?

CHURCHILL: Nothing financially. I left when informed that they planned to sell the plant. I would have been just an employee of Vulcan Materials, the new owner. I was not forced to leave, but I could not see a promising future there. That's when I went back to graduate school.

STURCHIO: The atmosphere of Frontier before the sellout must have been gratifying?

CHURCHILL: Well, it was a gratifying accomplishment but I was right in the place that I wanted to avoid at Shell. It was a 24-hour-a-day job. You drove twenty-five miles to Seagraves at the end of the day only to get a phone call to go back and spend the night fighting some emergency. We blew up the plant once. We had our adventures. The place was lousy with rattlesnakes. We even had a rattlesnake in our office. I don't know what would have happened had Cannon stayed on.

STURCHIO: How large an operation was it? How many people were there relative to what you had been doing at Shell?

CHURCHILL: All told, there might have been twenty-five people involved, counting all the operators, secretaries, and accountants. It was a very small-scale operation as chemical plants go. The purchasers eventually expanded the operation and built several other similar plants.

MARCHESE: Weren't you tempted to try to get some venture capital and repeat the operation somewhere else?

CHURCHILL: No, exactly the opposite. I saw what had happened to Cannon. He had been a very nice person and very relaxed. He became almost neurotic because of the pressure of making that operation go. For a long time it was very touch and go. He had people waiting for our products long before the plant was running. However, it did not change his relationship with me.

[END OF TAPE 1, SIDE 2]

CHURCHILL: I could not see myself in that life style. Eventually Cannon started another similar successful venture in New Mexico and I had some further involvement with him as a consultant. That plant was also very profitable.

STURCHIO: When was that?

CHURCHILL: At the time I was coming to Pennsylvania, about 1967 or 1968. I also revisited the original plant at West Texas at that time.

STURCHIO: Would you like to tell us about the time you blew the plant up in West Texas? Is there anything else to say about it?

CHURCHILL: There were tremendous electrical storms out there and we had frequent trouble with lightning knocking out our power. We were floating on the local power plant, and when we generated excess power it went to them. One night there were perhaps twenty power failures. Every time our operation or theirs would crash we were out in the storm trying to straighten things out. During one of these power failures, the hydrogen and chlorine flashed back and blew up many of the cells. I remember watching the cells going up in the air--they looked like giant ice cream cones. Nobody was hurt, but a lot of equipment was ruined. We soon put things back together.

STURCHIO: This process was Cannon's invention. Were there any patents, or did he keep it as proprietary information?

CHURCHILL: I do not know whether or not there were any patents on that first process; probably only on peripheral aspects.

STURCHIO: At Shell, did you get involved in patent activity?

CHURCHILL: When I was at Shell I once took some samples during an upset of a butane isomerization plant. The product looked peculiar, and I found that we had made neohexane, which at that time no one knew how to make economically. I went back to the laboratory and found that when the catalysts we were using for butane isomerization became sludged they would make neohexane preferentially. I wrote a prospectus for a patent, but Shell decided they wanted to investigate the process further in Emeryville. After I was working for Frontier Chemical I was reading Hydrocarbon Processing one day, and there was the patent --exactly as I had written in my notes, but without my name on it. I wrote a letter of inquiry to Shell, but I didn't get an answer from them. I realized I would have to sue, but there was nothing to sue for since I would have been obligated to assign the rights to Shell anyway. All I wanted was fair credit. I was somewhat irritated because they put some people's names on the patent who obviously had nothing to do with it. I didn't even know them.

STURCHIO: I can understand that. Speaking of Emeryville, you said that while you were still at Shell, you didn't see much movement other than up the ranks at that particular refinery. Was there much movement out to the development company in California?

CHURCHILL: It was very unusual to move from the operating company, Shell Oil, to Shell Development. They were two essentially separate companies. Although I worked for Shell Research when I worked at Houston--which was part of Shell Development--I was just on loan there. I really didn't think that this was a possibility. Also I'm not sure that with only a bachelor's degree I knew enough to be comfortable in a research organization. When I informed Shell that I was going to leave, giving sixty days notice, they wanted to know why I was leaving. They found it impossible to believe that I wasn't put off by something. They immediately offered that if I wanted to go to their New York office or even to Emeryville, they would be glad to arrange it. However, I had made an ethical commitment to Cannon. I often wondered if I had previously gone in and said that I wanted a different assignment, whether they would have made that offer or not. I doubt it. However, I have no regrets in that respect.

MARCHESE: Did Shell or other companies have the policy of taking promising young men and giving them an advanced education, so that in time they could have had advanced degrees, if they had stayed with Shell?

CHURCHILL: No. We asked. I started to take one course at Washington University when I was with Shell, but I was switched to shift work and couldn't go anymore. I had to drop out in the middle of the course. Shell did not encourage such efforts.

They said that if you had so much time and energy left over, they would find something additional for you to do. This may not be their current attitude.

STURCHIO: You decided after the sellout at Frontier Chemical that there was a restless feeling. You felt that maybe you did want to go back to Michigan after all. Was there any thought about going somewhere else? Had you maintained contacts with the faculty at Michigan?

CHURCHILL: I had not maintained much contact. I had seen some of the people at AIChE meetings. I had talked to Katz in maybe 1945 at the AIChE meeting in St. Louis about the possibility of coming back for graduate work. He said, "If you're interested, why don't you write to me?" When I decided to go back to graduate work, which was maybe six months before the transfer of the company became effective, I wrote to Katz. I also wrote to Love. I was at that time torn between going back to chemical engineering or to mathematics. I actually had perhaps more formal advanced preparation for mathematics. Clyde Love said I would be foolish to do this. If anything, the five years' experience would be looked at as a negative factor by mathematicians. On the other hand, it would be an invaluable asset for chemical engineering. So, that's what I did.

MARCHESE: And you returned to Michigan in 1947?

CHURCHILL: Yes.

MARCHESE: Were there other reasons why you went to graduate school, other than the disenchantment with the lifestyle of the person who advanced at Shell?

CHURCHILL: At both Shell and Frontier, when I became involved in things, I would never get to complete them. I was often very interested in improving something. In the urgency of operation,if something is working, you leave it alone and do something else. I thought I would rather do research or teaching. That was really my motivation for graduate work. When I considered mathematics, I was thinking only about teaching.

MARCHESE: Was there a strong attachment to become an educator, rather than an industrial chemical engineer?

CHURCHILL: I had not made a decision as to whether I would do research in industry or teach. I was hoping for a faculty position, but I was not well informed about such opportunities.

STURCHIO: Of course, there were urgent opportunities in the chemical industry at that time. Was that part of your thinking, that if you went back and got your Ph.D., you would be able to write your own ticket?

CHURCHILL: I thought that even if I went back to Shell, I would have a much better position. I had confidence that with a Ph.D. I could obtain a more satisfying job.

MARCHESE: You got your M.S.E. in 1948?

CHURCHILL: Yes.

MARCHESE: Did you become a research assistant while you were getting your M.S.E., or after you received it?

CHURCHILL: I used my savings to go to graduate school the first year, took all course work and completed the master's degree in that one year.

MARCHESE: Did you find that the department had changed over the four years? Were there different personnel? Was the atmosphere different?

CHURCHILL: Well, there was probably a fifty percent turnover, but I knew most all of the faculty at least by name. Many of them were either former graduate students who were around when I was an undergraduate, or former faculty. R. R. White, with whom I eventually worked more closely than anyone else, was new. Joe Martin and G. Brymer Williams came that same year. Of course, Brown and Katz were still there, and Brier and Foust were back. So, I knew almost everybody. They didn't know me particularly well, but I felt quite at home.

STURCHIO: This would be a good time to talk about the impact of applied mathematics on chemical engineering around that period. By then there had been Sherwood and Reed (11) in 1939, and Marshall and Pigford's text in 1947 (12). MIT and Minnesota had begun courses on applied math. Back in 1948 Michigan produced one that Martin taught.

CHURCHILL: Yes. I took that course the first time it was offered. We used the books of both Sherwood and Reed and Marshall and Pigford.

STURCHIO: There must have been a sense that something was happening in chemical engineering with all this going on.

CHURCHILL: I was absolutely stunned by the changes that had taken place in five years. If I had had any sense at all, I would have gone back and done a year of undergraduate work instead of going directly onwards, because the curriculum had changed overwhelmingly in that period.

MARCHESE: In what way?

CHURCHILL: They were now teaching engineering thermodynamics, reaction engineering, and applied mathematics. The faculty members at Michigan were in the process of writing a book on unit operations (13). There was a great step forward. Almost every undergraduate course was unrecognizable.

STURCHIO: Do you think it had anything to do with the war years and the problems of scale-up in various kinds of enterprises, like the catalytic cracking work you had been engaged in? Had the faculty at Michigan had similar experiences that led them to reconceptualize the way they wanted to teach?

CHURCHILL: I think it was more a transition of a generation of people. Katz and Foust and Brown, who were in a sense the older school, but not that old, were receptive to change. The younger generation of Ph.D.s--York, Brownell, Banchero, R. R. White, J. Martin and Sliepcevich--had studied a lot of mathematics and engineering science. These people brought in much new material. Also, several seminal textbooks had appeared. Hougen and Watson was the first book on chemical reaction engineering (14) and Sherwood and Reed and Marshall and Pigford, perhaps the first ones on applied mathematics.

MARCHESE: Did you find that what was being taught fit nicely into chemical engineering problems, or was there an awkwardness initially?

CHURCHILL: Joe Martin was one of the great all-time teachers, and so I never felt any awkwardness with him. Of course, I was overprepared for his course. This may have been a great stroke of luck. When I came back I had much more mathematics than any other student. Martin and I had instant rapport in that regard. Even as a student I helped to develop that course.

STURCHIO: How many students were in the program?

CHURCHILL: When I went back to graduate school that year, there may have been two hundred graduate students in chemical engineering at the University of Michigan. I had graduate classes with fifty or a hundred students. I probably would have been psyched out in advance if I had anticipated this. They were almost all veterans who had returned to school on the G.I. Bill. This influx was caused by a five-year backlog of people with full financial support. All these people knew they needed retraining. At first I felt totally out of place, but the five years of industrial experience saved me. Because of my background in mathematics I immediately plunged in and took more math courses and obtained the equivalent of a master's degree in mathematics at the same time. I was very rusty in thermodynamics. I took the graduate course in engineering thermodynamics without the preparation of an undergraduate course. This was a traumatic experience. The next semester, I went back and self-studied the undergraduate course and did all the problems I could find because I realized I would have to pass the qualifying exam on that material. I was also self-studying the undergraduate notes on unit operations, all of which was totally new to me. We weren't covering that in graduate courses, but I felt that if I didn't do all that background work I would never catch up with the other students.

MARCHESE: What kind of mathematics did you find in that course? What were you being taught to apply?

CHURCHILL: I would say Marshall and Pigford as well as Sherwood and Reed were typical of the types of things we were doing. We were solving problems in conduction, convection, distillation, and separations using Fourier series and Laplace transforms--the kind of things that are now standard mathematics for engineers. Our undergraduates at Penn now study what I took then as an advanced course.

STURCHIO: What were the texts in your thermo courses and in any other courses you took in your first year of graduate school, in addition to the applied math texts? Was it the same generation of texts that Brown used before?

CHURCHILL: No text at all. Brown didn't lecture. He used the Socratic method of asking questions. I was bewildered, because I didn't know where they were coming from--except for the home problems. I was really desperate. Fortunately, the year before I came back to graduate school, I bought Dodge's Thermodynamics (15) and self-studied it. Although I couldn't read it with complete understanding all by myself, it was still a big help. Also, I had bought Ruel Churchill's books on complex variables and modern operational methods and had self-studied them as well (16).

STURCHIO: That is while you were at Shell and Frontier?

CHURCHILL: At Frontier.

STURCHIO: Is that Churchill any relation to you?

CHURCHILL: We once decided that we are probably sixth or seventh cousins. Our families came to the United States and Michigan by the same pathway.

STURCHIO: At that time were other engineers in equivalent positions to you in industry doing similar things? Were people reading on the outside? At Frontier, you must not have had a large library to consult when you came up with a problem.

CHURCHILL: The nearest library was ninety miles away in Lubbock. So I didn't really use the library. I just ordered books through Ulrich's Bookstore at Michigan. I had written back to Katz and Love and asked what books I might study. Katz suggested I study Dodge.

MARCHESE: In 1948 and 1949 you were a Research Assistant, and in 1949 you became a Research Associate. What was the difference, and what were the responsibilities?

CHURCHILL: It was sort of a promotion. During the war, J. C. Brier was in charge of Southwestern Proving Ground in Hope, Arkansas, where they tested ammunition. When he came back, he had had a contract with the Army Chemical Corps to study the ignition of propellants, with which he had a great deal of trouble. He offered me a job working on that contract, and I actually developed my doctoral thesis out of some of that work. He had three or four other people on that project. Two were physicists and the others were chemical engineers. I guess he was pleased with my work, and he promoted me so I would have more money the next year.

STURCHIO: Did you continue that when you started to teach?

CHURCHILL: Yes. In 1950, Brown invited me to be an instructor. I guess I made a good impression on certain people, because Katz later told me that Brown had suggested hiring someone else and he said, "Why don't you hire Stuart Churchill?" I plunged into teaching the next day. I really felt over my head. Brier was a little unhappy because I was already committed to working full-time for him. So I went half-time on the research and half-time on teaching.

STURCHIO: What kind of course load did you have?

CHURCHILL: The first semester I only taught two sections of stoichiometry. However, I realized that I was very unprepared. I did not want that to happen again. So after a week I went in and asked Brown, "What am I going to teach next semester?" He said, "Maybe the unit operations course." So I thought I would sit in on it. I walked into Katz's class, and he saw me sitting in the back. He said, "Are you back there to learn about teaching?" And I said, "Yes." "Well," he said, "When I'm away, I will let you teach this course." I never saw him again. [laughter] I taught perhaps half of the classes for the rest of the semester because he was involved with some consultation out in western Canada involving tar sands. Sliepcevich, who was teaching the other section, found this out and I ended up taking many of his classes as well. I really didn't dare say no because I so desperately wanted to become a faculty member. That was a difficult but inspiring time.

MARCHESE: At the same time you worked on your Ph.D. research?

CHURCHILL: Yes. I joked about working half-time on my research project, full-time teaching, and half-time on my thesis during that period, and that's sort of what happened.

STURCHIO: What books were you teaching from?

CHURCHILL: In the stoichiometry course, I taught out of Hougen and Watson (14). But in the unit operations, we used the notes that became Brown and associates (13). They were in sort of half-baked form. Then I taught thermodynamics with Martin, and I think we only used his notes. That was the classical procedure at Michigan, which I guess I have always fallen into. These people always passed out notes. They were always writing a book.

STURCHIO: Well, it seems to be common in many areas. We just got a set of notes for Fred Billmeyer's textbook on polymer chemistry that was passed around in the early 1950s, before it became a book (17).

CHURCHILL: I later used <u>Transport Phenomena</u> by Bird, Stewart and Lightfoot (18) in its preliminary note form, on a trial basis.

STURCHIO: In many cases, when you sent information to the authors, did you find that they did try to revise the text on the basis of the feedback?

CHURCHILL: Oh, I'm sure. I certainly appreciate such suggestions, particularly when someone writes thirty or forty pages of corrections and comments. Although to tell you the truth, Bob Bird never acknowledged the fifty or so pages of corrections which I sent him, but that may have been just an oversight.

STURCHIO: By 1950, you're working on your thesis, working with Brier, teaching what seems to be an overload of introductory courses.

CHURCHILL: That was probably my worst semester. I was able to taper off after that, but I was always involved in a course or two I hadn't taught before. For instance, as I said before I had studied engineering thermodynamics for the qualifying exam, but I had never had an undergraduate course. As an instructor in that course I had to be a student as well as a teacher. I was sitting in on the lectures on new material one day and teaching a recitation section on them the next. I was also taking graduate courses at this time. At Michigan, the students took an incredible load of graduate courses at the doctoral level, a practice which seems to have vanished at most schools, including Penn, which is I think a loss.

MARCHESE: Were you being graded for them, or did you just audit them?

CHURCHILL: Oh, no. We had to take for credit courses in depth in at least two other fields, such as physics, chemistry and mathematics. Beyond the master's degree we also had to take perhaps six to ten courses in chemical engineering itself.

MARCHESE: What portion of your time were you able to devote to your dissertation?

CHURCHILL: Not much during the academic year, but part of my work as a research associate was complementary to my doctoral research, because a lot of the equipment was used simultaneously. Also we worked longer hours in those days. The graduate students were always around the building until midnight. Things have really changed in that regard.

STURCHIO: Who were your fellow graduate students at the time?

CHURCHILL: I know where most of them are today and see many of them periodically. Matt Gilkeson is currently with the ASEE in Washington, Herb Wolfson, Bill Doerner and Claude Corty work for Du Pont in Wilmington, Jim Knudsen is at Oregon State, Ed Young is on the faculty at Michigan, Jim Kohn at Notre Dame and Cheddy Sliepcevich at Oklahoma. Jake Eichhorn and Dale Biggs are with Dow at Midland. We have stayed fairly close because we see each other periodically at AIChE meetings. My fellow graduate students are one set of people with whom I maintained a continued association. Next to my own doctoral students, they are my best and largest group of friends. The sense of camaraderie was very, very high at Michigan. We worked, ate and relaxed together. We studied and philosophized together. Despite the very large number of people, that was one big happy family.

STURCHIO: Did the faculty get in on this as well?

CHURCHILL: I knew all of the faculty but less personally. They were fairly sociable. We went to a few social functions with them.

MARCHESE: Did they mellow because you were a graduate student?

CHURCHILL: Yes. There was a great social distinction between being an undergraduate and a graduate. The faculty paid little personal attention to the undergraduates outside of school, or at least to me when I was an undergraduate. As a graduate student you became part of the family. And I think that's true to a large extent at most places and in varying degrees to this day.

STURCHIO: In 1952, you and Knudsen taught a course on heat transfer and fluid flow. Did that come out of your research, or was a course needed on that?

CHURCHILL: Katz and Foust had been teaching a heat transfer course for a long time. I felt what they were covering was old hat and out of date. Knudsen had been doing a graduate seminar in which he worked with Katz in compiling the results of the research on heat transfer at Michigan. He was in the process of writing a book on heat transfer which later became very well known (19). Katz decided that year that we should each take responsibility for a third of the graduate course on fluid flow and heat transfer, and in that way we could spend a lot of time putting in new material. He wanted to change the course from one end to the other, and that's what we did. That effort didn't have much relationship to my research. It mostly came out of self-study. I particularly helped to upgrade the mathematical parts. Maybe a year later, I participated in a seminar with Myron Tribus. Technically that was perhaps the most profound experience in my life. He opened up a whole new world to me in heat transfer. We picked new technical things that were coming out in the literature to self-study and report on. It was a great self-education. This was a quantum jump in what we or anyone else had been doing. I won't say it was the most advanced course in the country, but it reached the level of the most advanced work in the world at that time.

STURCHIO: It seems that your paper on "Some Fundamentals of Energy Transfer" was significant (20).

CHURCHILL: No. I don't think that was a very significant paper. It just happened that Foust was supposed to give a lecture at an ASEE meeting, and about two days before the meeting he came and said, "I can't go. Why don't you give the paper?" And I said, "On what?" And he said, "I haven't prepared anything, so why don't you write something." I wasn't prepared to do anything profound on such short notice.

MARCHESE: Were you considering a teaching career during this time?

CHURCHILL: Yes, informally, because during that whole period, from 1947 through 1952, I was examining what the faculty were doing and thinking how I would do it differently. By 1950, I was committed to teaching as a career and hoped that I would have a permanent job at Michigan.

MARCHESE: Could you talk a little more about your work with Brier?

CHURCHILL: Well, Brier was of another generation, but he was supportive. He had good chemical engineering judgment, but he really just let me do what I proposed. I did have other people on my committee, including Katz and Foust who were more knowledgeable in heat transfer. My thesis was not really in Brier's area. I defined this problem when we were doing classified research on propellants, as something that could use the same equipment and still be publishable. The problem still exists with projectiles and solid rockets. How do you ignite the material? We were studying the flow of the hot gases from black powder. I realized there was no data in the literature for heat transfer at large temperature differences. There were data that industry used for a hundred-degree difference, but no correlations or data for a thousand-degree difference. That's what I studied, using the hot gas stream designed to ignite the propellants. I read the literature and decided it would be nice to measure the local heat transfer coefficient, the rate of variation of the local heat transfer coefficient around the cylinder, since that rather than the overall coefficient controls ignition. There had been only two or three such local measurements in the literature, perhaps two in Germany and one at MIT.

STURCHIO: So this 1955 paper on the convective heat transfer from gases at high temperatures was the work you carved out of the work for Brier (21)?

CHURCHILL: Yes. There are other things that came out of that project too, including the paper on the ignition of propellants with Brier (22).

MARCHESE: Is this problem considered to be of interest to chemists?

CHURCHILL: I guess the answer to that is yes. The work was first sponsored by the Army with howitzers in mind and later by the Air Force and the Navy. One year the Army decided to drop sponsorship, and at the same meeting the Navy and Air Force turned up and said, "We're interested in that work because of its applicability to rockets." Then the Army decided it wasn't so bad after all. I think the problem is still being worked on. You don't see much of the literature because it is classified. Many people, particularly the Russians, are still publishing articles on this subject. At what temperature will a solid composed of nitroglycerin and nitrocellulose ignite, and how long will it take to ignite? This is a very critical problem anytime you shoot anything, whether it's fireworks or a shotgun or a howitzer or a rocket. We identified a few effects that other people had not--the composition of the gases, the level of the gas temperature, and the velocity of the gas. One other student, Charles Thatcher, also got his Ph.D. on this work. He is now at the University of Arkansas. I never worked on that problem again after I finished my doctoral thesis, but I sometimes still read the literature.

STURCHIO: You finished this all by 1952. When did you get the offer as assistant professor?

CHURCHILL: I don't remember the date of the formal offer, but I remember Katz saying they would make me an assistant professor in the fall if I finished my Ph.D. by May 1. I had a temporary appointment in the spring, and they were probably making up the faculty lists for the fall.

STURCHIO: Although you dropped that train of research, the interest in heat transfer did overlap with the seminar you mentioned.

CHURCHILL: I've been doing related work on heat transfer and combustion all my life. But something else happened that caused me to make a major change in the direction in my research at that time. Sliepcevich, who was a good friend as well as a member of the faculty, had turned in a proposal to the Armed Forces Special Weapons Project. They were looking at assessing the effects of thermal radiation from nuclear weapons on the population; in particular, whether it was possible to shield people from nuclear weapons by using fogs or smoke screens. This may sound trivial now, but with a Hiroshima-type bomb that might have been possible. At the time Sliepcevich was about to go on leave to Monsanto for a year. He came to me and said, "I don't really think I want to get into this, but they've come and begged me twice."

[END OF TAPE 2, SIDE 1]

CHURCHILL: [Victor K.] LaMer was a famous physical chemist at Columbia. He and two or three others were on an advisory committee and they decided the United States should undertake an effort to evaluate this protective process. They asked Sliepcevich because he had done work on smoke screens during the war. He refused. Then they came back and said, "We must have some new bidders on this project." So he came to me and said, "Listen, I'm going to be away, so how about helping me put this together. We will double all the costs, so we're sure we won't be given a contract." We turned it in, and they decided to let us do the work. There were lots of repercussions over this because we were the high bidder. But the reviewers apparently thought we had a different and promising approach. Hoyt Hottel was one of those on the board. They apparently wanted badly to get some new blood into the work. Specifically, we were trying to predict how much of the thermal radiation from a nuclear weapon comes through smoke screens to the ground.

It was a frantic period of working and calculating, and we discovered we had a great need for better computing machinery. Large-scale digital computers were just coming along and we were continually pushing at their frontier in this work. Our first computers were IBM Card Program computers which we had to wire on the back of the board. The computer merely processed punch cards. At that time John Carr was building a new computer at Michigan and he said, "This is just the type of problem this computer is being built for." That computer never did work in time for us to obtain any results, but we did one way or the other obtain solutions to the problem of scattering through the atmosphere. One difficulty was that the students did not have any clearance at all, and this work was classified "top secret."

[laughter] So we posed the problem as sunlight coming through a cloud. It's really the same problem. So, all these uncleared students were working on this super secret project not knowing what it was all about. It was ridiculous to hide all our papers and notes, but the military did not take a loose view of that kind of thing. Sliepcevich never did come back to stay.

STURCHIO: He stayed at Monsanto?

CHURCHILL: He stayed at Monsanto for a longer period than expected, came back briefly, then went to Oklahoma. I inherited all of that research.

MARCHESE: Quite a bit of your early research dealt with radiative scattering. I counted seventeen articles published in the first ten years.

CHURCHILL: They all came out of or were inspired by that work. We were developing theoretical methods. These were very exciting days. [Subrahmanyan] Chandrasekhar, who won the Nobel Prize this year, was one of our consultants on the project. He had devised one of the elementary solutions for this problem. It was very inspiring to work with people of that caliber. I'm afraid that some of this work we are talking about may have contributed to the continued controversy over contamination of people in Nevada by fallout. I was in a discussion with several generals when one said, "What would happen to a cloud if one of these bombs went off inside it?" Off the tip of my tongue I said, "Nothing. The cloud will still be there when the explosion is over." They thought this was absolute nonsense. They decided to run a test to disprove my assertion. It proved we were right. I remember getting a phone call, and this guy said, "The cloud is still there." [laughter] The reason is that very little of the thermal radiation is absorbed by the droplets, and that just dumps to the air. The only way you can evaporate the droplet is to heat up all the air. There is a lot of air and after the fireball, which only lasts a second or two, the air hasn't become very warm, and the drops are still there. Later we published a paper in that regard (23). I had trouble getting clearance, even though it never mentions the word "nuclear."

MARCHESE: While we're still on the topic of radiation, in this area you published mostly with Chiao-Min Chu and G. C. Clark. Would you like to elaborate a bit on their effect on you and vice versa?

CHURCHILL: Yes. Chiao-Min Chu was a doctoral student in electrical engineering at Michigan. We had two desk computers, the type you hand punch. That's what we were using initially. He came by and asked if he could use one of them. We said, "Yes," and "What are you working on?" It turned out that he was working on a problem of radar scattering. We started comparing notes and found out that we were really working on the same fundamental problem. We developed a tremendous rapport and interaction. We worked together on research for maybe five or ten years. He never had security clearance, which was always a touchy problem. He is a true genius and a lot of the things that he did were a great contribution to the field. He was really an intuitive mathematician.

I remember talking about this work with [George E.] Uhlenbeck at Michigan, a famous physicist who discovered the spin of the electron. He said, "Well, I am glad engineers are working on this problem because it's insoluble, and the physicists would know better." Peter Debye was then at Michigan. I used to talk to him about our work some afternoons, but I couldn't tell him exactly what I was working on. He gave us some very good suggestions and was very supportive. Chu is now a professor of electrical engineering at Michigan. I've seen him a few times, and we write Christmas cards, but that's about all. George Clark was initially a doctoral student of Sliepcevich who switched to me when Cheddy left. He did some of his thesis on this project. We simulated a cloud with an aqueous dispersion of latex droplets with the same index of refraction. He determined the effect of proximity of the particles in such a dispersion.

MARCHESE: You had mentioned previously that the funding for the research, at least before the war, did not involve the federal government. Can you tell me the ratio of research being done for the federal government and the private sector?

CHURCHILL: Right after the war, the military services had high budgets which they did not wish to lose. They decided that because of their experience with the atomic bomb, radar and the proximity fuse, that it might be worthwhile to invest in research. In the period immediately following 1945, most of the engineering research in the United States was sponsored by the Department of Defense. Often it had only casual military implications. I think they looked at this research as an investment in public relations with the academic community. For the first fifteen years, perhaps ninety percent of my research was sponsored by the military. Although two of the items I mentioned had direct military relevance, most of the others did not. They sponsored my early work in combustion and said they were only interested in increasing the state of knowledge in that field.

MARCHESE: But what did the private sector do in order to get its share of research?

CHURCHILL: When I started my career there was $1 million a year in sponsored research, mostly industrial, at the University of Michigan of which $700,000 was in chemical engineering. A few years later, the total figure was $14 million. By then chemical engineering had about $1.2 million and the industrial research component was about $800,000, but it never went up thereafter. Then Sputnik came along, and the National Science Foundation put money in. So industry didn't exactly quit, but they never increased their participation. They still had great impact upon what was done because we all wanted to do work of industrial importance.

STURCHIO: Was it in 1952 when you began the scattering project?

CHURCHILL: I believe so. You could look at the publications.

MARCHESE: You had four publications on multiple scattering.

CHURCHILL: That was in 1955, so I think we started the work in the fall of 1952, at the latest 1953.

STURCHIO: Did you keep up your contacts with Chandrasekhar, Uhlenbeck, and Debye during this time?

CHURCHILL: Well, they kept up as long as I was at Michigan. I visited with Chandrasekhar when he was here a couple of years ago. Uhlenbeck left Michigan at about the same time that I did and went to the Institute for Advanced Study. I haven't seen him since. I think he has since died. Debye left and went to Texas, and then Cornell. The last time I talked with him was at a light scattering conference at Clarkson about 1960.

MARCHESE: Your publishing in light scattering ceased about 1965.

CHURCHILL: Eventually I turned this effort into industrial chemical engineering. Bert Larkin and John Chen (now chairman of chemical engineering at Lehigh) did nonmilitary research on radiative transfer. I then turned to natural convection and combustion, partly because of Bill Martini. Bill was the only student that I ever had who walked in the door with his own doctoral problem. He had worked for North American Aviation building nuclear reactors. One blew up because of a hydrogen-oxygen explosion, much like the potential one at TMI [Three Mile Island].

MARCHESE: Is that the paper you wrote with Martini in 1960 (24)?

CHURCHILL: Yes. The first one was in 1957 (25).

MARCHESE: Then a revised version was published in 1960?

CHURCHILL: Yes. He was one of my first students. He started about 1952 or 1953. Bill wanted to study how to put hydrogen and oxygen back together again slowly and without any moving parts. Somebody had the idea of a platinum catalyst and a condenser. He conceived this horizontal, cylindrical reactor in which the hydrogen and oxygen would rise and react with platinum along one vertical wall. The heated, reacted gas would then rise further, pass down over the cooled surface of the other side of the cylinder, condense and drip back into the nuclear reactor. That is the problem that we intended to study.

Bill was the first person to attempt to solve the problem of two-dimensional natural convection using a computer. The computer we had that year, an IBM 601, was not really big enough for this problem. So we used experimental temperatures to solve the momentum equations, and experimental velocities to solve the energy equation. The computed results agreed with his data. It wasn't a complete solution, but we had proved that the equations represented the physical behavior. People were not sure at that time whether these equations really defined the behavior. The next student who came along was David Hellums. He had a bigger computer and he solved the entire problem. That was the first paper ever on that subject (26). It is still cited frequently. Hellums invented or first utilized many of the methods that are used to this day for numerical integration.

STURCHIO: Were you using finite-difference methods because that did cut down on the size of the computations?

CHURCHILL: We had non-linear partial differential equations to solve. We didn't know any analytical method to solve them. I had learned in mathematics that it was possible to carry out such solutions for (linear) conduction problems. We were rash enough to think we might be able to do so for convective (non-linear) problems. It turned out that in 1928 Thom in England had solved a forced convection problem using this method, but we were not aware of his work. So we faced a completely new problem. Also, the problem of natural convection is much more complex than that of forced convection.

MARCHESE: Speaking of computers, I'd like to know what caused you to use computers?

CHURCHILL: Sliepcevich is responsible for that. He had a student named [R. O.] Gumprecht who used a computer to evaluate light scattering functions by the summation of series of pure, mathematical functions, which he also had to compute. Because it was during the war, they computed these functions in connection with smoke screens. They had thus put their foot in the door using computers, and that's why the military called Sliepcevich about the radiative problem. That's what forced us into using computers. However, the problem of natural convection involved a completely different kind of computation. I knew we could solve the linear partial differential equations, so we just bravely tried the non-linear ones and Hellums found a way.

STURCHIO: Where did you get the money to buy a computer? Since it was so early in the development of large-scale computers, it must not have been a simple matter.

CHURCHILL: We didn't need to buy a computer or even spend funds for computation. That was an entirely different era. Since the computer was there you could use it just as the library. Eventually we had to worry about funding. Martini's work was unfunded. The light scattering work was funded. Hellum's work was not funded. In fact, Hellums was supported on another project. David Hellums is the only student of mine who made four or five fundamental contributions. He developed a method for simplifying partial differential equations. He was the first person to solve both two- and three-dimensional natural convection problems with a computer and after he went to Rice, he developed a fluid mechanical method, which is still used in all kinds of problems.

STURCHIO: Is that where he is now?

CHURCHILL: Yes. He's Dean of Engineering. I had dinner with him there last week.

STURCHIO: Since we've been talking about Hellums and Martini, maybe we should talk about some of your other students.

CHURCHILL: Martini was incredibly driven to finish rapidly, working both night and day until he got his doctorate in less than three years. He did experimental and theoretical work that people still cite. He now works on Sterling engines at Richland in Washington. Peter Abbrecht started at the same time. He studied turbulent forced convection in tubes, and was the first person to measure eddy diffusivities on both heat transfer and momentum transfer. He made very precise temperature and velocity measurements which still are a standard resource. Peter had a curious career. After he finished his Ph.D. he went to work for

Upjohn. Then he worked for 3M, and got into biomedical research. He came back and obtained an M.D., and was subsequently on the faculty at Michigan. There he continued to "practice" engineering, studying the fundamentals of kidney function. Now, he's in Washington D.C. We still keep in touch. He was an incredibly good experimentalist.

STURCHIO: [Charles M.] Sleicher was another one?

CHURCHILL: Sleicher was not my doctoral student. He actually was a doctoral student of Myron Tribus, but Tribus left. This was before I got my Ph.D., and although I closely followed Sleicher's work, Katz became his formal doctoral advisor. He produced the first fundamental solutions for turbulent heat transfer. It was very fine work. He is now chairman of chemical engineering at the University of Washington in Seattle. He did work with me on the scattering problem. Charles is also responsible for the development of wine-making in Washington--at least good wines--an interest we shared in those days.

I guess the next student was Morton Moyle. He was doing work as a research assistant in aeronautical engineering, but wanted to do his thesis in chemical engineering. He had some interest and background in detonation, so we decided to study the detonation of hydrogen and oxygen mixtures at low temperatures. That involved both experimental work and computations. Mort worked for Du Pont and taught at Lehigh. He was killed in a traffic accident early in his career.

STURCHIO: He worked at the same time as Martini?

CHURCHILL: Yes. Then Roy Gealer came along. His work turned out to have a very unexpected aspect. He studied the effect of high pressures on detonations. He was going to study hydrogen-oxygen detonations up to pressures of ten thousand pounds per square inch, but at four thousand pounds, we blew everything to pieces. [laughter] The question was, "Why did the equipment blow up?" We finally decided that the detonation was triggered by a Teflon gasket burned in the oxidizing mixture. We tried to publish a paper on this in the AIChE Journal but they refused because the explosion represented an unplanned and uncontrolled experiment. We then presented a paper at the San Francisco meeting of the AIChE. The room was full of people, mostly from the nuclear establishment, who had also blown up hydrogen-oxygen reactors, and wanted to know why. At that meeting we confirmed the source of the problem. It was always a gasket, something like Teflon, that was presumed to be inert, but was not at that partial pressure of oxygen. Fortunately, Roy had done all of his other work at lower pressures, and so we stopped at that stage.

STURCHIO: A theme running through a lot of this work in the mid-1950s is the connection with the nuclear energy industry.

CHURCHILL: That was a big thing then. We then presumed that nuclear plants were going to be our total source of energy. G. G. Brown was Acting Director of Engineering for the A.E.C. Katz actually prepared a preliminary design for the power plant of the first nuclear-powered submarine, the *Nautilus*, for Admiral [Hyman] Rickover. All of that work was done in one weekend. That's an interesting story. Katz and the others were talking about the possibility of designing the power plant for a nuclear submarine. Rickover asked how long it would take, and the people from industry said five or ten years. Katz said, "Nonsense. We can do it over the weekend." [laughter] Rickover challenged him to prove his boast. So Katz came back to Ann Arbor and said, "Everyone drop everything. We've got a big job to do over the weekend." That's how the submarine power plant was designed. The final design was not much different. Katz had several of the doctoral students working on liquid metal heat transfer, and everything was right at his fingertips. That was Katz. When you have to do something, do it right now.

STURCHIO: Did that bring support to Michigan from Rickover?

CHURCHILL: I don't believe so, but we actually had one of the first nuclear reactors on any campus. I had one doctoral student, Bill Luckow, who worked on nuclear reactor dynamics, but his experimental work was done at the Argonne National Laboratory.

STURCHIO: Were a lot of the faculty at Michigan working on research problems in this area?

CHURCHILL: Katz was working on liquid metal heat transfer, Brownell was working on nuclear irradiation of foods and materials, and Martin was working on nuclear-induced chemical reactions. That was the general field in which the government was providing the most money, and generally the work was not classified. One necessarily floats to where the funds are available.

STURCHIO: Were the others using a computer? Was there support from the computing industry? This was before the Ford Foundation grant.

CHURCHILL: Not much before the Ford Foundation Project. Jim Wilkes, perhaps my most capable student, followed David Hellums and developed the first truly two-dimensional finite-difference solutions for natural convection. He became very mathematically inclined, and with Brice Carnahan wrote a book on numerical methods which became a best seller (27). Carnahan did his doctoral thesis with Joe Martin on nuclear-induced chemical reactions using the University of Michigan reactor. Warren Seider and Mike Samuels also worked on numerical methods with me. Eventually because of the Ford Foundation Project, Warren became interested in computer-aided design. We had good computing facilities for those days, which helped a great deal. In 1960, there were only three or four universities that had major computer facilities--say, Illinois, Michigan, UCLA, and MIT. Maybe there were a few others. It was a difficult field unless you had access to such facilities. Michigan decided the computer was analogous to a library. It was going to be there for you to use, and you didn't necessarily have to obtain funds. At other schools, people had to go out and get the money first, and the computational work never got off the ground. It was very slow going in many universities.

MARCHESE: Were you called upon by other people in the chemical engineering field as a source of information, so that they could introduce computers into their work?

CHURCHILL: Occasionally. So were a few others. Leon Lapidus began to work with computers at Princeton after his doctoral research at Minnesota and Neal Amundson was doing work with computers very early. A lot of people at the time were very anti-computer. It's hard to believe now, but many people then thought that this was an unfair way to solve problems, that it was not something university people should be doing. It was considered dog work.

STURCHIO: It was too crude or inelegant?

CHURCHILL: They were merely reacting defensively. That's what Katz discovered with the Ford Foundation Project. He found that if somebody knew how to use the computer, they no longer criticized its use. If they didn't know how, they always said, "It's not relevant." At that time very few people knew how. I was just lucky to be pushed into that field early.

MARCHESE: Did you deal directly with Amundson, or did you work independently of him?

CHURCHILL: Completely independent. I barely knew him. Eventually, I became aware of his work and learned a lot from it. I became chairman just about that time and we made an effort to hire one of his students. We thought of Neal then as being the best applied mathematician in chemical engineering (which I think is still true), rather than being a computer scientist. It was only later that I realized how much he was using computers because that phase was not as prominent as his analytical work.

MARCHESE: Did your mathematics interest lead to these kinds of applications?

CHURCHILL: Well, I think that that is one effect. I had a stronger mathematical background than most others in chemical engineering, and was looking for someplace to put it to use. That was my unique characteristic.

STURCHIO: It must have been exciting to have the computers around as well, so that you could combine the expertise in applying mathematics to certain problems with the brute force to actually crank out solutions that other people couldn't do.

CHURCHILL: Of course, but much of the work was very frustrating, and the computers we had then had less capability than modern hand-held models. The computer I have in my hand can solve problems better than the computer that Hellums used in his work. Wilkes may have been the first one of those students to have a computer better than a modern hand-held computer. It was also difficult to program those computers. You had to learn and use machine language. Two doctoral students in mathematics at the University of Michigan essentially developed Fortran IV. They were Bernie Galler, now Dean of the Literary College at Michigan, and Dean Arden, later head of the Princeton Computer Center. However, the Mathematics Department decided this was not mathematics. Their language was called MAD for Michigan Algorithmic Decoder. They supplied it to other universities, and IBM soon adopted and adapted it because of the distinct edge it gave them over everybody else. Dean Brown said, "If you won't give these men a Ph.D., we will create a degree in computer science and give them that degree." The mathematics department relented, so Galler and Arden actually obtained their degrees in mathematics.

STURCHIO: What year was that?

CHURCHILL: 1958 or so.

STURCHIO: It was in the mid-1950s that ENIAC was developed.

CHURCHILL: I used the ENIAC. We may have run the first major program on it after it was reassembled at Aberdeen. The light scattering calculations by Gumprecht were done in part on the ENIAC. George Clark also used ENIAC. We must have run twenty or thirty major programs on the ENIAC. Computer software was then the major difficulty. I remember Clark working three weeks with a desk computer trying to find a mistake in a program that would never converge. He finally found a near singularity. We could not identify the difficulty until these hand calculations revealed that thirty significant figures weren't enough at one point in the calculations.

There was no one around to help you. When Hellums was developing an algorithm for the computation of natural convection, there was no book that said, "Do this." We invented or rediscovered many of the fundamental methods.

STURCHIO: Did you encourage Hellums and Clark and your other doctoral students to get a very strong mathematics background?

CHURCHILL: Yes. They all did. My students knew that if they worked for me they would have to take all the available classical mathematics and physics. They did, and they were supportive of each other.

MARCHESE: Looking back at your career, you had a very strong expertise in mathematics. You said earlier that those without the mathematical background tend to gravitate to industry. Do you find a correlation there?

CHURCHILL: I think so. I think if you're an experimentalist you became very frustrated in a university because you work with such poor equipment and without technical assistance. Although many faculty members do experimental work, it is a tough life. Whereas, in industry or governmental laboratories you have much better support experimentally. On the other hand, theoretical work is easier in a university. You can always find somebody to help you. Those who have a strong desire to do precise experimental work in engineering usually end up working in industry or governmental labs.

MARCHESE: I noticed that in your work you not only stressed calculations, but you also stressed the necessity for more careful experimentation.

CHURCHILL: I think that all but one or two of my students did experimental as well as theoretical and/or computational work. We always felt that we had to prove that what we computed was physically valid.

MARCHESE: Did R. R. White have an effect on your career?

CHURCHILL: I suppose that White had more influence on my career than any other single person. We often talked more about the philosophy of research and education. He was a brilliant idea man who rarely finished anything. He would suggest things and persuade someone else to go do them. He was not very mathematically inclined, although he was certainly capable in that respect. He was an eclectic person who liked to obtain first-order and novel solutions, but he became bored very rapidly. It is too bad we lost him from the chemical engineering profession. He had many very original ideas. Not all of them were right, but they were invariably thought-provoking.

STURCHIO: What happened to him? You said he was lost to the profession.

CHURCHILL: He left the University of Michigan mid-career. He was sort of Brown's disciple, and when Brown died he felt cut off. He needed a new job and a new challenge every six months. He quickly became bored doing the same thing. He got an offer to be Director of Research for ARCO in Philadelphia and served there for about four years. He really turned over the organization and had a great impact. Then something happened, perhaps the president left, and White left. He was briefly Vice President of Arthur D. Little. When I say briefly, I mean about six months. Then he was Vice President of Champion Papers for about two years. He went to Grace Chemicals as President for about six months. Then he went to Case Western Reserve University as Dean of Management Science. He soon went to Washington with the National Academy of Science about fifteen years ago and has been there since. As far as I know he has had no impact on chemical engineering research or education since he left the University of Michigan. I have seen him on television a few times. He runs a program called "The Forum," but this is clearly a waste of his intellect.

[END OF TAPE 2, SIDE 2]

INTERVIEWEE: Stuart Winston Churchill

INTERVIEWERS: Joseph C. Marchese and Jeffrey L. Sturchio

LOCATION: University of Pennsylvania

DATE: 28 March 1985

MARCHESE: Professor Churchill, do you have any remarks you wish to make now apropos the first portion of the interview?

CHURCHILL: We were speaking of the academic influence at Michigan. That is, how many people went into teaching. The University of Michigan and MIT were totally dominant in doctoral work and graduate work in chemical engineering in the 1950s, and perhaps up to 1960. Some very large fraction of all Ph.D.s, and therefore, all teachers, came from those two schools. At both MIT and Michigan, faculty members wrote many influential books. But some changes occurred at about that time. First, other schools began to get into the act. Second, the faculty at MIT turned its attention almost entirely to the Practice School, consulting, and their own small companies. From then on, they had almost no influence on education. Very few of their people went into teaching. Some of those who did were outstanding, but they were exceptions. Both MIT and Michigan also had been doing something that was injurious in the long term. They only hired their own students as faculty members. MIT had mostly MIT graduates on their faculty for forty or fifty years. All of the effects that you would expect from inbreeding occurred. They didn't know what was going on outside, and they lost their influence with the rest of the profession. Michigan, to a great extent, did the same thing. The only difference was that Ph.D.s from Michigan continued to go into teaching. These factors led to some loss of pre-eminence for both schools.

STURCHIO: When would you say that Michigan and MIT began to decline in pre-eminence, and which schools grew stronger? Wisconsin, for instance?

CHURCHILL: Perhaps in the 1960s. Both schools began to hire outsiders but the damage was already done. California at Berkeley, Wisconsin, Minnesota, Princeton, Delaware, and Illinois rose to the fore. These were all good schools before. For example, Princeton certainly had very few Ph.D.s before 1950, but suddenly, it became a much more prestigious place for graduate work.

STURCHIO: In 1950, let's say, Michigan and MIT were pre-eminent. In 1965, was this still the case?

CHURCHILL: Michigan and MIT kept on doing the same things very well, but other schools were doing new and different things. None of the innovations in education in the 1960s came from Michigan or MIT. The book by Bird, Stewart and Lightfoot was written at Wisconsin (18).

STURCHIO: We spoke last time about Wisconsin's work, the transport phenomena, Amundson's work at Minnesota, and his work on applied mathematics in chemical engineering. Of course, we've been discussing your work. Did Delaware and Princeton have a similar orientation toward applied mathematics?

CHURCHILL: Delaware did through Robert Pigford. Neal produced many outstanding disciples. Lapidus went to Princeton and [Andreas] Acrivos to Berkeley and eventually to Stanford. I think Neal's influence came primarily in that way.

STURCHIO: So it was a second generation?

CHURCHILL: They constitute a second generation but Neal kept on as a leader himself. I also believe that the impact of books is hard to underestimate. People associate new development with books. Everybody else was getting into reaction engineering and transport phenomena at the same time as Wisconsin, but they documented the trend and rightfully earned the credit and influence.

STURCHIO: Your book on rate processes is one that's well known and widely respected (28). In the literature of science and technology studies, there's a shibboleth that scientists like to read and write literature, but engineers don't. Written works have a larger influence in science, whereas engineers pay more attention to practice. What you're saying is that many of these things are not entirely true.

CHURCHILL: Somebody has to write the influential books, and it may be the people who are a little outside the field. Bob Bird's background is largely as a physical chemist rather than as a chemical engineer, but he was probably influenced in chemical engineering by Hougen and others, and they may have given him the push to do something different. Neal Amundson was not only primarily a mathematician, but for a long time he hired nothing but chemists and mathematicians at Minnesota. I believe he did so because, for better or worse, he could in that way personally influence the direction of chemical engineering at Minnesota.

MARCHESE: What effect did Transport Phenomena have upon your own work and your own development? Would you also compare and contrast your approach with theirs?
CHURCHILL: It did not have a great impact. Jim Knudsen and I were doing much the same thing in heat transfer, but we had not articulated this direction nearly as well. During all my career at Michigan, we never used Transport Phenomena as a text except for the trial I mentioned previously [see page 32]. Nevertheless, I think that it is the most important book in chemical engineering that has been written in my career.

The rate concept came out of the paper presented at the 1958 meeting (29). We were asked to write a paper on the experimental foundations of chemical engineering, and we found that there weren't any. Everywhere we looked, the available data didn't confirm what people thought they did. This was a revelation to both of us. The idea and methodology of critically examining rate data is to be credited to R. R. White, but the rate concept, that is the distinction between rates of change and process rates, is mine rather than his. That experimental foundation paper was certainly written on his initiative, but he and almost everyone in the field confused rates of change with process rates. The article by [Robert] Kabel states that very well (30). There were predecessors who said this, in fragmentary form and only for reactors. We quickly recognized that this concept was very useful as a structure in teaching.

There's another thing about that concept which was unintentional. All previous books in chemical engineering, such as the transport phenomena book, were primarily physical. Although we think of chemical reactions as being the central part of our field, the unit-process approach, the unit-operations approach, and the transport approach all ignore chemical kinetics. They are all concerned with physical processes. Hougen, in his book on chemical reaction engineering (14), introduced kinetics as a subject in chemical engineering, but he didn't integrate the treatment of kinetics with transport. The real objective of my rate book is the demonstration that all transport processes and reactions can really be treated in the same manner. We also looked at cutting down on the amount of information the student had to carry around. If he had such a structure he could shed a lot of uncorrelated information.

MARCHESE: You emphasized synthesis of the simplest models necessary to explain a phenomenon?

CHURCHILL: Yes. We were looking at where information on rates comes from, how you make measurements, how you interpret them, and how you use this information. We looked at that whole sequence as a continuum. I don't know that that has ever been done before or since in chemical engineering.

STURCHIO: What kind of effect did it have for the next couple of years? After all, the paper could be seen as a refreshing look at a problem in the fundamentals of chemical engineering or a troublesome look at the way people had been doing things wrong all along. Presumably, it either excited or provoked a lot of people.

CHURCHILL: The paper and the subsequent book were great artistic successes. Many, many people still speak favorably of that paper. Charles Forman once said that it was his favorite paper. It made people think a lot. There were people who quarreled with the idea that their correlations might be works of imagination instead of having a firm basis. There was some negative feedback, but I think that idea was hard to resist and has been generally accepted.

MARCHESE: Have you seen an improvement in the work that's done? In that paper, "Experimental Foundations of Chemical Engineering," you indicated that "Present knowledge as represented by the literature has the appearance of correlation resting upon correlation, theory upon theory, and the data presented consist of a few graphs greatly reduced in scale" (29). You went on to say that "rationalization of data, in terms of models and mechanisms, is of paramount importance for scientific and technological development. However in the absence of an understanding of the quantity and quality of the data being rationalized, rationalization becomes meaningless speculation." Has the profession taken steps in the last twenty-five years so that you wouldn't write this again?

CHURCHILL: The above is still true, but not nearly as broadly. This is an uncomfortable concept for many people. They do not wish to face the reality that the data they use are not very reliable. That causes great insecurity. However, other things have happened. Firstly, instrumentation has improved so people do make better measurements. Secondly, theory has greatly improved so that the structure for analyzing and representing these data has expanded tremendously. Thirdly, computer solutions provide an adjunct to the experimental data and although shortcomings are there because of the use of erroneous models, at least there's no imprecision. And fourthly, I think the impact of our criticism made people very nervous about doing some of these same things again. But a week seldom passes in which I don't see some glorious example of these same erroneous interpretations we criticized.

STURCHIO: The thrust of this whole program seems to be to use theory to reform practice. You always discussed examples of real world phenomena--diffusion, conduction, convection--trying to come up with a much more sophisticated and precise theoretical understanding of what's going on in real reactors and real pieces of apparatus. Was that characteristic of what academic chemical engineers were doing at that time, or do you think that you really did have a unique approach to harness theory to practice and provide the experimental foundations for chemical engineering?

CHURCHILL: I think we did have some position of leadership in this area because we were among the first people to obtain numerical solutions. Most of the people doing theoretical work at the time were handicapped by the use of only classical analytical methods and therefore were forced to make so many idealizations that there wasn't much relationship with the real world. That 1959 paper forced us to be meticulous about what we did. We never wanted to be in the position of failing to conform to our own standards. This provided a strong motivation to avoid idealizations, to use sound structures for correlation, and to test our computed results against physical measurements.

STURCHIO: Of course, you always had the mathematical tools to be able to change very complex problems in partial differential equations, to transform the equations to things that could be handled and could be solved using the computers that were available at Michigan.

CHURCHILL: The critics of Bird, Stewart and Lightfoot's book--Bob Pigford said it was a bad influence on the profession--suggest it led chemical engineers to turn to problems they could solve instead of to problems that needed to be solved. That was not the fault of Bird, Stewart, and Lightfoot. They are wrongly blamed for that. But their book was complemented by a great thrust of engineering in general and chemical engineering in particular to develop a theoretical base. All of a sudden, people were off modeling and using engineering science. Of course, they drifted far away from practice. Perhaps the rate book was regressive. That is, we tried to focus on real problems when the main thrust of the field from Amundson and others was purely theoretical. It may still take some time to coalesce these thrusts.

MARCHESE: Were there other factors that led them in this direction?

CHURCHILL: Well, the computer, of course, made it possible for people to do more things theoretically. The years right after the war saw a great turning point in mathematical preparation. In the 1950s, all graduate students began to study applied mathematics. Those who were good at it immediately put it into practice. Also, the ECPD, through accreditation, told schools that they should be teaching engineering science and not practice. That had a profound impact upon education in chemical engineering.

STURCHIO: What implications did that thrust in chemical engineering have for the views that industry had of the utility of chemical engineers at the time?

CHURCHILL: It caused an instant and great shock because up to that time, when students left school, they could communicate with other engineers and go out and practice what they were doing in school. There was no gap, except in maturity. All of a sudden, the graduating students talked a different language, didn't know anything about practical problems, and had a new capability which nobody in industry knew how to use. There was a lot of antagonism. The large companies soon adapted and gradually these students changed engineering practice. That's no longer an issue, but there was a period of turmoil.

STURCHIO: That would be in the mid-1960s?

CHURCHILL: Yes.

MARCHESE: You wrote an article in 1964 called "Education of Chemical Engineers for the Aerospace Industry" (31). You dealt with challenges put forth by the aerospace industry that questioned the preparation of the students for work there.

CHURCHILL: I anticipated at that time that a large fraction of our students would go to that industry. That was probably overly optimistic or pessimistic, depending on how you look at it, because I thought the SST was going to be the WPA of the 1960s and would soak up, as NASA eventually did, an incredible amount of engineering scientists. When the SST work was rapidly phased out, the employment of our students by the aerospace industry crashed. There was a short period when a number of my students were working in the aerospace industry. That influenced me to do work on detonation and shock waves.

STURCHIO: It was a question of reentry vehicles and that sort of thing?

CHURCHILL: Yes.

STURCHIO: That article was one of a few that you were writing in the mid-1960s in the aftermath of the ECPD studies on engineering education (32). You've just spoken about the trend towards a somewhat austere and abstract new chemical engineering science. That seemed to be the way things were heading around that time. The ECPD/ASEE study that came about, first in 1965, and the final report in 1967, seemed to argue that engineering education needed to put less stress on scientific and engineering fundamentals and more stress on practical courses, oriented toward exactly what students might be doing in the industry. What impact did this have on Michigan? What impact did it have on the way that you and your colleagues viewed the role of educators?

CHURCHILL: I don't think that it had a unique impact on the University of Michigan. It had a unique impact on all of chemical engineering. The AIChE had always been a highly independent participant in accreditation. They started accreditation before the other societies and retain to this day a privileged position which causes continual jealousy and reevaluation. That cycle is going on again right now. This is partly because the AIChE has a stronger academic influence than the other engineering societies. At one time half of the officers and half of the active members were probably academic. When something like accreditation came up, they threw themselves into it, whereas the other societies delegated their authority and paid little attention.

We have always had a different position in accreditation than the other branches of engineering. When the Goals Report was written, it contained a number of premises which were unacceptable to the chemical engineering profession. The assertion was that a B.S. graduate was no longer employable by industry and, furthermore, that when people finished school, they no longer used calculus or engineering science. The authors of the report learned this by asking people ten years out what they were doing. Of course, the ten-year engineer who is a leader has long since dropped technical work and has become a manager. He's more concerned about personnel relations and economics than he is about calculus. He can't remember whether he ever used it or not. They used this data to draw the wrong conclusion. The other societies were somewhat passive. I was one of three AIChE representatives. One of the others was Howard Rase. We objected strongly and the AIChE backed us up. The AIChE led a successful battle to prevent that report from prevailing and as a consequence it essentially went down the drain.

STURCHIO: The motif in the various responses and rebuttals that you published around the time of the report was this emphasis on the fact that engineering students should be well rounded in the fundamentals. As you said a moment ago, they would have a framework in which to approach problems, rather than worrying about all the details.

CHURCHILL: The previous ASEE study of engineering (known as the Grinter Report) did have a profound effect. Through accreditation, schools were asked to devote roughly one quarter of their curriculum to humanities and social sciences and to teach engineering science. They were asked, if they had military science, not to make that part of the regular curriculum. Most of them reduced the total credit hours from the equivalent of 140 semester hours to 120. These several changes knocked out about half of the time that was available for engineering courses. People were suddenly forced to teach somebody to be an engineer in half the time. Furthermore, the amount of math and science was increased. Dropping all of the courses in practice was an over-reaction. Eventually some design work went back in. That crisis would have occurred even if the Grinter Report had not been written. Time has somewhat healed these stresses. Our students take about half the undergraduate courses in chemical engineering that I took as an undergraduate. At Penn they may take only two-thirds of what a student does at Drexel.

STURCHIO: Do they get the same amount of design courses?

CHURCHILL: They take less in number, but they're probably more firmly grounded in the fundamentals. We assume that they have enough preparation to survive by their wits. The students here are always a little insecure when they leave because they realize that the other students at some schools such as Drexel have a course in about five or six topics whose names they have hardly heard. In industry, they find that this doesn't really matter. The engineering science view has prevailed in the long run, but in the 1960s and 1970s it was a great topic of concern.

STURCHIO: Since we're now talking about the role that you played on this intersociety committee for engineering education, this might be a good time to talk a little about the activities that you were involved in with the AIChE in the mid-1960s. You were elected to the presidency, and you were involved in a number of initiatives with university-industry cooperation and international chemical engineering. Would you like to say a few words about that?

CHURCHILL: There was an expectation and tradition at Michigan that all faculty members would take part in activities of the AIChE and be leaders in that society. Almost every faculty member had been on the National Council; for example, G. G. Brown, Katz, A. H. White, R. R. White, and later Joe Martin and myself, so this was a natural kind of thing to do. I never thought of any alternative but doing this. After being on the

Council, I was not selected as a candidate for the presidency by the Nominating Committee, but I ran as a petition candidate and won. This is also a great tradition in the AIChE which they're now becoming very nervous about. It's always been a democratic society with truly free elections. That means they have had at least two candidates for every office and sometimes three. It separates the AIChE from other societies who usually nominate a single slate and therefore are subject to inbreeding. The ultimate impact is that the average president of AIChE has been something like forty-five years old, compared to almost seventy for the civil and mechanical engineers. It's possible for somebody to stir things up in the AIChE and do something in a few years instead of going through twenty years of service on committees to qualify for the inner sanctum.

MARCHESE: I'd just like to indicate that you served as the vice president in 1965, as president in 1966, and as past president in 1967. This is when you presumably had the greatest impact.

CHURCHILL: All people who serve the presidency go through those terms. However, W. B. Franklin, who was president the year I was vice president, had a heart attack and therefore withdrew from all activity through half of his year. Effectively I served as president for one and one-half years. It was just unfortunate timing for Bill.

MARCHESE: Could you express the rationale for the formation of the Blue Ribbon Committee to facilitate university-industry cooperation?

CHURCHILL: Yes. Most of the things one does as president only show up after you're gone. It's a deliberative body that's like that of CHOC [Center for the History of Chemistry]. The executive committee only meets six times a year, and the Council itself four times. So, controversial proposals drag on forever. That was very frustrating for me. What you wanted to get done as president would at best be done two years later. That committee you mentioned brought in its report long after I was gone. We were concerned about what we perceived as a gap between industry and academia, largely because of increasing government support for research. Industry thought universities were not interested in them. We were. So that was an attempt to try to breach a gap. I doubt that it had any great impact.

MARCHESE: Did it increase the funding that industry provided the universities for their vast research?

CHURCHILL: Not noticeably. It may have had some impact on hiring young people as consultants. I know that at least two or three companies took that recommendation to heart, because two of my faculty were chosen as examples even though the companies did not believe that those young people were capable of contributing to them technically. They realized that indoctrination in terms of their problems would influence future students. That may have been the major impact of that report.

STURCHIO: You were actually very progressive in trying to improve university-industry relations because in the last few years, starting around 1980, that was a major issue in science and technology and policy. Back in 1965-1967 that was the heyday of federal support for research, and I can imagine that industries would not have been very sympathetic because so much of it was coming in from the government.

CHURCHILL: As you mentioned, I was then writing a paper on training students for the aeronautical industry (31). So that period probably represented the nadir of the relationship between the chemical industry and chemical engineering education.

[END OF TAPE 3, SIDE 1]

MARCHESE: With regard to the Government Relations Committee of the AIChE, would you elaborate upon its inception, its program and its effects?

CHURCHILL: I do not wish to take any credit for that at all. I opposed and still oppose that program. It may nevertheless have occurred during my regime. I can remember other things I opposed and was wrong about. When the AIChE started its continuing education program, I was very skeptical. I thought universities could do this better and were doing an adequate job. After the first year, when the AIChE accomplished almost nothing and attracted nobody, I said, "I told you so." They decided to continue its support. About that time the program took off and it has proven to be one of the great accomplishments of the AIChE. I can now see that this was a very important thing to do, but I didn't believe in it at the time.

STURCHIO: What was the background of the Government Relations Committee? Was it to try to inform some people in the councils of AIChE that chemical engineering should get more of its fair share of the federal dollar? Or was it concerned with regulatory matters?

CHURCHILL: I think they had mixed objectives. There was a feeling that there was no input from chemical engineering to Congress and to the government, and that the National Society of Professional Engineers and others who were licensed to, in a sense, lobby for engineering were not representative of our views. F. J. van Antwerpen should get full credit for that program.

STURCHIO: Another motif in your presidential address, and in some of the other occasional lectures that you published around this time, was this sense that scientists and engineers were getting no credit for their accomplishments and were taking all of the blame for societal problems such as pollution that seemed to the public to emerge from science and technology. Did the fact that the AIChE should be in the position of lobbying with Congress come out of a dissatisfaction with their public image?

CHURCHILL: I'm not so sure how closely it was related. There was certainly a bad image for engineering in general and perhaps for the smokestack industries in particular during that period. There was also the impact of the Vietnam War. The image of engineers was very poor. We all felt this was an unfair knock on engineering. I remember we once wrote, "If there is pollution, the engineers are the ones who are going to control or alleviate it."

MARCHESE: Could you elaborate a bit on your attempts to broaden the AIChE's relations with groups of chemical engineers overseas?

CHURCHILL: AIChE has always had ambivalent views about international relationships and meetings. We deliberately decided not to have overseas sections. We made a decision to encourage the formation of chemical engineering societies abroad rather than trying to take them under our wing, which I think the Institution of Chemical Engineers (London) has done. When the Mexicans became very interested in chemical engineering we encouraged them and went to Mexico almost every year to help them start their own chemical engineering society. We had early meetings with the Puerto Rican Institute of Chemical Engineers and the VDI [Verein Deutscher Ingenieure] and others. When I was president, I visited Japan in order to arrange what became the first PACHEC [Pacific Confederation of Chemical Engineering] meeting. We wanted to foster these other groups, but we also thought that the right way was to help them become independent, not some part of us. There certainly were people who thought there ought instead be a world organization, but we foresaw that most of the problems had a nationalistic element and could not be solved by an international society. For example, in all of Latin America, engineering education is very weak primarily because the academic people are not paid. They are almost all volunteers. This is true in Mexico and in most of South America. We wrote several position papers asserting that professors of chemical engineering should be paid.

STURCHIO: The academic system was dependent upon adjuncts who made their living through industrial work or consulting?

CHURCHILL: Yes. I don't know that we had any impact on that, but during the period when I was on the AIChE council and an officer, I believe we were effective in encouraging professional activities in Latin America. I believe I went to Mexico every year while I was an officer of AIChE. I should have gone to countless other meetings for which I didn't have time. I did go to Germany, England and Japan to visit with their councils. We had a joint meeting with the English in London during that period, with the Puerto Ricans in San Juan and eventually one jointly with the English and Canadians in Montreal.

MARCHESE: We've spoken previously about some of the particular activities that you performed at Michigan, some of your accomplishments. Would you be able to begin to wrap up your accomplishments through the mid-1960s before you moved here to Penn? Could you perhaps tell us what were your major achievements at the University of Michigan?

CHURCHILL: I think research-wise our development of numerical methods of solving problems in thermal radiation and natural convection was important. We began the work on combustion which I have continued for twenty years. Also, I am proud that about ten of my students at that time went into teaching.

MARCHESE: You published three books in two years. In 1957 you published <u>Legendre Polynomials</u> with Clark and <u>Angular Distribution of Coefficients</u> with Clark and Chu. Then, in 1958, you published a third book entitled <u>Light Scattering Functions: Relative Indices of Less Than Unity and Infinity</u> with Boll, Gumprecht and Clark (33). What motivated this tremendous outburst of writing?

CHURCHILL: These, which all resulted from this work we were doing on the attenuation of thermal radiation from nuclear weapons, represent an outburst of computing rather than writing. They represent the distillation of a theoretical development in which doubly infinite series of uncomputed functions were reduced to a very simple form which is still widely used today--a simple convergent series of Legendre polynomials. Those books are tables of the computed functions that resulted. Today, we would probably just publish the computer programs, but everybody may not want to recompute all that. Xerox has reprinted these books, and they still sell a few copies a year.

MARCHESE: The Dow Chemical Corporation is in Michigan. What effect did this corporation have upon the orientation of chemical engineering research at Michigan and upon your research?

CHURCHILL: The answer is originally very little. Dow was at first more oriented toward Case Institute of Technology (now Case Western University) than the University of Michigan. All of their early officers went to school there. But, starting in 1952, we taught a regular graduate program in chemical engineering at Midland. It was a University of Michigan program, not a Dow program, but with a few exceptions, all of the students in it came from Dow or Dow Corning. As a consequence, we developed a good relationship with many of the young, upwardly mobile people at Dow. I taught the first class in 1952 and the program is still going strong. This may have been a factor in the gift which led to the Dow Building. Dow had the problem at that time, which they still have, of attracting people to Midland. They countered this by saying, "Well, Midland is pretty isolated, but we will bring the University of Michigan graduate program here." Quite a few students went on for a Ph.D.

STURCHIO: How many students were involved?

CHURCHILL: I have a picture of my first class here. There were twenty or thirty students in most of the classes. We taught one class or more a semester.

STURCHIO: It's similar to what Columbia did with Western Electric in the early 1920s. They would teach people electronics. Were the students in these courses in Midland entry level engineers, or were they research scientists? Where did they come from?

CHURCHILL: They came from throughout the company. Eventually they were mostly just out of school. However, when the program started, we also attracted more mature employees who were supervisors. So, the first classes were mixed. We always had someone from Dow to help teach the course. He became an adjunct professor at Michigan. This interaction filtered a great deal of Michigan into Dow and vice versa.

STURCHIO: Did that lead to much formal consulting work between Dow and your department, or was it mainly pedagogical?

CHURCHILL: It increased the amount of consulting but I do not recall that they directly supported much research at Michigan. Don Katz, Bob White, and I consulted for them, as did many other people at that time.

STURCHIO: Earlier you said that you had reflected on differences between Shell and 3M, which latter company you had consulted for while you were at Michigan. After you got to Michigan and joined the faculty formally, how did you go about setting up your consulting relationships? I know now that one of the biggest questions junior engineering faculty often face is how they will find some consulting work. It does seem to be necessary for beginning engineering faculty to do consulting.

CHURCHILL: Generally, these arrangements with young people arise because somebody comes to the department chairman and asks, "Do you have anybody that might work on this problem?" The chairman then suggests somebody. Most of my early consulting work arose that way. The early consulting work that I did was promoted by Don Katz, Bob White, or G. G. Brown. Either I helped them or they suggested me because they had more such work than they could do. The 3M work came to me directly.

STURCHIO: Which companies did you start with early in the 1950s and develop lasting relationships with?

CHURCHILL: I remember doing work for Mine Safety Appliances in liquid heat transfer that came out of the nuclear submarine work that I mentioned before. I consulted for Washington Water Power Company for a year or two. That resulted from a paper that I had written. I consulted for many years for Owens-Corning Fiberglas. This came about because of their interest in some research I was doing on radiative transfer through fibrous materials. Maybe the longest affiliation of all was for what was originally Constock Corporation and eventually became Conch Liquid Methane. This came about because Sliepcevich, who was at Oklahoma, had suggested my name to Billy Wood Prince, the head of the Chicago stockyards. They were interested in commercializing liquefied natural gas. I probably worked on some aspect of that for ten or fifteen years.

STURCHIO: You mentioned earlier that you often found new research problems that you could work on back at Michigan while you were consulting for various companies.

CHURCHILL: Some of our early numerical work and some of the work on natural convection came out of that liquefied natural gas work. I became interested in regenerative heat transfer because of the Washington Water Power Company work. The combustion work I do to this day is partially an outflow of some work I did for 3M on combustion in ceramic shapes. Sometimes people come to you because you have expertise. Sometimes you develop the specialized expertise in your work for them. I did work for Chrysler Corporation for a number of years on missiles because of their interest in our work on ignition of propellants.

STURCHIO: These are a very diverse group of corporate contacts, and you've mentioned that it was also a diverse group of problem areas to work on. Would a typical negotiation with a company for consulting work be just because of your general expertise or do they usually come with a specific problem for you to solve?

CHURCHILL: Except for 3M, companies always came for a specific problem. The 3M work started with a vague objective. They had a problem of disenchantment among their chemical engineers. Although I didn't realize it for a while, they wanted me to somehow provide motivation for their chemical engineers, to tell them that they were doing something important. Although 3M is a large chemical manufacturing company, they don't sell any chemicals except as components of products such as Scotchgard. The process of making the fluorocarbons has to be done by the chemical engineers, but they cannot be found in the profit and loss sheets. The manufacture of the chemical may add only a few cents to the cost of a product that 3M sells for five dollars a pound. The engineers only receive attention if the product fails. I did succeed in convincing the 3M company that chemical engineers could do research, rather than just being hands to run reactors and heat exchangers.

MARCHESE: In your career as a consultant, what have been your most notable contributions to industry?

CHURCHILL: Generally, the impact of coming from outside, opening the eyes of the people who work there, and showing them that they can approach their problems in different ways. Often industry does not realize that a body of knowledge exists--particularly, a theoretical method of solving a problem--that they can use. Therefore, they struggle and actually have blinders on. Somebody from outside can inspire them to try different approaches.

MARCHESE: You mentioned that your industrial consulting work has enriched your research. It has brought forth problems to work on. In what way has it affected your teaching or your research in a negative fashion?

CHURCHILL: I don't think that it has affected my teaching or research in a negative fashion except perhaps that I may be away a day now and then and therefore not available to students. I've always been cautious about this. The time I was away from school in professional society activities exceeded by a factor of five or more the time I was ever away consulting. Furthermore, I have always tried to incorporate students, both undergraduate and graduate, in consulting activities.

STURCHIO: From what you said earlier it sounds as though some of your consulting work was a fertile ground for thesis problems and related activities. We know a little more about the history of consulting activities of chemists in industry. Industry always speaks about consulting as a good way to insure a flow of trained graduates. Alternatively, the academic chemists feel that it's a good way to find out where the cutting edge of inquiry resides. Could you say a little bit about the connections between the impact of that kind that your consulting work may have had at Michigan and later on at Penn?

CHURCHILL: I believe that consulting has been a big factor, not just in my career but in the profession in bridging this gap between industry and academe. Generally, chemical engineers have consulted for their engineering counterparts instead of for management. This has produced a great, direct relationship, and has been a mechanism by which a lot of results from education have permeated the companies. Du Pont or Exxon have almost infinite resources, but they still have some myopia. Their own people are often so boxed in that they are unaware of what others are doing. Also, they are often handicapped by confinement to short-range objectives. One of the things I often observe in companies is that they have someone in another division of their own company that can solve their problem in ten minutes, and they don't know he exists. This was particularly true at 3M because they have such secrecy that nobody has much idea what they are doing in the next building. I often talked to two sets of people the same day and realized one set had the expertise needed by the other.

MARCHESE: While we're speaking about Michigan, you apparently helped to arrange with Du Pont and Hercules for a special program to provide industrial experience for young faculty members. Could you elaborate on how it began and its purpose?

CHURCHILL: This was an outcome, in part, of the AIChE discussion of the gap between industry and academe. I convinced some of these companies that they should hire young faculty members and that the long-term impact on both would be very great. Industry would complain that "Nobody is doing any polymer work," or "Nobody is doing this or that kind of research." I said, "Well, the easiest thing is bring one of your faculty members in and interest him in doing research at that field." I remember Joe Goddard worked on that basis for a number of years at Hercules. He is currently the head of chemical engineering with the University of Southern California. Dick Balzhiser, who is Vice President of EPRI [Electric Power Research Institute], went on such a program at Du Pont. I used them as examples for the AIChE Blue-Ribbon Committee.

STURCHIO: Were these connections with Central Research of Du Pont and Hercules in Wilmington?

CHURCHILL: Yes. The arrangement at Hercules came about because they invited me to a faculty visitation and complained about our lack of interest in polymers. I told them the fault was theirs and how they could do something about it. Fortunately, Bob Cairns (who just died) was there. He was high up enough to say, “Well, send us somebody next week, and we will change this." One of the problems is to deal with somebody who has the motivation and power to change things in a company. In the university, you can speak to an assistant professor and if he doesn't mind, you can carry his message to the Dean, but the young people in industry hate to use up their good will by asking the boss for some privilege because the boss then wonders, why are they concerning themselves about this peripheral matter?

STURCHIO: So Cairns was the one at Hercules. Was there a counterpart at Du Pont?

CHURCHILL: Offhand I don't remember. Perhaps Al Mueller.

[END OF TAPE 4, SIDE 1]

MARCHESE: When I asked you about your achievements at the University of Michigan, you were perhaps overly modest. I noticed that you wrote seventy-eight technical publications, supervised twenty-seven Ph.D. theses, and served on twenty-seven committees, as well as being heavily involved in the AIChE and arranging with Du Pont and Hercules for this program. You apparently had a great interest in international chemical engineering, and visited quite a few countries. I just want this information to be part of the record.

STURCHIO: After your term as AIChE president, you went back to Michigan in 1967. This is just about the time that you moved to Penn. Could say a few things about your decision to leave Michigan and come to Penn?

CHURCHILL: I was not unhappy at Michigan. I was completing the first term as chairman and I decided that I really preferred not to do administrative work. I was tossing around invitations to be considered for dean at Minnesota and several other places. One day while flying back from such an interview I thought, "That's not really what I want to do. I want to teach and do research." Although I had already turned down the Patterson chair at Penn, they asked me again through a very fortuitous circumstance. In the interim I had decided, if I really am to get back wholly to teaching and research, I'm going to have to get away from Michigan. I do not believe I can really disengage myself from all the committees and administrative responsibilities such as the vice-chairmanship of the University Senate.

I came to Philadelphia to give the seventy-fifth anniversary lectures at Drexel in February of 1967. There was an incredible snowstorm and Drexel closed for the day. The Penn people knew I was in town and Art Humphrey called me at the Sheraton Hotel and said, "Why don't you come and spend the day at Penn?" Ultimately that led to my coming here. I have always been glad that I made this move. I wrote as a condition of coming that I would not serve on committees or accept administrative responsibilities. I have not been able to maintain that position, but at least it was of some help in disengaging.

MARCHESE: You mentioned that you came here in order to focus upon research and teaching. What was the nature of your research in your early years at Penn? You had worked previously on convection, combustion, rate processes in general, and mathematical applications. Was there continuity or discontinuity?

CHURCHILL: There was a great discontinuity. I had just undertaken a large effort on plasma chemistry in which we invested half a million dollars, and I couldn't take that equipment with me. Also, I did not want to go through the effort of rebuilding the laboratory in several other areas in which I was working. The only continuity that I carried, I guess, was the numerical work that didn't involve anything except one's head and a few computer programs, and the combustion work which I started over again here. I was fortunate that the first year four fine graduate students started doing their work with me. Then for a few years I was starved for good students. If you look at my career at Penn, I've had far less graduate students here. During the late 1960s and early 1970s, there just weren't that many Ph.D. students anywhere because of the Vietnam War. I would have preferred to have as many as I had at Michigan but that was not possible here. But that first array of graduate students made it all worthwhile.

STURCHIO: What was the size of your group at Michigan when you left?

CHURCHILL: The year before I probably had about eight graduate students, despite my administrative duties.

STURCHIO: In restarting your combustion work, what kind of sources of financial support did you find? Did Penn provide that as part of the package?

CHURCHILL: Penn provided some help, but not very much for equipment. We just did the experimental work with bailing wire. I did not ask for more support for hardware. Perhaps if I had asked for more, they would have given me more. The timing was fortunate. Penn had that Ford Foundation Grant to make this a center of excellence in graduate work. In chemical engineering, we allotted all of those funds to supporting students and attracting good students. Joseph Chen, Hiroyuki Ozoe, Romeo Manlapaz, Jai Gupta, and Eddy Hazbun started with me. They made the transition very pleasant.

MARCHESE: For the record, this Ford Foundation Grant that you mentioned was awarded to the University of Pennsylvania in the amount of $3 million for the expansion of its full-time graduate program in chemical engineering leading to the Ph.D. degree. An effort was also undertaken at this time, around 1963, to recruit top-flight faculty to the department.

CHURCHILL: I believe that statement should be corrected. I suspect that the $3 million was for all of engineering. The other departments generally used the grant for equipment. It was Art Humphrey's decision to recruit faculty and students, and I think that was very wise.

STURCHIO: In what way was Penn different from Michigan? For example, it must have been a much smaller department than Michigan.

CHURCHILL: It was different in many ways. The comparison is difficult because my move coincided with the Vietnam problem--the disenchantment of students and all of the associated distractions. Our students were subject to the draft, and no able-bodied U.S. student had any assurance that he could stay long enough to complete his graduate work. At first I thought that was a local problem, but eventually I learned that the same thing was happening at Michigan. Although I was not too pleased with some aspects of my life at Penn, in retrospect these problems had very little to do with Penn itself. Although we had less undergraduates at Penn, our classes were the same size as at Michigan because there we had divided the students up into small sections.

STURCHIO: Michigan stopped having those enormous lectures that you spoke about when you were a student?

CHURCHILL: Yes. That occurred just in 1947-48, with the return of the veterans. Even in graduate classes, we broke up into two and three sections. I found a lot of things difficult at Penn. We had much poorer machine shop facilities, and consequently it was very hard to get equipment built. Michigan had a great supportive staff because it was larger. I had to do many more things myself here.

I did find one thing pleasantly surprising. There was less administrative superstructure at that time at Penn. I was very impressed. There was the president, the vice president for engineering, the department chairmen and yourself. I had no hesitation in calling Gaylord Harnwell or David Goddard on the phone. They would always answer and talk with you. Unfortunately, the reverse has happened in the meantime. When Martin Myerson became president he added echelons and echelons of administration. They have not increased under Sheldon Hackney but neither have they greatly decreased.

MARCHESE: Before we get too far from the discussion of your research, you mentioned that one of the discontinuities was to reinitiate research on combustion. Could you tell us about when you began doing that here at Penn and why?

CHURCHILL: I began working on thermally stabilized combustion at Michigan. One of my later doctoral students at Michigan, Tom Bath, tried to study a process I thought might be feasible, namely burning gas in a refractory tube without a flameholder. He succeeded superficially but could never maintain the combustion without the tube breaking. In the interim, when consulting for Marathon Oil, I discovered that they had some furnace elements which might serve that function. I succeeded in getting them to give us a box of these furnace elements. That got Joseph Chen's combustion work off and running. We got started on that work pretty soon after I arrived.

MARCHESE: Were the Chen articles that you're speaking of published in 1972 (34)?

CHURCHILL: Yes. Those are among the most important articles of my career, and I am still continuing that work. One was theoretical and the other experimental. Joseph Chen started in 1967, quite shortly after I came. Then Jai Gupta came along and worked on problems which grew out of my consulting work for Conch. We were designing underground storage tanks. We had no idea at all of the rates of heat transfer through the ground. Jai agreed to make such measurements. We found some remarkable behavior that we didn't expect concerning the migration of water. That work was not continued because we never succeeded in gaining outside support.

MARCHESE: Could you tell us about these people that you worked with? You mentioned Chen and Gupta. I noticed you published extensively with Hiroyuki Ozoe, [Hayatoshi] Sayama, [Kazumitsu] Yamamoto, Paul Chao, and Naom Lior. Tell us about these people, your interactions with them, what research you worked on with them, their effect upon your work, and your effect upon their work.

CHURCHILL: I've done more work with Hiroyuki Ozoe than with any other student or colleague. He did his doctoral work with me on natural convection, following up work that had been started at Michigan. He did both experimental and theoretical work. We continued to do this work jointly when he went back to Japan to teach. He was in a difficult position in a Japanese university, locked into a system where he still to this day has not been allowed to have doctoral students. So, he and I continued to interact. I do some things here and he does others there. He has better access to computers than I have, so often he's done the computational work there and we've done the experimental work here--although that has been reversed on occasion. I spent four months in Japan working with him in 1977. In 1979, and again in 1984, he spent extended periods here. It's been a remarkable collaboration. We write to each other almost every week, three and four letters a month. Whenever I have a thought about something, I dash off a three-sentence letter and put it in the mail, and he does the same. That way, it's almost as if he were here.

MARCHESE: This has been a remarkably productive relationship. And just for the record, you and he have coauthored thirty-three articles, sixteen alone in the last five years.

CHURCHILL: It has been synergetic and productive. Paul Chao is a much later student. He continued and improved upon some of the things that Hiroyuki Ozoe was doing. In a sense, he was also drawn into that network. He performed remarkably in that he did theoretical work, experimental work, and also stood the computer science department on their head by developing a method of displaying all of these computations live on a cathode ray tube. We still have some interaction. He has been working for Mobil for the last five years. Paul Chao's father, who is or was head of the cement business in Taiwan, gave me a hard time because I did not persuade Paul to enter academic work. Paul's wife works for the university. She has a very good job, and they want to stay in Philadelphia. That has kept him from teaching. Jai Gupta, one of my earlier students, went back to India and has taught there. He too has visited regularly at Penn for summers and we have continued to interact. He has just published what I believe is going to be a very important book on heat transfer.

STURCHIO: In looking over the list of your students since you've been at Penn, the ones we've discussed are Japanese, Taiwanese, and Indian. Is this international flow of students characteristic of chemical engineering over the last twenty years?

CHURCHILL: There are two sides to that question. During the period of 1967-72 or so, it was difficult for U.S. students to be deferred and therefore to go to graduate school. Almost all of the students at that time were foreign. Joseph Chen was from Taiwan. Ozoe was from Japan, Manlapaz came from the Philippines and Choi from Korea. Two exceptions were Mel Bernstein and Aaron Weiner. Both had physical disabilities that prevented military service. About five years ago, more than half of the graduate Ph.D. students in chemical engineering in the United States were from outside the U.S. This is because graduate work in chemical engineering has not been particularly attractive for American students. There were few worthwhile jobs in teaching, and industry didn't want Ph.D.s. At the University of Pennsylvania we have since succeeded in moving counter to that. Penn now has almost no foreign students in chemical engineering. Our graduate student body must be ninety percent American. We have three students from University College, Dublin, one from France, and several from South America. We have almost no Oriental students. You could fill any graduate school with excellent Taiwanese and Indian students, because there's a large crop that see no opportunities in their homelands and would like to come to the United States.

STURCHIO: It's interesting. This trend is like that in the late nineteenth century when American students went abroad for training in science and engineering.

CHURCHILL: I have students who are teaching in many countries. I mentioned Jai Gupta and Hiroyuki Ozoe. Romeo Manlapaz went back to the University of the Philippines and disappeared, apparently because of the political upheaval. I don't know to this day what has happened to him.

STURCHIO: Let me ask you again about the differences between Penn and Michigan. You mentioned the difference in administrative structure and administrative responsibilities, something about the size of the department and the graduate student body. How about computer capabilities?

CHURCHILL: Michigan probably then had one of the two or three best computer facilities in the country and Penn one of the worst among the top hundred schools. That has remained so. My work has continued in this field despite the poor computer facilities and access to them at Penn. It's hard to say why this has happened, but it has.

STURCHIO: In what ways did that frustrate your work in the late 1960s and early 1970s?

CHURCHILL: Well, it was frustrating because we had to do things in more difficult ways than other people. We were using poorer (by a generation) computers than other people, but it has had some good effects too. The pressure to do just as well with poorer facilities has perhaps inspired us to devise better methods.

STURCHIO: Did it need more mathematically elegant solutions?

CHURCHILL: No, I don't think so. We just have to be more clever; infinite computer facilities make you very careless. One other difference does not reflect favorably upon Penn. Michigan was one large community of scholars. I knew almost everyone in sociology, history, math, physics, chemistry and, of course, engineering. When I came to Penn I went into a state of shock. Nobody in chemical engineering talked to anybody outside of chemical engineering. Penn has changed for the better during the past two decades although it is still a very structured and noncollegial place, in my view. This is so partly because people don't live close to one another. Also, the Faculty Club is not a place to mix, whereas it was at Michigan. The faculty sat down together regardless of department and we got to know one another. This was very beneficial research-wise. I would know what the chemists, physicists and mathematicians were doing. When I had problems it was easy to call someone.

STURCHIO: This showed up in your writing while you were at Michigan. You must be one of the few chemical engineers ever to quote Aristotle, Mark Twain, Francis Bacon, and a number of others.

MARCHESE: Let me return to your research. One thing that impresses me after reading the papers that you've written is your remarkable ability to do something experimentally and then to come right back in the same article or an associated article and propose some sort of theoretical model or solution. You have this ability to wed, as it were, experimentation and theory. I think this comes out very clearly in those 1972 papers that we mentioned, on which you collaborated with Chen. You also indicated that among the more important of your works were the articles on convection that you published in 1983 (35). I again see there this wedding of the experimental and theoretical. Would you care to comment a bit about that and also about the importance of these 1983 articles?

CHURCHILL: I have two different comments. First, I think it's because I've always asserted that a theorist should run a critical experiment to make sure he has the right model. My students have always known that they would be required to do both theory and experiment. With perhaps one or two exceptions, every student has done so. A chemist once told me it came as a great shock to him to see this in my papers because no chemist would ever devote a paper both to theory and experiments. My observation is that no one else will do the critical experiments to test your theory or vice versa.

Generally, a synergism results from doing both. Joseph Chen's work is remarkable in this respect. He had done experimental work first and then succeeded in developing a computer model. Because he had no experience in computing and because we had such poor facilities, I didn't think anything was going to come of the latter. All of a sudden, he burst out with a great computational result which predicted seven stationary states in combustion. This came right out of the blue. He had not found these multiple states experimentally, and we didn't believe in their existence until we had subsequently looked for and found them. His work demonstrates the complementary nature of experiment and theory. The next student, Mel Bernstein, confirmed the existence of all of those solutions experimentally. That is somewhat characteristic of my research. Ozoe and Chu and Choi and all of the students who have continued to work on combustion and natural convection have followed this path.

This approach may, however, not be appropriate in all fields, such as thermodynamics. In the transport area, you never know whether or not the model is right. We use a two-dimensional model; is the behavior really two-dimensional? We have become very clever in formulating critical experiments. That's how I characterize my work to prospective research students. We try to model some phenomenon and then we try to think of a critical experiment that will test the model. That concept relates to another set of papers beginning in 1972 (37). I was trying to correlate data and I realized that often one knew the asymptotic behavior, and that such information ought to be incorporated in the correlation. As a general rule, up to that time, people tackled correlation independent of any theory. The generalized procedure we developed works almost infallibly. If you can find two asymptotes you have no trouble correlating data very precisely. Of course, that leads you to devise and think about asymptotic behavior and asymptotic solutions. I may have written more papers on that subject than on any other.

MARCHESE: You wrote with R. Usagi (37). Who was he?

CHURCHILL: No comment.

I have done quite a bit of work correlating not only my own data but that of others as well. I agreed to write one section of the Heat Exchanger Design Handbook and they were so impressed with the generality of the correlations that they asked me to do three more sections (35). In this instance I used that model to reassemble all the data for natural and mixed convection.

STURCHIO: That routine in a way repeats with your Rate Data in 1974 (28), in that it's the same kind of approach to be seen from the late 1950s on in your research.

CHURCHILL: Yes, it fits in very well. Actually, I originally left that method of correlation out of the book but J. D. Seader, who reviewed the manuscript for the publication asked, "Why did you leave that out?" I said, "I haven't digested it yet." He said, "I'm going to tell McGraw-Hill not to publish the book unless you put it in." So, in a very early version it's in that book. I was in Houston Saturday and Sunday giving a two-day course telling people in industry how to use this technique for correlation.

STURCHIO: So, the 1974 book and this technique presumably have had some impact in the last ten or twelve years.

CHURCHILL: Oh, yes. In the long run that technique of correlation may have more impact than anything I've done. It may not be very profound, but it is so incredibly useful. It also happens to fit very well with computers because no one wants graphical correlations or tabulations. It yields a simple equation you can use to summarize and reproduce the data and is therefore of direct use in design. This methodology relates to an earlier paper which may be of equal importance. That is the paper with Hellums on mathematical simplification of boundary value problems (38). That technique is, as far as I know, the first one which reduces all partial differential equations to ordinary ones if it is possible. Why this was overlooked by mathematicians, I have no idea. [Garrett] Birkhoff at Harvard did some related work, but did not include the boundary and initial conditions or reduce the method to practice.

STURCHIO: I suppose it's because the mathematicians didn't have to worry about the practical applications.

CHURCHILL: Mathematicians are no longer interested in "analysis." It has bothered me all my career when people say, "It's obvious that," or "If you...." It bugged me when people would say, "By letting $z = y^2$ over x, you can reduce this expression to an ordinary differential equation." I kept asking myself, "Why?" I finally asked that question the right way to David Hellums. We went through a very exciting week during which we both found an improvement every day. We said, "We'll leave the decision on the choice of variables to the last step. Then we ought to be able just to see what to do." The technique resulted from that approach.

[END OF TAPE 3, SIDE 2]

CHURCHILL: We have done a little more work on reduction since then, but essentially the initial work was done so completely it's hard to improve on. I published a later article in the proceedings of an Australian conference which updates this technique a bit, but basically it's all in that initial paper. Everybody uses this technique now. I even teach it to sophomores. They may be the only sophomores in the world to know how to do this. I gave my junior class a test involving this technique today. They can produce things that others believe to be the result of black magic.

MARCHESE: You seem to have this tremendous ability to synthesize, to reach out and integrate. You have brought mathematics and chemical engineering closer together. You've worked on problems that you can integrate into computer-type problems. You extended your hand to people across the ocean as you were working at Michigan and AIChE. I see that as a motif of your work and of your education.

CHURCHILL: That may also be a weakness. One of my problems is that I don't put any boundary on what I do. Don Katz used to tell me that I was in great danger in my career because I didn't pick some nice single problem and become the world's expert on it. He said every time he turned around I was working on a new problem. I don't think I went that far, but I have looked at a broader range of problems in chemical engineering than most of my colleagues. I may be an amateur in all of these things, but sometimes a lack of sophistication helps one to see the basic structure and the relationships between chemical-reaction engineering, heat transfer, mass transfer, mathematical modeling and correlation.

STURCHIO: They may all be different problems, but you use the same powerful approach.

CHURCHILL: Somebody over there has the solution for something over here if you can only identify their commonality. Chemical engineering tends to be very compartmentalized. It's hard to know who has worked previously on the same problem because they may write in different terms. During the last few years, I have written and piled up at home, parts of twenty books, almost all done. I'm having a great struggle trying to get this activity wound up because the material keeps building up and I dare not stop reading. But, these books will be quite different from anything in print. Four on fluid mechanics have gone to the publisher and should come out in a single volume in 1988. That's one of my limitations. I try to read over too wide a scope. Therefore, certain things get away from me.

STURCHIO: Anyone working on twenty books certainly has his hands full!

CHURCHILL: I probably will slacken off on writing research articles during the next several years simply because I have been making a great effort to complete the set of books on fluid mechanics. It does take time. You can't do everything simultaneously. I have decided that at this stage of my career I should synthesize my work if it is ever going to reach other people. This can only be done in books. It's not possible in technical articles. I don't intend to stop doing research, but I may be less hurried in getting that research into print. I have a complete draft done for the book on the technique of correlations, but I'm dissatisfied with it. The only time I really have to write a book is in the summer. I do not have sufficient uninterrupted time during the academic year.

STURCHIO: How has chemical engineering changed over the past forty-five years?

CHURCHILL: It's changed incredibly, from an entirely empirical field with a little algebraic structure to one that has become very scientifically oriented with a great deal of theory. The latter is now dominant compared to experimentation. This was at first a gradual process but began to accelerate in the 1950s. Computers had a big impact on this transition--computers and the fact that the graduate students after the war took advanced mathematics. What really transformed the field, as you mentioned, were books such as Marshall and Pigford and Sherwood and Reed. One of my distant relatives, R. V. Churchill, wrote three books on modern operational mathematics, boundary value problems, and the theory of complex variables (39). Those came out at that time. For the first time there were books which engineers could use to study these subjects.

The balance has continually moved in the direction of theory and is still moving. However, the source of all new things is observation. Theoretical speculation doesn't mean anything until people go out and confirm it. I was critical for a while, although not so much anymore, of Neal Amundson and Gus [Rutherford] Aris and their counterparts. They never did any experimental work and in a sense, asked other people to test what they were doing for them. My observation is that nobody ever tests anybody else's theory--you only test your own. For a while, they were doing things so abstract that no one else would ever know if they were right or not. Other people have since tested their work to some extent. They proved to be doing more realistic work than I had originally thought.

STURCHIO: What effect has this tendency towards theory in the past thirty years or so had on chemical engineering design and actual applications in industry?

CHURCHILL: The design element fell right into the basement after the Grinter Report, but it has been revived by computer-aided design. The universities may now be farther along than industry. Warren Seider and Art Westerberg at Carnegie-Mellon and their peers are the leaders in this field now, not industry.

STURCHIO: Now they are designing elements in large-scale processes from first principles?

CHURCHILL: At least they're synthesizing. They are taking models of various kinds and putting them together. One could do that before only on a small scale. Computers have had a big impact on design. This work was started in part by the Ford Foundation Project that Donald L. Katz led at the University of Michigan. The objective of that study was to find out whether or not computers should be used in undergraduate education in engineering. Strangely enough, that was a controversial matter at the time. Katz discovered rather soon that if a faculty member learned how to use a computer he immediately became favorable. If he didn't, he always was opposed. It became quite clear that the primary problem in the way of obtaining a fair evaluation was to make sure that the faculty did not have a personal insecurity about computers. That is, that they themselves knew how to use them. I think that's true of many areas. We now suffer from a lack of people who are good experimentalists. In the 1960s, very few people did any experimental work. These were the people that went out to teach. Most of the young leaders in chemical engineering in the universities of this country are pure theorists. They, of course, will not teach experimental work or do it. There is a revival in some areas, such as the biomedical engineering area, where people know that they have to do experimental work. There are whole other areas where no one ever does experiments.

STURCHIO: Do you think that the impact of that is going to be felt in the future, or is that legacy going to decline?

CHURCHILL: In time that legacy will decline. Experimental work will increase again, but it's at a fairly low point now. It's not as extreme at Penn as at some places. Warren Seider does no experimental work, and I don't know whether or not Lyle Ungar will. Almost everybody else on our faculty is basically an experimentalist. That's unusual. Well, on second thought, [Alan] Myers is not basically an experimentalist, and Ed Glandt is not an experimentalist. So maybe a third of our faculty does little or no experimentation. Many universities have a majority of nonexperimentalists.

STURCHIO: We spoke earlier about the relative reputations of Michigan, MIT, Minnesota, and Delaware in the 1950s and early 1960s. How does that look now in the 1980s?

CHURCHILL: It's changed radically in the past ten years. One of the problems is that the recognition of improvement or decline lags five to ten years behind the real situation. You can go into an absolute funk and nobody will notice for five or ten years. I think MIT did not belong in the top ten departments in chemical engineering five years ago. They probably thought they were first, but no objective measure would have placed them even among the top ten. If they rated fifth or sixth in the polls, it was only by inertia. Michigan was once first or second and I'm sure fell out of the top ten during this same period. Minnesota and Illinois improved in real terms as did Delaware, Northwestern, Houston, and Penn. The most remarkable rise of all of these was by Penn. From 1960 to the present, Penn probably made the biggest upward step of any school in the United States. We came from nowhere. If you look, you would hardly find Penn rated in the 1950s and 1960s. They had a very small faculty with no impact. Now, we have reason to think we're one of the top two or three. A poll might not put us quite that high because of inertia. We were tied for tenth or eleventh in the last one about six or seven years ago.

STURCHIO: Is it possible to characterize the departments that have come up in the last ten to twenty years in a particular way-- the way that we've talked about, say, the impact of applied mathematics on Minnesota and Michigan and the other departments in the 1950s?

CHURCHILL: You have, somehow, to get the attention of other people, and particularly of the best graduate students. That's the weather vane that tells you where the best places are perceived to be. You could also look at publications, presentations at meetings, awards, etc. Art Humphrey probably was the major factor in this rise at Penn. He was prominent as a bioengineer when almost no one else was. For a long time the majority of students who came here came because of Art even though they may not have ended up working for him. Now we're unique as compared to almost any department in that we have a completely active faculty. All of our faculty perform at a similar level. We all do research, we all teach undergraduates and we all have an active research program with graduate students. We have no deadwood. Part of that is good planning, but part of it is pure luck. Most departments eventually decay because some people who look very good at forty quit producing at fifty. However, they seldom perform so badly that you can get rid of them. I would judge that Michigan has twenty-five percent of that type now. That's a big anchor around their neck which takes a while to get rid of. Even though you have good young people, their impact isn't felt right away. Delaware and Princeton have both had periods of terrible luck, but they soon surmounted it. Dick [Richard H.] Wilhelm, who was one of the top people in the country, and then Leo Lapidus both died very young. Delaware lost Pigford, [Jack A.] Gerster, [John R.] Ferron, and [James] Wei, but each time they immediately had respectable replacements. Princeton has been less responsive and hence has slumped in the last few years, but they will recover before the good students stop coming because they have a long tradition and a strong motivation.

MARCHESE: You have elucidated relations between various universities and academic centers, and how they have changed over the years. Would you also please contrast the relationship between academic and industrial chemical engineers with when you began to do your chemical engineering? Has this relationship changed since then, and if so how has it changed?

CHURCHILL: I think the relationship has always been either through the AIChE or through consulting, not through research. Such relationships ebb and flow with time. The relationship between industry and academe is not as strong as it was in the 1950s, because there were then less people and less schools. I was lucky to be at the center of such an active relationship. We had industrial fellowships from perhaps thirty companies at Michigan. No school has that many now. At Penn, we're lucky if we have five. Michigan does not have nearly as many now. Companies then focused their attention on a few schools who were producing a significant number of Ph.D.s. Now there are over a hundred graduate programs and attention tends to be more diffuse. Warren Seider has documented the decline of academic influence upon the AIChE. There are now few academic members of Council. I believe that this is serious, not that academic people are special, but because the AIChE Council is an important forum for the establishment of mutual goals and exchange of information. When leaders of the field in academe are acquainted with leaders in industry, a desirable synergism occurs.

STURCHIO: You spoke briefly last week about the social relationships among leading chemical engineers. How has that changed as the number of programs has increased?

CHURCHILL: That has not changed very much. This close relationship is very unique to chemical engineering. We all know each other and visit with each other. Thirty people come here from other universities every year to give lectures. We have someone every week. Almost all schools do that to some extent. This provides a great exchange. Most of us know each other on a first-name basis. The AIChE meeting, the annual one, is a grand gathering for a large fraction of the faculty in the country. I do not think the equivalent exists in any other field of engineering, except possibly in metallurgy. In chemistry the physical chemists know one another, or at least the thermodynamicists, but they don't know the other chemists. There are just too many.

STURCHIO: Do you think this is just a scale factor?

CHURCHILL: The scale factor helps but the situation did not change when the number of faculty members doubled. We all read the same journals. I don't read articles in all aspects of chemical engineering, but I still know something of the research of most other faculty members. I hope that this is maintained since it has a big impact on the character of chemical engineering. We all teach essentially the same thing to undergraduates as well.

STURCHIO: Does chemical engineering depend on a few central journals, and has that changed over the past?

CHURCHILL: There have been several real crises in that regard. With the onset of photocopying many of the journals crashed financially. For example, Industrial and Engineering Chemistry, which had 14,000 subscribers, almost vanished. Subscriptions to the AIChE Journal also crashed. As a consequence, for fiscal reasons, less and less pages were published. Commercial publishers filled the vacuum, and they have taken over a large role in publication. The review processes and the costs of subscriptions are a different matter. The commercial journals may or may not review as carefully. They sell these journals for $200 or more a year rather than for $20. Only libraries can afford to subscribe. It's an economic trend that's very unfortunate. A consequence of the high subscriptions is that the young people do not have personal libraries of journals. The argument that led to a recent decision to double the pages of the AIChE Journal is that otherwise we would not have a common literature.

MARCHESE: Tell us a bit about textbooks. How have they changed over the years? How effective are they now?

CHURCHILL: Textbooks have changed, but they generally lag behind the field. There's no direct profit in writing a textbook unless you chance to hit a very wide market such as Smith and Van Ness (40). Only a few textbooks ever pay back the author's time and, for that reason, they are seldom revised. There's a shortage of good textbooks. I seldom use one. I usually teach from notes. You are often acquainted with newer things than the textbooks. Perhaps that's only a characteristic of the research universities. Villanova would probably not teach a course without a corresponding textbook and a solutions manual. When you teach twelve hours a week, you don't have any time to make up new problems or put in new work. So, a two-tier pattern exists, and I may overemphasize the one tier. Minnesota, Wisconsin, Berkeley, MIT, and Penn are not really worried about textbooks.

MARCHESE: You received the Warren Award in 1976, which was given for the excellence of your teaching. Would you care to speak about your philosophy and style of teaching?

CHURCHILL: My style is somewhat unique. I know because of the feedback from graduate students. I'm not terribly well organized but on the other hand, I don't read or copy from books. I just talk to and with the students. Therefore, I go through the process of confronting a problem with them. They eventually come to grips with this interactive approach. Often I don't know exactly what I'm going to do when I enter the classroom, but I know the subject sufficiently well to be confident. Strange things sometimes happen. The students either like this or they don't. Some want somebody who comes in with everything organized and limited in scope. Soon they know that with me they're going to have to do some creative work on the test. It's hard even for me to say in advance how I'm going to test them. I learned this style in part from G. G. Brown and R. R. White because they taught the same way.

STURCHIO: During our last session you were talking about Brown, in particular, using the Socratic method. It sounded almost like a law school class.

CHURCHILL: Well, with Brown it was. I don't use quite the Socratic method. I cover more ground and ask less questions. By contrast, Joe Martin at Michigan, who was one of the greatest and most admired of teachers, was terribly precise. No comma was ever out of place. If you just listened, he laid out the field for you in perfect order. Warren Seider does the same thing. I enjoy looking at his blackboard after he gets done and contrast it to mine which tends to look rather disorderly. I wish I were a little more organized, but I guess my way works too. The undergraduate class I teach is rated as one of the toughest courses at Penn and is an elective, so none of the students have to take it, but they do.

STURCHIO: Has the trend towards a more theoretical approach in chemical engineering percolated down to the entry-level texts?

CHURCHILL: Yes, but I'm not sure to what extent they are widely used. There are several super-theoretical textbooks. There are textbooks in fluid mechanics that I can't read because they're too mathematical. Slattery's book is an example (41). People who use these books are either more mathematical than I am, or they do this for reasons of prestige. The students probably don't understand these books. There are good textbooks. When I last taught stoichiometry I used Rousseau and Felder's book (42), which I thought was excellent. I felt very comfortable with it. I just told the students to read it and I would provide embellishments.

MARCHESE: Has your style of teaching changed since you've come to Penn or are you doing the same things that you did at Michigan?

CHURCHILL: Not greatly. The content changes continuously but not my overall style.

MARCHESE: How about your interaction with your Ph.D. students? Do you deal with them in the same manner in which you dealt with them at Michigan?

CHURCHILL: Yes. After they've worked with me a year, they become part of my extended family. They become my life-long friends wherever they are in the world. When I went to Houston last week, I had dinner with three of them the two nights I was there. We keep in close touch. They sometimes call me, just to say hello. I did not intimidate any of these people, at least after they got to know me.

MARCHESE: You mentioned when you did your dissertation that Brier was your adviser and that while he supported you, you more or less worked on it on your own. How do you relate to your Ph.D. advisees while they are creating?

CHURCHILL: I probably lay a less heavy hand on them than most of my colleagues. I tell them to try to do this, ask them how they are getting along, and then make suggestions. A penalty of working for me is that you are expected to be creative and exert considerable initiative. Ultimately I expect them to do something other than I suggested. Very few students have done just what I've told them to. I push them in a direction, believing there's something there, and then they find something more important than what I thought would be there. Most of their theses reflect them as much, if not more, than they reflect me. I keep telling them, "It's your thesis, not mine." They do pick up, or at least are thoroughly exposed to my philosophies of research in the three or four years of our daily interaction.

[END OF TAPE 4, SIDE 2]

STURCHIO: How do you think things have changed for a young faculty member starting out now as opposed to 1952 when you got your first full-time job at Michigan?

CHURCHILL: Oh, it's drastically harder for the young faculty now. I was not immediately expected to go out and raise enough money to support all of my research. I had never really felt pressure for tenure. Maybe I just didn't know any better. I always assumed that I would do the right things and would be promoted. Indeed, I was promoted within three years so that issue never really came up. I was promoted to full professor in two more years and was thereby relieved of that pressure. I think the young faculty are now under too much pressure. They have to perform and produce almost instantly. We do all we can to help them. We assign them the best graduate students. We're supportive of them and don't ask them to do too many other things. Still, the pressure to publish and to acquire research funds is really very intense.

STURCHIO: Just to give us some order of magnitude difference, how much capital expense was involved in your first series of research problems at Michigan versus what a typical faculty member would have to come up with today to get started in research?

CHURCHILL: For most people today capital costs in the sense of equipment are not the problem. At Michigan most of the graduate students were supported by Dow or Du Pont or Exxon fellowships or the like. In a sense, all a faculty member had to do was scrounge a little money for equipment; the computer was free. Money was not a burden. Of course, we sought and obtained research grants to support students, but we did not feel that was a necessary thing. Here on the other hand, we have to find $25,000 or more a year just to support each student, plus money for equipment and the computer and supplies. It may take $70,000 a year from a research grant to support one student. That is the real burden--running on a treadmill to provide that money. In part this is because Penn is private and has over $10,000 a year tuition. At Michigan tuition was then of the order of $100. Today, Michigan's tuition is probably $2,000, so they are moving in our direction. All schools have this problem, more or less. The state universities have, until recently, been much better off financially than we have. But, financial worries are catching up with them too.

STURCHIO: It's interesting to reflect on how Michigan and other universities in the 1950s had a lot of industrial fellowships, which were the legacy of fifty years of interaction between academe and industry in science and technology. After World War II, when universities began to depend more and more upon federal support, it seems that the old ties between university and industry which led to these fellowships died away. Now, although we have new interest in industrial support of academic research, the kind of bread-and-butter support that you were just talking about, where graduate students had industrial fellowships, seems to have gone away.

CHURCHILL: That's the obvious conclusion, but a bigger factor may be that most of the industrial grants have always had the purpose of recruiting students. Fellowships go to the places that have the best students. Penn is one of the places which has the best students and therefore gets relatively more support, and chemical engineering more than any other department. Fellowships are in short supply for chemical engineering nationally because the chemical and petroleum industries, which have been very enlightened about this, are now in a tight-belt position. There's no question that our support, which had been going up for some time, has suddenly turned down. Fortunately, many of these companies have given us five-year grants. When these run out, it will be very tough to obtain renewals or replacements. Most of the companies take a very short-range view. As soon as they see a down-turn in income, they turn off their fellowships. The times that I'm speaking of at Michigan were the boom times for petroleum and particularly for petroleum production.

MARCHESE: Is this turndown in funds for fellowships a leading indicator? Does this occur before they actually experience difficulties?

CHURCHILL: Well, I once heard some stock analyst at an AIChE meeting saying that he indeed used that as an indicator. He also said that you could identify the upturns and downturns even more clearly by the hirings and expenditures of the equipment and design companies because long before an upturn in production, Du Pont goes to United Engineers and asks them to start designing a plant, and they have to hire people. That's the first key for an upturn in any phase of industry. Many chemical and petroleum companies are still looking casually for Ph.D.s, but they're not hiring many undergraduates.

MARCHESE: Speaking of Ph.D.s, you must by now have had almost forty Ph.D. students. Could you tell us what careers they have followed? What has been your role in helping them to find employment?

CHURCHILL: Well, I always try to help them find and choose a job, but this is less of a factor than for a chemist. The professor in chemistry actually goes out and finds the job. If one of my students wants to go to an industrial interview, I have a limited role other than writing recommendations or talking to people about how capable they are. In looking for a faculty position, students in engineering must also exert quite a bit of initiative. A faculty member may have more impact here than with industry by answering, when asked, how he rates the student. He may greatly influence their career by what he says. The market influences the choice between teaching and industrial research. When most of my students went into teaching, it was because teaching was a viable option financially and because there were many opportunities. Opportunities in teaching have been fewer in recent years and fewer of my students have gone in that direction. I have always encouraged them to think about teaching as a career, at least those that I thought were well suited. Some of them I did not feel confident of in that respect.

I would guess they're split about 50-50. Peter Abbrecht, who was my first student, went to work in industry, then came back to school for an M.D. He's now in biomedical research. He says he's a chemical engineer practicing in hospitals. He taught at the University of Michigan for a long time. Now, he's at the National Institutes of Health. Mort Moyle first went to work for Du Pont here at Gray's Ferry. Then he taught at Lehigh. He was killed tragically while very young. Bill Martini taught as an adjunct at the University of Washington but primarily worked in the nuclear industry just as he had before his graduate career. He's now working on Stirling engines as a consultant in that area. Herb Zellnik has often taught part-time in universities, but he initially worked for Scientific Design (Ralph Landau's company). Bert Larkin taught briefly at the University of Colorado but he has been working in the aerospace industry for many years. Marty Gluckstein has long been an adjunct professor at Wayne but works for Ethyl in Detroit. Don Sundstrom went to the University of Cincinnati, then to Allied Chemical, and has been at the University of Connecticut for a long time. Bill Luckow went into the nuclear industry and has stayed there. Roy Gealer has been at Ford Motors since he graduated. George Clark has been at Conoco since he graduated. I cannot tell you where Gene Stubbs is. Richard Ahlbeck works for Amoco. Irving Miller first went to United Aircraft and then taught at Brooklyn Poly. He's now Dean of the Graduate School at the University of Illinois, Chicago Circle, and still is working actively on biomedical research. Bill Zartman, one of the best students I ever had, has always worked for Exxon. Jim Leacock works for Scientific Design. John Chen worked for Brookhaven National

Laboratories for many years, then went to the Mechanical Engineering department at Lehigh and is now chairman of Chemical Engineering, and a Distinguished Professor there. David Hellums went directly to Rice. He was chairman and is now Dean. Tom Bath has always worked as a consultant in Washington, D.C. Larry Evans went to MIT. I remember talking to Larry and telling him not to go there because they never kept anybody but their own graduates. Ed Gilliland subsequently called me. He was angry and said, "Why did you tell Larry that?" And I said, "Well, it's true." He said, "No, I'm from Penn State." I said, "Name me somebody else." He said, "All right, but we'll give Larry a fair chance." Larry eventually became executive officer of the department and has remained there all of his career. Bob Rigg first worked for Standard Oil of Indiana but now is with Dow Chemical. Jim Wilkes went to Cambridge University to teach and then came back to Michigan. He has been chairman and still is on their staff. Carl Vinson went to 3M and then moved to Diamond Shamrock. Dudley Saville came from Cal Research to graduate school at Michigan, went to Shell Development, then to Princeton University where he has been teaching for fifteen years. Mike Samuels taught at Delaware and then moved over to Du Pont. He's still an adjunct professor, but is primarily with Du Pont. I spent many hours trying to persuade Warren Seider to teach. He had no interest whatsoever, and then Don Katz got him working on the Ford Foundation project. Warren was rated by the students as being the best teacher in the group and was hooked. So, he came to Penn.

STURCHIO: He was your last Ph.D. at Michigan?

CHURCHILL: Well, Dudley Saville and Mike Samuels and Warren Seider all finished in 1966 and 1967. I'm not too sure in what exact order.

STURCHIO: He was at Penn before you came?

CHURCHILL: We came at the same time, rather independently. He was very shocked. I had been on leave at Michigan, and therefore I owed them a year. I told Penn that I had to stay at Michigan for a year. "If you still want me, I'll come a year from now." President Harnwell said he would negotiate for me to come at once, but we agreed not to tell anybody at Michigan if I had to stay that year because I just did not want to be a lame duck, so to speak. Actually, Harnwell succeeded in negotiating that I come here at once. In the meantime, Warren had come here and somebody told him about my arrangement. He came back to Ann Arbor with a look of shock on his face. He had told Penn he would come, but then he wasn't sure he wanted to because people would think of him as my student rather than as himself. But that has not been a problem. We have remained friends, but we have never done research together.

Joseph Chen was my first student at Penn. First he worked with Hooker Chemical and then with Occidental Research. He later became involved in the real estate business in Orange County and dropped out of chemical engineering. Hiroyuki Ozoe went back to Japan and has always been at Okayama University. Romeo Manlapaz went back to Manila in 1971 and promptly disappeared. All of my letters have come back marked "address unknown." Other students have tried in vain to find him. The Ford Foundation said they would trace him, and then they told me to forget about it. They said, "He might be better off if you do not write." I have no idea what has happened. I met Jai Gupta at IIT Kanpur when I went there for a visit through the AID program, and persuaded him to come to Michigan. After his M.S. he went to work at UOP and when I moved to Penn he came here for his Ph.D. He has taught at IIT Kanpur ever since. Eddie Hazbun was working for Rohm and Haas after receiving a master's degree from our department. When I was consulting for them, a problem came up and he asked Rohm and Haas if he could do it for his Ph.D. thesis. That eventually worked out, but he moved to ARCO Chemical because Rohm and Haas refused to let him publish the experimental portion of his research work and is now their director of research. Humbert Chu was first with Exxon Research, and recently moved to Shell. I had dinner with him last week. Aaron Weiner died. He was working for Du Pont. Mel Bernstein is with Exxon. Shyy-Jong Lin was with United Engineers, but now is with Hydrocarbon Research. Byung Choi is with Mobil at Paulsboro. Paul Chou is also with Mobil at Paulsboro. Harry Tang is at Shell. Lisa Pfeferle, who finished last year, is teaching at Yale. Perhaps I missed someone, but those are most of the students.

STURCHIO: That's quite a group. The interesting thing from your running through the list like that is the movement between academe and industry.

CHURCHILL: A lot of them did. That has always been considered the right thing to do. In principle, everybody says that academic people should work in industry so that they know what's going on in practice. But the fact is that we can hire the best young people in the United States on our faculty when they finish their Ph.D.s, but we can't hire them at a later time when they obtain, as we say, golden handcuffs. Their salary goes up much more rapidly than at a university, they marry, buy a house, and if they are good, the company will do anything to keep them.

STURCHIO: Do you see a way around that?

CHURCHILL: My argument with industry has always been to hire new young teachers as part-time summer consultants so they can obtain experience and an industrial orientation. Warren Seider once felt a deficiency this way, particularly because he teaches design, but I don't think he does anymore. He knows he's ahead of industry, but it has been a long time to develop that assurance. He's been an active consultant. He worked three summers for Exxon. He has had a lot of interaction with industrial people.

STURCHIO: You were fortunate to have that five-year period in industry when you did.

CHURCHILL: That is more and more unusual. I have more experience than anybody else on the Penn staff. A period in industry prior to teaching doesn't happen very often anymore. The students ask, "You did this. Can't I also go to industry and then come back for my Ph.D.?" The answer is, "It's almost impossible financially. Young people do not mind continuing as students on a subsistence basis. But after they become used to a salary of $30,000 or $40,000 a year, they have a hard time leaving to live on $5,000 a year. They have established a comfortable lifestyle. They are used to having a car; they may be married and have children. Occasionally, it still happens. Three or four of my students did this. Dudley Saville, Bill Martini, Donald Sundstrom, Morton Moyle, and Irving Miller. Peter Abbrecht even went back for an M.D. ten years out of school, which is a real accomplishment. In so doing he stirred up the whole medical school. They didn't want him to finish because he already was doing superior research as a graduate student. No one has a solution to the problem of industrial experience. Industry always says they'll be glad to have your people with them on leave for a year, but young people cannot consider that alternative because they must obtain tenure in six years. The university is not going to make a sufficient exception for a year which will result in a big gap in research and publication. The leave may be beneficial, but it's not going to show up when a provost looks at their credentials. This remains an unsolved problem.

MARCHESE: You have gone through your Ph.D. advisees and brought us up to the present. Penn's chemical engineering department puts out a publication indicating current research interests. I'm just going to go through them and you tell me if this in fact is what you're working on now and if you have anything you may wish to add. They indicate combustion, natural convection, liquid migration, rate processes and correlation, and finally computerized analysis. Is this what you're doing now?

CHURCHILL: The natural convection work is currently in abeyance. My only connection with that is some work with Hiroyuki Ozoe in Japan. I have no student working in that field here because the government has cut off all support for the area of solar collectors and passive solar heating. I hope to resume that research, which I have worked on throughout my career, when the direction of the research wind shifts. The water migration work is in that same status. It's something that I have dabbled at for a long time and want to complete, but nobody's willing to provide financial support. All of my current students are working on combustion because I have three contracts in that area.

MARCHESE: Would you tell us which scientific organizations you belong to currently?

CHURCHILL: The American Institute of Chemical Engineers, the American Chemical Society, the Combustion Institute, and honorary societies like Sigma Xi and the National Academy of Chemical Engineering.

MARCHESE: Would you also list your major awards?

CHURCHILL: The Professional Progress Award, the Walker Award, the Warren K. Lewis Award, the Founders Award of the AIChE, and the Max Jakob Memorial Award in Heat Transfer of the ASME and AIChE.

STURCHIO: Would you like to say anything about your family?

CHURCHILL: I have four children. None of them are in engineering, although my son certainly has strong scientific proclivities. He's a consultant in San Francisco. He works in computer systems although he has had no formal training in that area. That's Stuart Lewis. My oldest daughter Diana is thirty-two, married, and has two children, but works full-time at home for Xerox doing nonscientific work. My third daughter, Cathy, who is in Berkeley, California, is an artist. She works in art restoration but she's a painter and sculptor at heart. She has been an artist since the day she was born; I have paintings she did when she was three. She has won many awards but has had a very difficult time trying to support herself with art. My youngest daughter is nineteen and is a freshman at Hope College in Michigan. I was divorced from my first wife in 1968 and married my second wife, Renate Treibmann, in 1974. She is the editor of International Chemical Engineering, and I met her through the AIChE.

When we married we thought about living at Princeton or thereabouts so she could commute to New York and I to Philadelphia. She had just become editor and it would have been a shame for her to give up that kind of job after she had worked for it for so long. But Mr. Van Antwerpen said, "Well, you can take your office to Philadelphia." So, since 1974 she has done that job in our home.

STURCHIO: It must be hectic having two chemical engineers in the family.

CHURCHILL: Well, we have many mutual friends and mutual interests. I help her and she helps me.

MARCHESE: I've been meaning to ask you something about yourself. You are known as "the leopard." Could you please tell us how this occurred?

CHURCHILL: This was just a nickname given to me by some Japanese friends. They gave me a medal with a Japanese symbol for a leopard. It is a fun thing, nothing very serious.

STURCHIO: That brings us close to the end. Is there anything that we should have asked you that you would like to talk about?

CHURCHILL: I wrote down a few things we have not discussed, but they are not important. As I said, I have been very lucky all of my career. I have always been paid for doing what I like to do best. When I was Vice President of the AIChE, I remember introducing Dr. Pike, an Episcopal bishop who was speaking at our San Francisco meeting. We were having cocktails before the luncheon and he was telling us what he was going to talk about, which was the impact of computers and the shorter work-week on society. I remember telling him that I don't think it would have much effect on chemical engineers because they work seventy hours a week and would keep on doing so. We had a little discussion on this and, remarkably, he completely changed his talk and gave a talk to that effect at the meeting. I was impressed at his ability to shift topics at the last moment. He recognized he might have had the wrong message for the wrong crowd.

I had also told him that chemical engineers had the lowest divorce rate of any group (which was true at that time; I don't know whether it still is). He said he knew why. He said most marriages he knew couldn't stand the husband being home four or more hours a week, and that obviously chemical engineers were so motivated by their work that the shorter workweek would not impact most of them. My wife sometimes jokes and sometimes may be serious about the fact that I would probably be happy working all the time. It may well be true. I do enjoy tennis, skiing and running, gardening, and other things but doing research or writing papers or books is a work of love too. I guess that is what all of us would like of a career, to be allowed to do the things that are self-satisfying and self-rewarding. This has been much more possible in an academic career. In industry, I enjoyed what I was doing and felt excitement and accomplishment, but I was doing somebody else's thing, not my own.

MARCHESE: And your work extends beyond yourself. It has an effect upon others.

CHURCHILL: It does, and as you see, my friends in the world are my former students and the students here now. That's an advantage to teaching, which is not true in any other field. You continually deal with young people and fresh ideas and are forced to be involved with change. I think that is very healthy and desirable.

I would like to make one additional comment prompted by reviewing the work of my doctoral students. They all worry whether someone will preempt their research while it is in progress and whether the results will prove important. In retrospect almost all of the experimental studies remain benchmarks to this day. Some of the numerical solutions have been superceded because of better computers and algorithms but even they generally have a recognized historical role. The few theoretical results which have stood the test of time are the most famous because they are most often referred to. I may not have always chosen the best topics for research but my students have none-the-less erected a structure of which I am very proud.

STURCHIO: That brings us to the end of a very informative series of reminiscences on your long and distinguished career. I would like to thank you very much for taking the time to discuss this with us and for getting it all on the record.

CHURCHILL: It was fun too, because I've reflected on things that I hadn't thought about for a long time.

[END OF TAPE 5, SIDE 1]

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