CHEMICAL HERITAGE FOUNDATION

CEDOMIR M. SLIEPCEVICH

Transcript of an Interview
Conducted by

James J. Bohning

at

University of Oklahoma

on

1 March 1993

(With Subsequent Corrections and Additions)

SLIEPCEVICH, Cedomir M.

THE CHEMICAL HERITAGE FOUNDATION
Oral History Program

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(Date) June 17, 1998

(Revised 17 March 1993)

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

CEDOMIR M. SLIEPCEVICH

1920	Born in Anaconda, Montana, on 4 October

Education

1941	B.S., chemical engineering, University of Michigan
1942	M.S., chemical engineering, University of Michigan
1948	Ph.D., chemical engineering, University of Michigan

Professional Experience

1942- present	Private Consultant
	University of Michigan
1942-1948	Associate, Research Institute
1942-1946	Teaching Assistant
1946-1948	Instructor
1948-1952	Assistant Professor of Chemical and Metallurgical Engineering
1948-1952	Chairman of the Graduate Standards Committee for Chemical and Metallurgical Engineering
1953-1955	Associate Professor of Chemical and Metallurgical Engineering
1953-1955	Chairman of the Graduate Standards Committee for Chemical and Metallurgical Engineering
	Monsanto Chemical Company
1952-1953	Senior Chemical Engineer
	University of Oklahoma
1955-1959	Professor and Chairman of Chemical Engineering
1956-1962	Associate Dean of the College of Engineering
1958-1963	Chairman of the School of General Engineering
1963-1991	George Lynn Cross Research Professor of Engineering
1989-1991	Robert W. Hughes Centennial Professor of Engineering
1991- present	Professor Emeritus of Engineering
	Constock Liquid Methane Corporation
1955-1960	Manager of Research, Development, and Engineering Conch Methane Services Ltd. (London)
1960-1963	Principal Consultant

	University Engineers, Inc.
1963-1978	President
1963-1978	Chairman of the Board of Directors
	University Technologists, Inc.
1978- present	President and Chairman of the Board of Directors
	Autoclave Engineers, Inc. (Erie, PA)
1945-1990	Consultant
1961-1990	Board of Directors
	Constock-Pritchard Corporation (Kansas City, MO)
1961-1963	Board of Directors
	Republic Geothermal, Inc. (Santa Fe Springs, CA)
1974-1975	Board of Directors

Honors

1958	Curtis McGraw Research Award, American Society for Engineering Education
1959	International Ipatieff Research Prize
1962	National Sigma Xi Lecturer
1964	George Westinghouse Award, American Society for Engineering Education
1967	Sesquicentennial Award for Distinguished Alumni, University of Michigan
1972	Member, National Academy of Engineers
1972	Peter C. Reilly Lecturer, University of Notre Dame
1973	Engineer of the Year, Oklahoma Society of Professional Engineers
1974	Engineer of the Year, National Society of Professional Engineers
1974	Oklahoma Hall of Fame
1975	Distinguished Service Citation, University of Oklahoma
1975	Award of Merit, Oklahoma Academy of Science
1976	Donald L. Katz Lecturer, University of Michigan
1978	William H. Walker Award, American Institute of Chemical Engineers
1986	Gas Industry Research Award, Sprague Schlumberger, Operating Section, American Gas Association
1992	University of Oklahoma established C. M. Sliepcevich Professorship in College of Engineering
1993	First Honorary Member of the University of Oklahoma College of Engineering Distinguished Graduates Society

ABSTRACT

Cedomir Sliepcevich begins the interview with a description of his family and early years in Anaconda, Montana. A firm educational beginning in Anaconda influenced Sliepcevich to attend college. He enrolled at Montana State College in 1937 in the chemical engineering program. During his sophomore year, Sliepcevich knew he wanted to go on to graduate school. In 1939 he transferred to the University of Michigan and there received his B.S., M.S. and Ph.D. in chemical engineering. While a graduate student, Sliepcevich studied thermodynamics under George Granger Brown. During the summer of 1942, he worked with Fred Kurata on a National Defense Research Council classified project on screening smokes. While earning his Ph.D., Sliepcevich was an instructor at the University, where he taught thermodynamics. After receiving his Ph.D. in1947, he also worked as a consultant for the U.S. Army V-2 rocket test program. In addition to his career in academia, Sliepcevich continued to do consulting work for various companies, including Monsanto Chemical Company, Constock Liquid Methane Corporation, and Autoclave Engineers, Inc. In 1955, he joined the faculty of the University of Oklahoma as Professor and Chairman of Chemical Engineering. Sliepcevich was instrumental in redeveloping the University's doctoral program and engineering curricula, and established the Flame Dynamics Laboratory there. He founded his own firm, University Engineers, Inc., in 1963, which specialized in fire protection systems for liquid natural gas. He officially retired from teaching in 1991, and continued to work as a consultant on many research projects. Sliepcevich concludes the interview with reflections on his career.

INTERVIEWER

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

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INTERVIEWER: James J. Bohning

LOCATION: University of Oklahoma

DATE: 1 March 1993

BOHNING: I know you were born on October 4, 1920 in Anaconda, Montana. Could you tell me something about your parents and your family background?

SLIEPCEVICH: Both of my parents were born in the 1880s in small communities (Gacko and Trebinje) in Herzegovina, Yugoslavia, within fifty kilometers of Dubrovnik and about one hundred kilometers south of Sarajevo. They did not know each other until they met in Anaconda, during the first decade of the 1900s, which at that time was experiencing a large influx of immigrants from southeastern Europe and Italy.

My father [Maksim] came to this country in 1906; his first home was Chicago because he had some distant relatives there, and there were some job opportunities for laborers in the steel mills. He did work for a while in the Gary Steel Mills, in which hiring and firing of people followed a pattern to accommodate business cycles. Nobody really had a steady job; you would go out to the mills every day and hope for the best, like a few days of work.

While my dad was in Chicago, he heard about the Union Station being constructed in St. Louis. The word had gotten out that they needed some strong backs, so he went to St. Louis, where he again endured the up and down employment cycles for about a year, when he heard about the expanding copper smelters in Anaconda, Montana. There was already a supply of immigrant labor there composed of Slavs, Italians, Scandinavians, Germans, but mostly Irish. When he arrived in Anaconda in 1908, he found that the labor supply was already greater than the current demand for labor in the smelters, but it also was apparent that this situation would change as soon as the expansion in the smelters could be completed. In the interim my dad found part-time work in the nearby gold and sapphire mines and the logging camps.

When my mother [Jovanka] arrived in this country in 1905 she went directly to California where she had some relatives (second cousins) who had preceded her. After living there for a couple of years, she went to Montana to visit other relatives. That’s where she met my dad. My mother and her younger sister were the only ones from a large family who immigrated to this country. She had two brothers who were very well educated. I may have to qualify that; one was an attorney [laughter] and the other one had a Ph.D. in history and theology and became a very prominent individual in the government in post-World War I Yugoslavia.

My father had one brother who did go through the equivalent of high school and another one who had completed grade school. My dad did not have any formal education, whatsoever; other than what he picked-up while working as a houseboy in a monastery. After immigrating to this country, his entire life was consumed in eking out a living. He worked as a laborer in the smelters in Anaconda for forty-seven years. At age seventy in 1955 he was forced to retire in accordance with the dictates of the union.

Both of my parents had a great belief in education, which they emphasized constantly. My oldest sister, Natalie, was the librarian at the Hearst Free Library in Anaconda, Montana, for fifty years, retiring in 1988. My other sister, Elena, devoted fifty-three years of her career to teaching and research, finally retiring in 1992 as a professor of health education and of medicine at Southern Illinois University in Carbondale. Her only hiatus from the classroom was when she headed a nationwide School Health Education Study [SHES] in Washington, DC between 1961 and 1972. I had my first two years at Montana State [College] at Bozeman.

BOHNING: Before we get to Montana State, I'd like to know a little about what it was like growing up in Anaconda and to talk about your early education there.

SLIEPCEVICH: Well, in looking back, I feel that I was fortunate to have had my elementary and secondary education in Anaconda which featured the old-fashioned, taskmaster schools, in an environment of uncompromised discipline. Practically all of the parents gave the teachers total support. In other words you didn't screw-up in school. It wasn't only the reprimand (which was sometimes physical) that you got in school; of more concern was what you got when you came home. You were always wrong and the teachers were always right (even when the teachers might have been wrong, at times). Nevertheless, the teachers were the most respected, and even adored, by parents, students and the community at large.

On the other hand, Anaconda, and its neighboring sister city, Butte, had—like most labor towns—the reputation of being real rough. They proudly sported the fact that they had more saloons per capita than any other place in the country. People were tough, even the kids were. If you wanted to compete and survive, you had a rough road to hoe, particularly during the depression years of the 1930s. The populace was divided between the rich and the poor with very few in between. The dividing line was the white collar versus the blue collar. The former included all the bootleggers—subsequently legitimatized—during the Depression, the white-collar employees at the smelters and the schoolteachers. Interestingly, one of the most pre-eminent metallurgical research laboratories in the world was located at the smelters in Anaconda, which was home to a large staff of prominent scientists and engineers.

To the outsider, Anaconda was an enigma in itself. Despite its rough edges the general populace, regardless of wealth or formal education, had a genuine appreciation for the arts—particularly opera. Even to this day, world famous guest artists make appearances in Butte and/or Anaconda. I attribute this existence to the multicultural European ancestry of the community. The first wave of immigrants retained their cultural background and observed their particular holidays in all the grandeur of their native customs and costumes. As a result we learned to understand the Germans, the Italians, the Scandinavians, and the Irish, particularly, who dominated the town. The end result was that although the populace lived hard, fought hard, drank hard, they nevertheless respected the better things of life, and somehow or other they managed to stick together.

Anaconda and Butte were considered as one entity to the rest of the state although they were twenty-five miles apart. The underground copper mines were in Butte, a mile below surface, while the sea level elevation in Butte was one mile. The smelters were located in Anaconda to take advantage of its copious water supply to feed the smelting operations. I guess I never appreciated the homology between Anaconda and Butte until I went to college at Montana State in Bozeman. There, people would point at you and immediately recognize you—with some degree of trepidation [laughter]—as being from Butte-Anaconda.

In many respects, I believe our environment instilled a deep sense of pride. I really learned some good lessons in life during the Depression years when the smelters shut down. At that time it was hard for me to understand why my family did not go on welfare because those that were seemed to be so much better off materially than we were. However, my father and mother would never accept charity. My dad went out and worked as a hired hand on farms that seemed to be prospering to feed the welfare program. Our family struggled, but we managed. I was fortunate to find summer work as a section hand (gandy dancer) on a railroad track maintenance crew in the summers of 1935, 1936, and 1937. I also had a morning paper route with one hundred twenty-five customers. When I look back now I understand that these experiences gave me another slant on life. It left the indelible impression that you've got to work for what you get, and I guess there's really no substitute for it, is there?

So with that, I went to Montana State.

BOHNING: Had you traveled any before you went to Bozeman, or was that the first time you had left home?

SLIEPCEVICH: Not really, except for 1928, which was the last good year before the stock market crashed. The smelters were booming, and laborers were making the unheard of sum of four dollars and twenty-five cents a day. My mother, my two sisters, and I went to Oakland, California, to attend a wedding of one of my mother's relatives and to visit other relatives in California that my mother had not seen since she left there twenty years earlier.

Other than the California trip, my travels had been confined to within the state for high school athletic or scholastic competitions. Since some of our destinations were as much as three hundred fifty miles away, we frequently stayed overnight one or two days.

BOHNING: You mentioned earlier that you worked summers on the railroad as a section hand.

SLIEPCEVICH: I first went to work on the local railroad serving the Anaconda smelters and Butte mines during the summer of 1935, and I had to adjust my age to get on the railroad. I was just fourteen years old, and sixteen was the minimum age. Besides, I didn't even weigh one hundred pounds then. To allay my folks' concern, I told them I was hired to be the water boy. In addition I continued peddling papers in the morning, seven days a week. On the railroad, I was expected to carry my load as a section hand: changing and tamping ties, cutting rails using a sledgehammer and chisel, and spiking. Since most of the section crew had little, if any, background in mathematics, the boss frequently asked me to help him with the occasional need for surveying track and doing some calculations. For this reason, the following summer I served in the capacity of a straw boss in charge of an extra (summertime) gang for repairing track. My immediate concern was how I would get people, some old enough to be my grandfather, to work for me. The first time I went out, we had to set track and put in a whole bunch of ties. It was a rush job so I didn't even have time to think about how I was going to get any work done. When we arrived at the site the crew just stood there looking at me, and nobody was making a move to do anything. Rather than say anything (I was reluctant to issue orders) I simply started working myself. I found out, so long as I worked, they worked, and we had no problems. It was a great experience, and it taught me a useful lesson in life—do as I do, not as I just say. I worked three summers on the railroad, and in order to keep my seniority, I would work a few days a month during the school year. During school vacations, they also let me work, so that I was able to put away more money for college. In fact, I was doing so well that I really debated about going to college because jobs weren't all that plentiful for college graduates when I finished high school in 1937. My debate, however, was very short-lived. My dad gave me the option of either going to college or moving out of our house. I got his message in a hurry, even though I did not have any idea in what I would major because I was interested in almost anything, academically. I had gotten a strong influence from my chemistry teacher, and math was my favorite subject. Throughout my life, however, I probably had given more thought to being a lawyer or a medical doctor.

BOHNING: What made you decide to enroll at Montana State College in Bozeman?

SLIEPCEVICH: I chose Montana State for two reasons. During high school I had been to Montana State several times to compete in statewide scholastic contests, and in the process I won some tuition scholarships. My second reason was that three of my good friends were enrolling at Montana State. One was Raymond Murphy, who wanted to major in civil engineering, and the other two, Joe McGeever and Jack Keig, had picked electrical engineering for their major. In addition, all three were outstanding football prospects. Consequently, when the four of us went to Bozeman about two weeks before fall registration to find housing accommodations—Montana State at the time did not have dormitories for men—it was more than a coincidence that the freshman football coach squired us around to find rooms. He also thought it would be beneficial if he took us to meet the dean of engineering. These gestures on the coach's part constituted probably the most aggressive recruiting of athletes that was practiced in those days. Dean William [Merriam] Cobleigh was an old timer who was very respected in the field of chemical engineering in those days. He wanted to visit with each one of us individually. Murphy went in to see him first and learned all about civil engineering. McGeever, and after him Keig, likewise got the electrical engineering pitch. Then Dean Cobleigh came to me and said, "You're next!" I responded, "Oh, I'm not going to be an engineer." He said, "That's all right, come on in and talk to me." I obliged but I wasn't eager; all I wanted to do was to be polite and get out of there! His first question was had I any preference for a major and I said, "No." He then asked "Do you like chemistry?" "Oh yes." He then asked, "Do you like math?" I said, "That's my favorite subject." I was determined to be agreeable. He said, "Good! We'll put you down for chemical engineering." [laughter] Had he asked the same questions about home economics or secretarial science, I probably would have given him a similarly positive response that day.

Now, the only positive thoughts I'd ever had about engineering were, in retrospect, extractive metallurgical engineering. Because I was raised in Anaconda and had spent time in the smelters while working on the railroad, combined with my deep interest in chemistry, I really knew and understood the entire process of extracting copper from low-grade ores. I also observed the unfavorable working conditions (typical of all pyrometallurgical processes in those days) that my father had to tolerate for forty-seven years. In the back of my mind, I kept thinking there most be a better way to recover copper.

BOHNING: What was Montana State like in your time, in general, and what was the department like in particular?

SLIEPCEVICH: I'd say the chemistry department at that time was superb at Montana State. The physics department was good, but I had the impression that it wasn't as well-recognized as chemistry. In math, I really had no standard of comparison at that time, but now as I look back it certainly was adequate although somewhat unconventional. The only thing I can say is, I wouldn't trade those two years I had out there at Montana State, in terms of preparation and training, particularly in chemistry. Besides, those were the two best years I had in college in terms of really having a lot of fun while benefiting from a sound academic program. I participated in intramural sports, championships in basketball and baseball, and varsity football (but no Heismans). In addition, I was very active in my social fraternity, Sigma Chi, through which I met some memorable people that I might not have otherwise.

During my sophomore year I had my first inklings about graduate study. Dean Cobleigh, without any prompting from me, initiated a dialogue with me on the virtues of graduate work. It was evident to me that he was very partial to the staff and program at the University of Michigan. My only knowledge of Michigan was from the football side. Although growing up in Anaconda meant that you were either a Notre Dame fan or you kept your mouth shut; [laughter] I dared to be different. Although I liked Notre Dame, and I particularly liked Knute Rockne, I was more caught-up with Michigan's Hurry-Up Yost. So that's all I knew about Michigan at that time.

As a result of my conversations with Dean Cobleigh and further probing on my own with some of my other professors, I began thinking seriously about pursuing a master's degree at Michigan after completing my bachelor's degree at Montana. During this period one of my roommates, Jack Keig, by reading the catalogs I had obtained from Michigan, became enamored with the school because it was one of the few programs in the nation that offered a degree in naval architecture and marine engineering. Keig had become disenchanted with electrical engineering and was seriously considering a transfer to Michigan.

In the summer of 1939, I left my job on the railroad at the end of July because I was offered a job (paying twice as much as the railroad) at some flourmills in Bozeman. They were desperately in need of workers who were willing to load, individually, one hundred forty-pound sacks of flour into boxcars for shipment into Canada and subsequently to England—World War II had begun.

Early in September, my oldest sister called me to advise that my roommate, Jack Keig, had stopped by our house to bid goodbye since he was going to Michigan. She said, "You didn't tell us anything about that." I said, "Well, what difference does it make?" She said "Maybe you ought to go with him." I told her I would think about it, which I did for about thirty seconds and on the spur of the moment [laughter] I said, "Tell him to wait; I'm coming home, and I'll go with him." That's how quick that decision was made.

My sister, Natalie, had started working in the library, so she said she would help me financially if I wanted to go to Michigan. With that I went home, packed, and went to Michigan. I stayed for sixteen years. In the process I got to meet my idol, Hurry-Up Yost; we became good friends.

BOHNING: Before we go to Michigan, what kind of chemistry did you have at Bozeman?

SLIEPCEVICH: Back in those days, chemical engineers took all the chemistry courses that were required for chemistry majors. The first year was general chemistry, and the second year was qualitative and quantitative chemistry, which consisted of four hours in class and three, four-hour laboratories a week.

I had a professor—he was the only one who didn't have a Ph.D. in the chemistry department—by the name of Paschal C. Gaines. This guy was really something else. His courses were philosophically and fundamentally different from the traditional qual and quant courses, like those taught at Michigan. Gaines never asked, "How do you separate this from that?" Rather he asked, "Why do you do so and so when you are trying to make a particular separation of a compound or an identification of an element" (of course by wet chemistry in those days); and you had to justify your answer by appropriate calculations. His style was unique, interesting and informative. He was tough; he demanded precision and responsible practice in the laboratory. Above all he was simply a grand person who had a big impact on me.

The freshman course in general chemistry was very good and interesting, particularly since the head of the department, Dr. Oden D. Sheppard, who was very well known in the field at that time, gave the lectures. (It reminds me of Professor Joel Hildebrand of the University of California who regularly taught freshman chemistry.) We never had any graduate assistants teaching recitation sections; they were all professors. In fact in the two years I spent at Bozeman I never had a graduate assistant teach any of my classes.

BOHNING: Was your math area through calculus or beyond?

SLIEPCEVICH: Just calculus and analytical geometry for the first two years. What was unique about these courses was that we had two hours of lecture for the entire enrollment of more than one hundred students, and two hours of recitation for about fifteen students per class each week. In addition we had a four-hour math laboratory on Saturday mornings (corresponding to our recitation sections) where we worked assigned problems and turned them in for grading. In other words, we had eight hours of required classes per week in math for both freshmen and sophomore years. Nevertheless, we did not get beyond the traditional calculus except for a brief introduction to the solution of ordinary differential equations.

BOHNING: Did you have any specific engineering courses?

SLIEPCEVICH: Yes. Aside from the customary courses in drawing, surveying and foundry required of all engineers, we had to take an introductory course in chemical engineering while all the other engineers took a course in mechanisms, or machine design. Our course in chemical engineering followed the textbook, *Chemistry of Engineering Materials* by R. [Robert] B. Leighou (1). Dean Cobleigh taught the course and really dwelled on water supply and purification, which was his specialty. Frankly, I was not enamored with this course; it left me with the impression and concern that chemical engineers did nothing else but figure out where to locate latrines. [laughter]

BOHNING: I recognize Leighou's book!

SLIEPCEVICH: It was a different ball game when I went to Michigan. As a matter of fact, I almost returned to Montana after a few days because of some difficulties that I encountered in transferring credit, for the math courses I had completed at Bozeman. The evaluator for mathematics, Professor Clyde E. Love, at Michigan was not familiar with the math department at Montana State (in contrast to the chemistry and physics evaluators who accepted everything I had at Bozeman.) The mathematics evaluator asked me to bring him a copy of the textbook we had used in Montana (2). After he examined the two volumes I brought him, he declared, "Nobody could learn any math from this book. For this reason I am advising that you repeat the sophomore level calculus course." My response was, "I'll be damned if I will. I have straight As in all my courses, including mathematics." He then replied that my only other option was for me to enroll in <u>his</u> section of ordinary differential equations, which he was teaching for math majors only, in order to prove I had acquired adequate preparation in calculus. For the first time in my life, I was really having difficulty in a math course.

Although the textbook for the course (3) was conventional, for some reason I seemed to have difficulty connecting mentally with Professor Love, but he was a wonderful person. I didn't know what he was really trying to do to me, particularly his assigning extra homework problems for me. Some of these problems really seemed unreasonable to me. On the other hand, Professor Love was always willing to meet with me outside of class. When he handed me a special assignment he would say, "Well, if you get stuck, let's talk about it." I recall an occasion when I called him at his home on a Sunday and after some discussion he cheerfully suggested I meet with him at his office within the hour. My original agreement with Professor Love was that if I could pass his course in ordinary differential equations he would accept for transfer credit all the math I had at Montana State. However, after completing his course, he advised me that he was still withholding transfer credit because he sensed that I struggled to get through. For this reason he was requiring me to take his advanced course, Partial Differential Equations, which again was in a section reserved for math majors only. Although he continued to use the same textbook by Kell (3) for reference, the substance of this course was on solving what he called, "research problems." In fact, the final exam consisted of ten such problems given over a period of two weeks.

Although I remember getting ninety-five percent on this final (the only graded test in the course), Professor Love again remarked that even though I was "really coming around" he wanted me to take his course in advanced calculus which he was offering in the fall semester. Since he still had me over a barrel [laughter] on transferring my math credits from Montana, I told him I would, but only if he approved my Montana math as of that moment. He agreed!

The following fall, (1940, which was my senior year) I lived up to my bargain and enrolled in his Advanced Calculus. Because I was not allowed to substitute the extra math courses for some of the required courses in chemical engineering, I had resigned myself to the fact that I would have to attend the summer session following my senior year to take two required courses in mechanical engineering, Heat Engines and Machine Design, in addition to my undergraduate thesis (which was required in those days).

Another complicating factor was that I was carrying advanced ROTC, which could not be substituted for any required course or restricted elective. Since it was a foregone conclusion that it was only a matter of time before we entered World War II, I wanted to complete the program so that I could get commissioned as a second lieutenant. On the other hand, I was concerned that if I were commissioned in June 1941, I might be called to active duty before I could complete my required summer session program. After much cogitation I concluded that if I dropped Advanced Calculus and carried twenty-four hours the final semester, I could graduate in June 1941. My only obstacle was that twenty-one hours were the maximum allowed—and then only with very special permission. Here, I must confess to some degree of subterfuge. I figured I would enroll in eighteen hours, and after two weeks I would drop Advanced Calculus and subsequently add the required courses. I had figured (correctly) that the enrollment system would not catch the overload hours. Incidentally without my revealing too many details of my plan, Professor Love understood why I was dropping his course.

All my plans materialized except one. When I took my final physical examination preparatory to being awarded a commission, the army medical examiner discovered that a perforated eardrum was a “no-no”. I applied for an exception and even went forward with an experimental program at the University of Michigan, which was supposed to give me what they called a “dry ear.” I had lengthy weekly treatments over a period of about two months, which were very painful—as I still recall and still can feel when I do. The treatments were not successful and I was again denied a commission. Fifteen years later, a brilliant otologist in Oklahoma City, Dr. J. V. D. Hough, who had pioneered and perfected a surgical technique for grafting human skin onto the eardrum, took me as one of his early patients and succeeded in closing the perforation.

BOHNING: What was the chemistry like then, when you started at Michigan?

SLIEPCEVICH: The first course that I had was physical chemistry which was scheduled for the two semesters of the junior year; it was specifically designed for chemical and metallurgical engineers, and it did not include a laboratory. These courses were generally reputed to be tougher than the corresponding courses for chemistry majors because they included a stiffer dose of calculus.

BOHNING: Who taught that?

SLIEPCEVICH: Lee Owen Case was in charge of the course and usually taught both semesters. However, when I took it, Professor Roger Henry Gillette taught the first semester (primarily thermodynamics) and Case taught the second semester (emphasis on heterogeneous equilibria or phase rule). However, these courses also included introductions to atomic and molecular structure, colloids, chemical kinetics, and electrochemistry. The textbook by Getman and Daniels (4) was used primarily as a reference because both Gillette and Case had prepared monographs covering their lecture material.

Physical chemistry, and a junior level course in chemical engineering called Inorganic Chemical Technology (5) were reputed as being the toughest courses at the University of Michigan. The chemical engineering department actually prided itself on how many students they could weed-out—like one-third—of their program with these two courses.

As you know, Michigan had some very outstanding professors on their chemistry staff.

The ones that come to mind are the organic chemists Leigh C. Anderson, Werner E. Bachmann, Robert C. Elderfield and Moses Gomberg along with physical chemists Floyd E. Bartell and Kasimer Fajans, all of with whom I had some contact.

In my book, however, Lee O. Case stands above all, even though he did not have the recognition or prominence that he so justly deserved. He not only was a brilliant master of heterogeneous equilibria, he was a superb teacher and an exemplary human being. One of his unusual traits was that in the first day of class, in which there were generally well-over one hundred students, he would call roll—always Mr. So and So or Miss So and So—and have the student respond by raising his or her hand. Thereafter, he never forgot the student's name; even years later when he encountered a former student he always seemed to remember the name.

BOHNING: Professor Lee Case obviously left an indelible impression with you. Let's turn to the chemical engineering faculty; e.g., Walter Badger, George Granger Brown, and others.

SLIEPCEVICH: Badger, [Edwin] Baker, [Jack C.] Brier and Brown were among the "old - timers" on the staff whom I referred to collectively as the four Bs because they could really "sting" if the spirit so moved them. Notably, only G. G. Brown had a Ph.D. among this group. By the time I arrived, Badger had recently left the staff to devote full time to his consulting work. Nevertheless, I had an opportunity to get acquainted with him. He impressed me as being rough and tough. [laughter] One thing I'll always remember about Badger is his definition of a perfect chemical engineer as a physical chemist with a lot of common sense.

In contrast to Badger was Edwin Baker. Although they worked closely and co-authored the book, *Inorganic Chemical Technology* (5), they were very different personalities. Baker was a gentle and quite sedate individual. He made many important contributions to extractive distillation, electrochemistry, corrosion and mass transfer in general. I did my bachelor's thesis under him on the effect of concentration on the plate efficiency of bubble-cap distillation columns, and he was superb. He gave me as much attention as he gave his Ph.D. students (of which he had many). I really liked Professor Baker and I was saddened by his unexpected death from a heart attack in the mid-1940s.

Although I was acquainted with Professors Brier and Brown, my first real encounter with them did not transpire until I entered graduate school. My favorite professor in chemical engineering on the undergraduate level was Professor Donald L. Katz, who taught the unit operations course in heat transfer and fluid flow. He was simply a tremendous individual—the most dedicated and hardest working individual on the staff and unquestionably the best teacher. He was sound on his fundamentals, but he also had an exceptional grasp of the practical side.

BOHNING: What prompted you to enroll in graduate school?

SLIEPCEVICH: The last semester of my senior year I was hired as a research assistant by Professor Donald W. McCready, from whom I was taking the unit operations course on distillation, extraction and drying. Dr. McCready had two sponsored projects: one to develop water-resistant plastic from lignin, and the second to find substitutes for tung oil polymers which were used primarily in brake linings. Both projects were classified and qualified for employee deferments. He promised me full-time employment during the summer and half-time employment during the academic year 1941-1942, if I enrolled in graduate school. Since I was intrigued by the ersatz nature of the project and its classified status, I accepted.

BOHNING: I assume this decision presaged your so-called "encounters" with Brown and Brier, which you mentioned earlier.

SLIEPCEVICH: The first semester of my graduate program in chemical engineering included the required courses in thermodynamics taught by the famous George Granger Brown. In those days chemical engineers did not have an individual undergraduate course in thermodynamics because the other required courses in stoichiometry, physical chemistry and heat engines (in mechanical engineering) were deemed to give adequate preparation.

Professor Brown had a large physique to go with his booming voice that rocked the classroom. His seeming arrogance was not misplaced. By all accounts he was an unsurpassed logician, particularly in thermodynamics. Although he had a superior grasp of mathematics, he constantly emphasized that it should be used only as a tool and not as a crutch. He had a remarkable facility to reason a concept through to a final equation without going through the formalities of a mathematical derivation.

Another pedagogical strategy that Brown used was to pick one student early in the course that he figured would argue with him. Brown was a strong believer that critical thinking leading to argument was the most effective way to learn thermodynamics, and he encouraged students to get together outside of class and do just that. He demonstrated this philosophy every day by picking-on this one student who that year happened to me. Of course I rarely won an argument, and at times really got worked over pretty good, but in the process I learned a lot more about thermodynamics than I would have otherwise. Besides, I had a lot of fun trying to push G. G.—surreptitiously referred to as Great God, but never to his face—to the edge.

Brown's class was legendary. Fifty years later, upon the occasion of a classmate's retirement as an executive with one of the automotive companies, he was asked—among other things—what his favorite course was at Michigan. Without hesitation he responded Chem E. 105, Thermodynamics, not only because he learned more from G. G. Brown than from any other professor, but also because he always looked forward to Brown and Sliepcevich "going at it."

BOHNING: And what about your encounter with Brier?

SLIEPCEVICH: Professor Jack C. Brier was a retired colonel for the Ordnance, having served in World War I and again in World War II. He gave the impression of being a rough and tough character. When I first met him, it was evident that he was champing at the bit waiting to be called back into the service. His colorful vocal outbursts did not belie his demeanor. On the other hand, he had unlimited compassion for his students so long as they were honest and hard-working.

During the second semester of our master's program, Professor Brier volunteered to introduce a course in plastics and polymers, since we did not have anything comparable to this subject in our curriculum despite the growing importance of these new materials. Most of this new course consisted of doing a lot of library research, discussions thereupon in class and a compilation of notes for our future reference. Because I was working on the research contract to find substitutes for tung oil polymers, Professor Brier asked me if I could make a presentation to the class, at least on the unclassified aspects of this project. It so happened that a few weeks earlier I had succeeded in making a rubbery-polymer from fish oil. I brought a sheet about 1000 cm^2 to class and proceeded to describe the chemistry, qualitatively. When one of the students asked me if I had identified the compound I replied: "Of course, its isopenta<u>fishy</u>lene!" I still don't know what possessed me to blurt out that particular name, and obviously, I thought I was making a joke. What concerned me was neither the class nor Professor Brier caught on and I was deeply concerned what Professor Brier's reaction would be when, or if, he or any of the students got the message. I knew Professor Brier detested "smart alecks." Fortunately for me the subject eventually died of natural causes.

BOHNING: Were you able to apply any of your research on plastics and polymers for academic credit?

SLIEPCEVICH: No! As a matter of fact, it was against departmental policy in those days to apply any research for which you were remunerated (except for unrestricted fellowships, of which there were few) for academic credit.

BOHNING: But, you did get your master's degree in June 1942?

SLIEPCEVICH: Yes, I did and almost simultaneously without any forethought I made a decision that was literally going to predestine my entire career. The plastic-polymer project was in the process of winding-down when Professor G. G. Brown asked me if I would work full-time during the summer of 1942 to help Dr. Fred Kurata, a Research Engineer, complete a National Defense Research Council's classified project on mechanical formation of screening smokes. It sounded intriguing enough to me to accept the job on the spot.

My classmate and close friend, Harry Drickamer, had already accepted a job with Pan

American Refining Company in Galveston, but before reporting to them he was going to stay in Ann Arbor for two more weeks to take the preliminary and qualifying examinations for the doctoral program. Those exams were a marathon: four days of four-hour exams the first week covering undergraduate work and four more days of four-hour exams the second week on the graduate level. Figuring I had nothing to lose, I decided to take the exams, too. As luck would have it, both Harry and I passed the examinations, which meant we were eligible to pursue a doctoral degree. Harry went to Galveston and I stayed to work with Fred Kurata on screening smokes.

In addition, I finished some work I had started earlier in the year related to the retrograde behavior of condensate wells, for which Professor Katz was the principal consultant to the industry. The results turned out to be quite interesting, so Professor Katz and I co-authored a paper (6). At the time, this work appeared to be sufficiently adequate to support a doctoral thesis but the department chairman (G. G. Brown) took a dim view of granting a Ph.D. to a twenty-one-year old, which didn't really upset me because I had not given any thought to a higher degree.

BOHNING: What prompted you to stay on at Michigan?

SLIEPCEVICH: Just about the time Dr. Fred Kurata and I completed the final report on screening smokes, Professor Katz asked me if I would be interested in working with him on an electron microscope study of asphaltenes. The fact that this instrument had just recently been developed in our physics department sounded exciting to me. Consequently, I committed to work part time during the following academic year (1942-43). While waiting for the asphaltene samples to arrive from the West Coast, I started playing around with the electron microscope, mostly to learn the techniques for preparing the samples to insert into the vacuum chamber. The first material I prepared for practice observation was some Portland cement, to which I had added just enough water to achieve hydration. As luck would have it, the first sample I inserted into the microscope I saw <u>every</u> hydrated constituent of Portland cement in this one field of view (which virtually defied probability). I next proceeded to identify each of the crystals by examining the individual constituents of Portland cement that I had subjected to hydration. Professor Katz and the research assistant in charge of the electron microscope laboratory in the physics department co-authored a paper (7) with me, which gained international recognition.

For this reason, I half-heartedly (maybe, impishly) tried to peddle this work for a Ph.D. thesis to G. G. Brown, the department chairman, and this time I got a different response. He rationalized since I had been paid for doing the work on a Faculty Research Project (a grand sum of one hundred fifty dollars) it was not acceptable for a thesis. I was twenty-two years old at the time so he couldn't hit me again with the unpublished rule regarding twenty-one-year-olds.

Just about the time I completed the electron microscope study, I was "drafted" as a teaching fellow for the spring of 1943 to teach the Physical Measurements and Fuels Laboratory course required of all chemical engineers on the junior level.

Later on that spring, Professor Donald L. Katz was retained as a consultant on heat transfer through finned tubes. Kellex Corporation, a division of M. W. Kellogg Company, was the prime contractor for constructing the thermal separation section of the Oak Ridge Atomic Energy Plant. As usual, it was a crash program and since Professor Katz was already heavily committed elsewhere, he asked me to design and construct the test unit and to obtain all the experimental data at very high rates of heat transfer. This assignment blew my mind because I realized that I was now working on a project related to building the atomic bomb. By the end of June 1943 I had completed the project and presented the final report.

BOHNING: I can see from your steady stream of projects that it would have been difficult for you to leave the University even if you had really wanted to depart.

SLIEPCEVICH: Right, and still more was on the way. The ink had hardly dried on the finned-tube report when I received a phone call from a company in Chicago that was involved in the development of the proximity fuse. I had done some consulting work for this company on several minor projects before, but this time it was for real. They wanted me to go to Johns Hopkins Applied Physics Laboratory in Silver Springs, Maryland to review and evaluate the work being done on the development of the power source for this device. This program appeared to me to be shrouded in more security restrictions than the atomic bomb, which made it more exciting. After spending much of July and the first half of August there, I was able to complete my evaluation and prepare a report. A few days before I was scheduled to leave Washington, DC, I got a call from Professor G. G. Brown. He and Professor D. L. Katz had been retained by the National Gas Association of America on behalf of forty gas producing companies to design, construct, and obtain operating data for a full-scale plant for sampling high pressure gas wells. Professor Brown said that he wanted me to go to Katy, Texas, to be the full-time resident engineer and that he and Professor Katz would visit there periodically as their heavy schedule of other commitments would permit. Again, this project was a wartime crash effort related to the synthetic rubber program. Since I had never been to Texas, much less on a gas field, I said to myself, “Why not?” I returned to Ann Arbor on August 15, met with Professor Brown on the morning of August 16 for a briefing on the project and left for Houston that afternoon to spend three months in the gas fields. Despite having to work from daybreak to sundown, seven days a week, I thoroughly enjoyed every minute of it, mostly because I had a chance to meet some great people. This project had a lot of visibility with the top echelons in the gas industry. I returned to Ann Arbor in November 1943. This time I decided to stay put until I completed my doctoral work. With the aid of some creative maneuvering, I was able to enroll late—two weeks beyond the deadline, or four weeks after the start of the semester—in the last course I needed for my doctoral class requirements.

Thus between November 1943 and March 1944, I pursued my degree until Badger, who had a very active consulting business headquartered in Ann Arbor, asked me to assist him in establishing design standards for the stability limits of Dowtherm when operated for extended periods at 500º C and 15 atm. I designed and built essentially a reflux unit consisting of a boiler, a vertical tube (10 centimeters in diameter and 3 meters long) and condenser through which I could circulate Dowtherm at various flow rates over extended periods of time (twenty-four hours a day for about a month). Samples of Dowtherm were taken periodically to determine decomposition rates.

Although two years had elapsed since I had completed my master's degree and I had made little progress on the experimental work for my doctoral thesis, I had acquired considerable experience: major responsibility on eight unrelated, consulting projects. I have always felt that chemical engineering at Michigan was unique in this respect in those days; I doubt if similar circumstances did—or even could—happen elsewhere.

BOHNING: It would appear that every time you attempted to get back to pursuing your doctoral degree, you were distracted by a consulting project. Yet, it is evident that you were acquiring some excellent practical experience at a high level of responsibility. In looking back, was it worth it?

SLIEPCEVICH: All was not lost on my academic pursuits, between 1942 and 1944. I was able to squeeze into this period the completion of the remaining course requirements for a Ph.D., the French and German examinations, and the twenty-one-day process plant design problem, as well as approval of my doctoral thesis prospectus by the department on February 1, 1944, just prior to initiating the aforementioned Dowtherm project. At this point I had completed a literature survey on the hydration of olefins to alcohols, which carried a high priority with the War Production Board. This endorsement was essential because it was the only way we could be assured of getting materials and instruments allocated to us for this experimental work.

Although it was my desire and intention to do a thesis under Professor D. L. Katz, that was not to be. Professor G. G. Brown, as departmental chairman, had simply decided that he would chair my doctoral committee. Unfortunately, in my earlier days in graduate school I had derived enjoyment out of needling the chairman about the narrow focus of the thesis topics in the department, particularly the accumulation of physical properties of hydrocarbons and the virtual absence of chemical reaction kinetics and reactor design. (At that time, I was not aware that Professor Brown had done some significant work with his graduate students on catalytic cracking.) Any rate, I had made my bed, and Professor Brown was going to make me lie in it. In other words, my thesis was to focus on chemical reactions. Since I had acquired a fascination for working at elevated temperatures and pressures, I somewhat conceitedly (and fatuously) added this complication to my research.

BOHNING: Before we delve further into your research, did you experience any "encounters" (using your expression) with faculty outside your department?

SLIEPCEVICH: I certainly did, and they were all encounters of the most memorable kind.

First on my list is Professor George Eugene Uhlenbeck, from whom I took his doctoral level course in classical thermodynamics for physicists in the spring semester of 1943. Professor Uhlenbeck was unique; even though he also taught the courses in statistical mechanics and quantum mechanics, his thermodynamics course was purely classical (macroscopic). His lectures were the ultimate in beauty and perfection. I never took a note during the class—he did not use a textbook. He would lecture for two hours while I sat there virtually mesmerized. That evening I could sit down and develop, from memory, a complete set of notes covering his lectures. Everything he said seemed so obviously clear and logical to me. Yet the physics majors (I was the only "outsider" in the class) seemed to struggle with the course, perhaps because it was so unlike any other courses they had taken. Had Professor Uhlenbeck departed from the classical, macroscopic approach and emphasized the microscopic (statistical) approach, then the physics majors would have been riding high and I probably would have struggled.

I had some discussions with Professor Uhlenbeck about the possibility of my switching to a Ph.D. in physics so that I could work under him. He seemed quite receptive except for the fact that he was leaving Michigan at the end of the semester to join the wartime radiation laboratory at MIT [Massachusetts Institute of Technology]. I recall his telling me at that time that he was not certain whether he would return to Michigan. He did not; rather, he joined the Rockefeller Institute at Princeton after he left the radiation laboratory.

My second encounter of note was Professor Ruel Vance Churchill, the noted mathematician. He had introduced a new graduate level course on higher mathematics for engineers and physicists in the fall of 1942 in which I enrolled. He (like Uhlenbeck a year later) had me hooked on math right from his first lecture. Upon completing this course, Professor Churchill suggested that I give serious thought to switching to mathematics for my Ph.D. Had I not been so involved in exciting engineering consulting projects at that time, I probably would have made the switch, not necessarily because of the subject but because of the individual (Professor Churchill) under whom I would work.

And, lastly, my undergraduate encounter with Professor Lee Owen Case in physical chemistry is still vivid with me. I took his doctoral level course in physical chemistry on heterogenous equilibria in the spring of 1944. He did not offer the course regularly—only when there was sufficient student demand, and that wasn't too often because the course had a reputation of being tough. My undergraduate impression of him was that he was tops, but after taking his graduate course his standing with me went even higher—to the point that I had some serious thoughts about switching my major to chemistry to work under him.

BOHNING: At this point in time, however, it appears that you had irrevocably committed yourself to doing you doctoral thesis in chemical engineering under G. G. Brown.

SLIEPCEVICH: I suppose that's a fair conclusion, but there was another factor that came into play during the summer of 1944. The chemical engineering department had finally decided to introduce a junior level, required course in thermodynamics. Up to this point—as I mentioned previously—the only required course in thermodynamics was for master's candidates; teaching it was the exclusive domain of G. G. Brown. However, when the undergraduate course was introduced, he assigned D. L. Katz to handle some of the lectures. Both of them were still very active in consulting work, which required them to be out of town frequently. Between the two of them they did a masterful job of juggling and timing their scheduled absences to minimize the impact on students.

One of the advantages (or penalties) of doing a thesis under G. G. Brown is that you automatically became his assistant for executing various tasks. In my case, he assigned me in the summer of 1944 to grade papers, make up problems, teach recitation sections, and, in emergencies, to lecture. For the next two years, I was literally swamped in these somewhat menial tasks—one thing it did accomplish is that it grounded me in Ann Arbor and stopped my gallivanting around the country on various jobs.

In the summer of 1946, Brown threw in the towel in so far as the undergraduate course was concerned by promoting me from teaching fellow to instructor and giving me complete responsibility for the course. However, at the same time, I continued to be his assistant in the graduate course in thermodynamics and actually taught his classes when he was away.

BOHNING: Did your assignments in thermodynamics delay progress on your doctoral thesis?

SLIEPCEVICH: Unquestionably, but I didn't mind since I loved being involved in the thermodynamics classes.

BOHNING: Did you continue to engage in other projects—as you previously had done—during this period?

SLIEPCEVICH: Between the summer of 1944 and the fall of 1947, when I actually completed my doctoral thesis, I stayed clean except for a few short term consulting jobs. However, by late 1947, I was beginning to drift again. My first project of any consequence was as a consultant to the Army to make thermodynamic performance analyses of a series of rocket fuel and oxidizer combinations. I recall having to optimize techniques for solving a minimum of ten, non-linear equations simultaneously with only an electronic (Friden) calculator at our disposal (electronic computers were not available in those days). This project opened the door for me to participate in the V-2 rocket test program at White Sands, which in turn evolved into a major program for me in light and energy scattering. This area would occupy my major attention for the next seven years.

BOHNING: Was this light scattering work in any way related to your work five years earlier on the mechanical formation of screening smokes?

SLIEPCEVICH: Actually, it was largely responsible for my getting the project because it was where I first learned about formation of smokes, particle size analysis, light scattering, et cetera. The substance of my participation in the V-2 test program was the generation and discharge of a visible smoke or cloud (several hundred kilograms in twenty seconds) from a V-2 rocket during flight, between 20 and 40 kilometers elevation, which could be photographed from the ground in order to study upper atmospheric winds and turbulence. It was here that I developed my first taste of meteorology, which was going to consume my interests again a decade later.

My first obstacle to participating in this V-2 program was to respond to a request for a proposal (RFP). At that time, I was unaware of the high level of interest and competition for this project. My approach was novel to the extent that I had proposed atomizing a liquid by means of a vibrating-type nozzle which we had developed during the earlier (Brown-Kurata) screening-smoke project. Apparently, there were enough other enticements in my proposal to invite me to a meeting of the technical advisory committee for the Meteorological Branch of the Army Signal Corps at Camp Evans, New Jersey. Dr. Michael Ference (the pioneering cloud-seeder) was the chief scientist and he chaired the meeting. The advisory staff was composed of about seven or eight high-powered intellectuals whom I had not met before but had heard of by reputation.

It wasn't long before the meeting evolved into a spirited debate. In my proposal I had stated that the majority of the particles produced by the atomizing nozzle would be in the 20-micron range. That statement immediately precipitated an argument as to the maximum size particles that would be visible, and therefore could be photographed, from the ground. The center of focus was the Mie theory of light scattering. Long distance phone calls were even made to consult other experts in the field. There I sat, letting them argue because I understood very little of their discussion. Finally, after a few hours they generally agreed that the particles would have to be around 0.5 microns, which at that moment seemed to disqualify my proposal because I was projecting particles about fifty times larger.

When Dr. Ference turned to me and said, "You've been very quiet; do you have any comment to make?" To this day I don't know what had possessed me. Throughout this session, for some reason I couldn't keep my eyes off of a beautiful photograph of a natural cloud hanging on the wall behind Dr. Ference's desk. I simply asked if he knew the origin of this photograph, to which he replied that he had taken it himself. I then asked if he had any idea how far away the cloud was when he photographed it, and he said about 70 kilometers. (What he had just told me, and the group there, was that clouds—which generally have particles in the size range of 20 to 40 microns—are visible even 70 kilometers away.) A deadening silence befell them, whereupon Dr. Ference told me to get back to Ann Arbor to get started on the project. He then adjourned the meeting.

BOHNING: Before we proceed further into this area, what did you accomplish in your doctoral thesis, which you said you completed late in 1947?

SLIEPCEVICH: Actually my thesis consisted of two parts: one, the design, construction and operation of a continuous apparatus for studying the kinetics of catalyzed (primarily) reactions at elevated temperatures and pressures in a tubular reactor, and two, the rates of dehydration of butanol over an alumina-silicate catalyst (8). The mechanical design part of the thesis launched me into an area from which I profited substantially—both academically and industrially—for the next half-century. Likewise, the chemical kinetics part of my thesis not only introduced me to the substance of reaction mechanisms, but also gave me the background to solve some testy problems in the real world. In other words, the thesis itself spawned some worthwhile practical applications, but obviously no Nobel Prizes; the closest was the international Ipatieff Prize, which I received in 1959. This prize is awarded by the American Chemical Society not more often than once every three years.

BOHNING: You indicated that your V-2 rocket project spawned other related activities for a decade or so. For example?

SLIEPCEVICH: First and foremost it provided me with the means to support my second doctoral student. I had been promoted to Assistant Professor in the fall of 1948 and was authorized to supervise theses. Roland O. Gumprecht was the most outstanding among the seventy doctoral students I have directed (fifteen at Michigan, fifty-five at Oklahoma). Gumprecht was a freshman when I was a sophomore at Montana State. He completed his work there in 1942 and served as an officer in the Army until he was discharged in 1946. He enrolled at Michigan for the summer session in 1946 with the intention of completing only his master's degree. The fact that I knew him at Montana helped me to "con" him into staying on for his Ph.D.

Gumprecht's thesis was focused on particle size measurements by light scattering. Since we were interested in particles up to about 50 microns in diameter (one hundred times the wavelength of light) the first order of business was to extend the Mie theory of light scattering. Although experimental confirmation of the theory had been confined to particle sizes comparable to the wavelength of light, it was generally believed that the theory could be extended. The reason it had not been done was the sheer magnitude of the computations that were involved. Gumprecht had estimated early on that it would take him at least fifty thousand hours with a standard (Friden type) calculator to complete the minimal computations. We initially had high hopes that the University of Michigan's electro-mechanical computer, the IBM Model 602A Calculating Punch, would be adequate; unfortunately, this computer would have reduced the calculating time by only a factor of eight to about six thousand hours.

Fortunately the ENIAC, the Army's high-speed (in those days) electronic computer at Aberdeen Proving Grounds in Maryland, became available to us because of the interest of the military in our work. Once the details of the programming had been worked out by Gumprecht and the ENIAC staff, it required about two weeks' time to carry out the computations. (To my knowledge, this work represents the first doctoral dissertation completed that was totally dependent on a high-speed computer.)

In return, we had agreed to publish tables of our calculated results so that others working in related areas could benefit (9). The results must have been important since the Nobel laureate, Professor Peter Debye of Cornell University, volunteered to write the foreword for the first volume in light scattering functions. Professor Debye had become interested in our work early on, and he was a constant source of encouragement. (In fact, if it had not been for him, I think I might have thrown in the towel because of the magnitude of the task we had undertaken without any prior assurances of success.)

Aside from the calculations, Gumprecht's thesis posed some difficult experimental problems to confirm and extend the Mie theory. First, an entirely novel optical system had to be conceived and constructed; problems of designing and <u>building from scratch</u> specialized amplifiers, regulated power supplies, modulated light source and transmitted light receivers, and manufacture of glass spheres, segrated according to size, from 5 to 100 microns in diameter. None of these items were available for purchase at that time. Another time-consuming effort in the early days of the project was programming the IBM 602A, which required hard-wiring about a dozen panels that looked like an intricate telephone switchboard. No one was available to help Gumprecht in this programming, not even IBM who had advised it couldn't be done. He simply had to figure it out, and he approached it from the attitude of "all in a day's work and nothing more!"

BOHNING: Going back to the tables for light scattering functions (9), were they produced locally?

SLIEPCEVICH: Yes, they were published by the Engineering Research Institute of the University of Michigan, but they were lithoprinted and bound in 1951 by Edwards Brothers, Inc. of Ann Arbor. Since Gumprecht and I were both former students of Dean William Merriam Cobleigh at Montana State, we dedicated the Volume I on light scattering functions to him. As I mentioned previously, Professor Peter Debye wrote the foreword to this volume, which contained five hundred seventy-four pages of tables. The second publication on Riccati Bessel functions contained two hundred sixty pages of tables, and the third publication on partial derivatives of Libendre polynomials had three hundred ten pages of tables. As I recall, the Research Institute distributed about one hundred fifty copies of this set of tables to libraries and a few hundred more were sold at six dollars and fifty cents for the light scattering functions and three dollars and fifty cents each for the other two sets. (I doubt that the Research Institute recovered all their costs, however.)

Over the next two decades, as high-speed computers became more commonplace, parts of our tables were checked independently by other laboratories throughout the world. Except for two <u>minor</u> errors of transcription in two entries the feedback I received was indeed amazing, particularly since the scattering functions were reported to seven significant figures.

BOHNING: What was the follow-on to Gumprecht's work?

SLIEPCEVICH: It came in two forms: one, to resolve some questions which arose during Gumprecht's work; and two, to put us in an excellent position to investigate multiple scattering from optically dense dispersions.

With respect to the first, by the time Gumprecht completed his thesis, we already had four more doctoral candidates in this pipeline to pursue natural take-offs. Gumprecht, with the help of these students, had really put together a unique optical laboratory for studies on aerosols (10, 11, 12).

With respect to the second fall-out, we really got into a major undertaking in spite of ourselves. About the time that Gumprecht completed his thesis, a request for a proposal related to multiple-scattering from dense dispersions came to the attention of the Research Institute. The genesis for this proposed study was classified top secret at that time since it was related to the attenuation of radiation from an atomic bomb blast by means of an artificially-created, dense aerosol—ever since 1941 I could not escape from screening smokes—consisting of, for example, dispersed oil particles. The Research Institute at Michigan was keenly interested—of the arm-twisting variety—in submitting a proposal, and in view of their support in publishing the tables, both Gumprecht and I were "beholden to them." Since both Gumprecht and I had serious reservations whether we could win a contract—aside from the fact <u>we were not overly enthused</u> about getting involved in this complicated problem—we decided to put our best foot forward and prepare a detailed proposal with a high dollar cost estimate. In this way, we could discharge our obligations to the Research Institute and at the same time price ourselves out of competition.

As luck, or fate, would have it, about a month after submitting the proposal, I received a telephone call from a Colonel in the Army Chemical Corps Procurement Agency advising me that even though our cost estimate was three times higher than the next highest bidder, we were being awarded the contract as of June 1952 because of its superior technical content. Fortunately for the Research Institute, and me in particular, Gumprecht changed his plans for leaving Michigan. He agreed to remain long enough to get this project underway and to indoctrinate a technical staff. Another complication was that I had previously committed to spend a year on leave at Monsanto Chemical Company in St. Louis, from June 1952 to June 1953. With this contract coming on, I got Monsanto to agree to a delay until September. That left me three months to work with Gumprecht and to find someone on the faculty to supervise the project (only faculty were permitted by the Research Institute to do so).

In anticipation of my absence, I had already picked my replacement, Professor Stuart W. Churchill, which wasn't difficult because he was the only one that I thought was qualified to step in cold on this project. In addition to his Ph.D. in chemical engineering, Stuart had a master's degree in mathematics, which was the bottom line discipline for this project. Stuart was initially somewhat reluctant to assume this responsibility, but fortunately we were close friends, and he wanted to help me out of the bind. As Lady Luck would continue to appear, we found another perfectly qualified individual in our midst. Like Gumprecht, Chiao-Min Chu had just completed his Ph.D. in electrical engineering on the scattering and absorption of water droplets at millimeter wavelengths. At that moment, Dr. Chu was working on a radar contract in electrical engineering. We had gotten acquainted with him when Gumprecht was wrapping up his thesis. Dr. Chu was, in every respect, a brilliant applied mathematician. He agreed to work with us, and true to our expectations, he was a genuine asset.

The crux of the project was the mathematical formulation of the problem and the derivation of solutions. The difficulties in solving the governing transport equation have been documented, and they don't seem to go away. In fact I noted in some recent journal articles that similar problems continue to plague our atmosphere models for long-range weather forecasting. In our case, we evaluated various approaches such as integral solutions, spherical harmonics, quadrature, diffusion (neutron scattering), and the Monte Carlo method but we had the most success with our own discrete flux (six flux to be exact) method (13). Although the contract did not call for it, we proceeded to carry out some experimental verification with reasonable success (14).

BOHNING: When did you leave for Monsanto?

SLIEPCEVICH: Around the middle of September 1952.

BOHNING: What was the nature of your assignment?

SLIEPCEVICH: The previous year Monsanto—in collaboration with the American Institute of Chemical Engineers [AIChE]—had initiated an industrial leave program, which consisted of having a young chemical engineering professor spend a year working full-time with them in order to gain industrial experience and thereby to enhance his capabilities as a professor. Unbeknownst to me, somebody had nominated me as a perfect candidate (and as I later had reason to surmise, for a less than altruistic purpose).

In discussing the program with Monsanto, I made it clear to them that I would not waste a year of my time observing industrial practices, teaching refresher classes, and working on trivial, academic-type correlations. I told them I would accept only if I could be put into the loop as a senior engineer (or such) and assigned to work on some <u>pressing</u> production problems. They agreed to think about it—since it was a big departure from their program—and to let me know. Apparently the assistant plant manager, Desmond B. Hosmer, of the W. G. Krummrich Plant at Monsanto (East St. Louis) Illinois volunteered to accept responsibility for my behavior.

The fact that Hosmer had a master's degree in chemistry from Michigan probably influenced him, aside from the fact he was somewhat of a maverick at heart.

The first day that I reported to Hosmer at the Krummrich Plant in September, he assigned me to figure out a way to make the one remaining step in their phenol plant, the fusion reaction, continuous. All the steps in this process, upstream and downstream from the fusion step, were running continuously. I remember Hosmer telling me that even though research and development had tried, unsuccessfully, for ten years to accomplish this goal, he expected me to get it done within the year that I would be there. I figured he was putting me on, so I went him one better and said, "Why wait that long? I'll deliver it as your Christmas present in ninety days." (I had surmised early on that Hosmer liked people to be as cocky as he was, so I didn't want to let him down.) The fusion step involved reacting caustic with a slurry of sodium benzene sulfonate at temperatures of around 350º C for an hour or more. The reaction vessels were shallow cast iron pots of one thousand- or two thousand-gallon capacity with a scraper agitator to keep solids from caking on the bottom, which was gas-fired externally. We were allowed to take any pots out of service for experimental purposes so long as by the end of the month we could meet company production quotas. Essentially, we selected one pot for experimenting and kept the other pots operating at overcapacity just enough to make-up the difference.

After studying a number of reports on previous (unsuccessful) attempts over the years, I learned at least what did not work. I then spent several days just visiting and asking questions of the plant foremen and operating laborers (most of whom had very little, if any, formal education). I benefited more from these discussions with them than I did in studying the previous technical reports. These operators, and more particularly the shift foremen, supplied me with the practical-experience factor, which I really needed to develop a rational solution.

To make a long story short, I proposed converting the existing batch pots to continuous stirred pots by replacing the scraper agitator with a turbine agitator, installing proportional flow control for the two feed streams, (one a nasty slurry and the other hot caustic) and devising a finishing section by an appropriately placed baffle in the pot to increase the yield on caustic from 99.3 percent to 99.9 percent, which simultaneously solved a serious air pollution problem by virtually eliminating sodium hydroxide from the vapors being vented into the atmosphere. At the same time, by running the pots continuously the daily throughput capacity was almost doubled so that we could take half the pots out of service. As I had facetiously promised, I delivered continuous fusion to Mr. Desmond Hosmer on Christmas Eve of 1952.

BOHNING: What then?

SLIEPCEVICH: It didn't take long for Monsanto to decide to proceed full-speed with converting the other fusion pots to continuous operation. This decision freed up a major portion of my time since engineering and maintenance were going to do the job although I continued to supervise it. The payout time on the complete conversion was less than one year.

BOHNING: Were you given any additional assignments?

SLIEPCEVICH: Yes, in fact, immediately. Hosmer told me that the market for their chlorobenzenes exceeded their capacity to produce, and he wanted me to look at it. After looking over the facility (one of Monsanto's largest production departments) and making some back of the envelope calculations, I concluded that the battery of continuous chlorination reactors was being underutilized. My first suggestion was to chlorinate faster, but I was warned that had been tried. The result was that the products of the reaction had set up like concrete, which necessitated replacing the reactors. After giving more thought to the problem and running some beaker-type tests in a hood in the plant chemistry laboratory, I concluded that it wasn't the chlorination rate *per se*, but the control of the reaction temperature that was critical. By installing temperature controls and adding more cooling water-coil capacity to each reactor, it was possible to more than double the production rate. Actually, when we completed the conversion on all of the reactors (I recall ten), the increased production turned out to be 2.77 times with a minimal capital investment that paid out in a matter of months. In addition, I was able to develop a new and cheaper method for converting the final product from the reactor in a form which did not have the objectionable characteristics of irritable dusting and caking. (With respect to the latter, I got my first practical opportunity to control "crystal habit" by altering the nature of the surfaces.)

My third and last major assignment was to collaborate with the Organic Division's Research and Engineering Department on economic evaluations and comparisons of several new processes for the manufacture of chemicals that Monsanto was considering for production. The phase of this project, which was under my supervision was completed before I left in September 1953.

BOHNING: It appears that your year at Monsanto was much more related to your own thesis in equipment design and chemical kinetics than it was to all the time you had invested in aerosols.

SLIEPCEVICH: In reality, I did not escape aerosols entirely while I was at Monsanto because I had to cure a severe problem of caustic entrainment from the fusion pots. Otherwise, I agree with you; I had some first class experience in chemical reaction kinetics and control. Most of my other exposure to this subject was through graduate research. By the time I went to Monsanto in 1952, I had had three students—from whom I had learned a lot—complete Ph.D.s in chemical kinetics and catalysis—all in continuous tubular reactors. In addition, I had two more doctoral candidates in this field who completed their doctoral work shortly after I returned from Monsanto. Five students can teach a slow learner like me a lot and in a hurry.

BOHNING: Weren't you also involved in some biomedical research about this time?

SLIEPCEVICH: Yes, I was.

BOHNING: What prompted you to go into that field and what was the nature of your research?

SLIEPCEVICH: I had several close friends in medical school. (Besides, I had harbored for many years a latent desire to become a medical doctor.) I was invited to attend some of their research seminars. I discovered early on that I was able to contribute (in a very small way, of course) to their discussions. As a result, I found myself perusing textbooks—out of curiosity and interest—in physiology and anatomy (15, 16).

When Philip E. Bocquet completed his master's degree, he was unsettled whether he wanted to continue working toward a Ph.D.; his biggest deterrent was that he couldn't find a thesis topic that would abide his interest. In those days, and throughout my entire academic career, I always insisted that the student select his topic and define its scope. All I did I was suggest a number of possible areas that might be fruitful—irrespective of whether I had on-going work in the field—and to establish practical limits on the scope. After going through several topics and not getting any reaction from Phil, when I mentioned medical research, his eyes lit up and I knew right then he was hooked. With that, I sent Phil over to the physiology department to talk with a brilliant young professor, David F. Bohr. When Phil returned to my office, he announced that he wanted to work on the development of a technique to measure blood flow *in vivo* in remote regions of the body, particularly the heart, by utilizing the streaming potential concept. Furthermore, Dr. Bohr had offered to provide space in his laboratory and to share expenses from a Public Health Grant.

I believe I was as excited about this proposal as Phil was, but I had one major concern: How could I get approval from the chemical engineering department for this offbeat subject? I recalled the difficulty I had experienced in getting departmental approval of Gumprecht's thesis in light scattering by particulates. Some of the faculty were very opposed to light scattering because they regarded it as physics—not chemical engineering. I think the only reason it got through was that the department chairman himself, G. G. Brown, as a result of his work on screening smokes, was interested in particle size measurements. Of course, the fact that I also had a fat contract on particles, from which 20 percent of the overheads collected by the research institute reverted back to the department that generated the project, did not hinder the approval.

Phil's project was similarly blessed from having at least one member of the faculty (besides me, of course) interested. Professor Donald L. Katz had done some pioneering work on streaming potential relative to his work on petroleum reservoirs and porous media. However, to be safe, when Phil and I prepared his prospective for submission to the department for approval, we pitched it as a fundamental fluid flow study. No problem! (17)

BOHNING: Didn't you have another student about this time working on a biomedical project?

SLIEPCEVICH: You obviously are thinking of William "Ernie" Henderson. Ernie was one of those rarities who from kindergarten through graduate college earned nothing but As, with relatively little effort on his part. His bachelor's degree in chemical engineering was from the University of Illinois; upon graduation he picked Michigan for continuing his studies. Again, as I did with Phil Bocquet, I gave Ernie my "shopping list" of thesis topics. What caught his fancy was the possibility of developing a flat-plate (as contrasted to tubular) dialysis unit that could be applied as an artificial kidney. Again we were faced with getting this thesis topic approved. Our strategy in this case was to emphasize ultrafiltration, not dialysis (which would have blown our cover) through cellophane membranes. After all, every self-respecting, God-fearing chemical engineer was interested in filtration. Ernie was successful in developing an artificial kidney, which the medical school—with our assistance—used for years both as a dialysis unit for humans and as a clinical apparatus for separations of blood constituents (18).

Probably the most gratifying aspects of both Bocquet's and Henderson's work was our participation with the medical doctors in their early tests on humans. Our function was essentially to train them and their nurses in the operation of these units. I still have vivid memories of the miracles (in those days) that I witnessed in connection with the use of our equipment.

BOHNING: Up to this point, have we overlooked anything that you would like to recall?

SLIEPCEVICH: One item comes to mind that I shall never forget. When Phil Bocquet was to have his final oral examination, Professor Donald L. Katz, who was on the committee, got after Phil. Professor Katz questioned Phil about some of his theoretical analysis and each time Phil would respond by putting a differential equation (in vector notation) on the board, but that's exactly what Katz was questioning him about. Bocquet and Katz went back and forth with the same result until Phil, in sheer frustration, said, "Well, you've got to understand a little bit about mathematics before we can discuss this matter any further." If it had been anybody else but Katz (who was anything but a vindictive individual) I figured Phil's goose would have been cooked on the oral examination. However, Katz calmly (and with a smile on his face) responded, "Phil, I never claimed to be the smartest guy in the world, but I believe I'm intelligent enough that if <u>you</u> really understood what you were doing you could explain it to me so that I could understand it. Now, you try it once more and give me a concept, but I don't want to see another equation on the board." Phil finally got the message and even surprised himself. I, myself, really learned a lesson, which I never forgot in life. It also reminded me of the lesson I learned from G. G. Brown: "Mathematics should only be used as a tool, not a crutch."

BOHNING: Have we overlooked any other interesting projects that you had up to this point that you haven't mentioned thus far?

SLIEPCEVICH: You mean some actual consulting projects that I had between 1944 and 1954 in addition to the research projects elaborated above? Well, I researched why beer bottles sometimes explode while just sitting on a shelf at room temperature, the best fuel injection system for aircraft gas turbines, the bursting pressure on rocket tubes, how a utility power station should be cited to minimize environmental (air) impact on adjoining population centers—there were so many interesting and stimulating projects. Most of these projects had short-time fuses, and each involved part-time work by me over a period of a few months. Admittedly the specific subject areas represent a wide variety of disciplines, but on the other hand they typify the great breadth of engineering. Believe it or not, I found most of these projects as exciting and satisfying as my "longer-hair" R & D projects. They were all problems that required real time solutions. None of this classic "more work remains to be done!"

BOHNING: Are we ready to leave Michigan?

SLIEPCEVICH: Almost! When I returned to Ann Arbor from Monsanto in September 1953, I was retained by Monsanto as a consultant. My contract required that I spend at least one day per month for the following year in St. Louis and to be available by telephone in between. My principal responsibility was to oversee the conversion of the fusion pots to continuous operation and the process modifications in the chlorobenzene plant based on the work I had done while I was there the preceding year.

In the spring of 1954, out of the clear blue sky, I got a telephone call from Continental Oil Company in Ponca City, Oklahoma, inviting me to visit their Research and Development Division to discuss a possible consulting arrangement. What had happened was that two of my former doctoral students, John M. Dew and Phil Bocquet, had gone to work for CONOCO's petroleum production research division. At a research meeting they learned that CONOCO had decided to form a Petrochemical Research Division, and to build quite extensive laboratories to support this mission. Since much of this research would involve chemical reactors at high pressure and temperature, an area in which CONOCO was short-staffed at that time, John Dew and Phil suggested to the Manager of Research and Development, Ed Baker, that he should contact me. The result was that I made my first visit to Ponca City on June 8, 1954, which initiated a fascinating and delightful relationship that I would have with them for more than a decade. Basically my consulting contract was open-ended in that I was authorized to spend up to ninety days per year (without any additional specific authorization) with the minimum stipulation that I would make at least one two-day visit per month to Ponca City, unless we agreed otherwise.

Exactly one month later, July 8, 1954, during my second visit to Ponca City, the most unexpected, yet significant, event in my entire professional career took place. While I was visiting with the Manager of Research, Ed Baker, he mentioned that he had to go to a meeting with L. M. Vickery, Manager of Engineering, and other technical personnel. He invited me to go with him. The purpose of the meeting was to hear a presentation from an engineer named John A. Murphy, who was the technical advisor to Mr. E. F. Battson, senior vice president of Continental Oil. Battson and Murphy were essentially a staff of two whose sole function was to be on the lookout for new business ventures, acquisitions and joint undertakings for CONOCO. Mr. Murphy advised the group that Mr. William Wood Prince, the owner of Chicago Stockyards and a major stockholder in the meat packing firm of Armour & Company, had approached Mr. Battson on joining him in a venture to liquefy natural gas along the Gulf Coast and barge it inland via the Mississippi River and its confluent rivers to cities and industries located thereon. As another stroke of coincidence, it turned out that I was the only individual in that Ponca City meeting that was familiar with liquefaction of natural gas. I had previously made a thorough study of the ill-fated Cleveland liquefied natural gas [LNG] plant which after eight months of operation was, on October 20, 1944, destroyed by fire and explosion, taking the lives of one hundred twenty-eight people. My interest in this plant was initially driven by my desire to include cryogenic processing in my graduate course in thermodynamics. Liquefaction of natural gas provided an excellent practical example for comparing liquefaction by expansion cycles versus cascade cycles. In the process of developing problems to assign to my students, I simultaneously became quite versed in behavior of materials at cryogenic temperatures and equipment design. As a result before the meeting adjourned, John Murphy asked me to allocate as much time as I could find to work with him as a consultant. At that time, Mr. Ed Baker (who really was my boss for CONOCO) told Mr. Murphy that he had no objections just so long as I continued to perform my services for the Petrochemical Research Department. In looking back that day, July 8, 1954, was the birth of the most significant event in my entire professional career.

My duties in CONOCO and the people with whom I worked were beyond compare. We were building a Petrochemical Research Laboratory, which involved detailed design and special fabrication of practically every item of equipment, except for some off-the-shelf instrumentation and analytical tools. As fast as we could, we were putting these facilities into use for synthesizing new products or improving our old ones. One of the early accomplishments was a process for producing α-olefins and alcohols—as precursors for the manufacture of detergents—based on Ziegler polymerization chemistry.

CONSTOCK (the acronym for <u>Con</u>tential Oil Co. and Chicago <u>Stock</u>yards) was a different ball game. We were the only group in the world working on the liquefaction and marine transportation of natural gas. Every step of the way we encountered new and heretofore unsolved technical problems, particularly in the design of ocean-going tankers. Little did I dream back in 1939 when my college roommate elected to major in naval architecture that twenty years later I would be directly involved—hands-on, and responsible as Manager of Research and Engineering for Constock Liquid Methane Corporation—in the design, construction and launching of a novel marine tanker that the world's tanker architects and builders—almost without exception—thought could not be done. Rather than dismay us, this negative prognostication simply charged our batteries. Our faith and determination paid-off (19). The day the Methane Pioneer docked at Canvey Island, England on February 20, 1959, was the start of an international commerce, which within four decades was to become a business valued in the hundreds of billion dollars.

BOHNING: Was this intensive involvement with CONOCO the reason you decided to leave Michigan and migrate to Oklahoma?

SLIEPCEVICH: Not really although it probably biased my decision a little.

BOHNING: What then caused you to leave since you seemed to have had the best of all worlds at Michigan?

SLIEPCEVICH: There were a number of factors that started to emerge after I returned from the year's leave with Monsanto. First of all, I had been based at Michigan since 1939 and I was hooked on Ann Arbor. My year at Monsanto, however, made me aware that life could be just as enjoyable elsewhere. While being away from the University for a year, I had a chance to reflect some and also to project. In reality, I had, up to this point, never made a lifetime commitment to academics; in fact, in the back of my mind I was still harboring the idea that I would go into industry as soon as I could clear my desk—so to speak—at the University. On the other hand I enjoyed teaching, working with my graduate students, and doing research and consulting work. Michigan, at that time, was the ideal place to be on all of those counts except for one attribute, foresight.

By the end of World War II, it had become abundantly clear that engineering curricula and programs across the country had to undergo major changes to meet the new challenges and demands that were surfacing almost daily in technology. I had tried as a junior staff member to infuse some of this new thinking (I had had an unusually good exposure from the nature of the work I had done during the war and thereafter) but I had virtually no success. Our department was quite smug about the fact that ours was the best in the business, which really was not an overstatement. Why should changes be instituted? There was only one direction that would lead to: down! It was evident that with that kind of attitude, our department, and possibly even the entire engineering school, was heading for a collapse.

Basically, because of these, and other undisclosed philosophical differences I was having with my superiors, I decided—not long after I returned from Monsanto—to move on. By then I had had seven doctoral students complete their degrees between 1950 and 1953 and I had eight more in process who were far enough along to complete their work within another year or so. Although I had several other students entering my pipeline, they were not far enough along that if I deserted them it would have a large impact. Therefore, I targeted the spring of 1955 for my departure date.

My next decision was to give academia another shot in a new environment. I had almost reached the conviction that if I could keep my finger in academics and in industry via consulting, I would have the best of all worlds. However, I had one complication about academics; I did not want to go from a stagnant environment to another one in which I would have to accept the status quo. Although I never had any aspirations to move into academic administration—in fact I had an aversion toward it—I realized that I would have to assume some position of authority in order to implement program changes.

Simultaneously, I had a personal score to settle. I had a quirk in my conscience that kept whispering to me that the early successes I enjoyed in my academic career were not because of me but because of my affiliation with the best department in the world. In fact, I had doubts whether I could even survive at another institution.

Without some misgivings, I decided to test the waters quietly in the spring of 1954. Much to my surprise, I discovered many doors open for me, both in academics and industry. My bottom line for academics was that I would have to find an institution that was genuinely dedicated to change and that could benefit measurably from my presence.

I found what I was looking for in the University of Oklahoma. The president there, Dr. George L. Cross, made the difference. He convinced me that he was determined to make either some major changes in the College of Engineering, or to abolish it altogether. In fact, he was under considerable pressure from the state legislature to collapse petroleum engineering into geology and get rid of the rest of the engineering schools. This proposed demise had a large base of support from within the university because it smacked of making funds presently earmarked for engineering available to the other departments. Dr. Cross impressed me as a forthright individual who could not be pushed around, as a man of integrity who expected the same in return, and as a person without prejudices who could see things as they really are rather than what he might want them to be. Briefly, after working closely with him for more than a decade, my first impressions of him were not only right on but also understated.

In addition, I had some good connections at the University of Oklahoma. My collegiate roommate at Michigan, Carl D. Riggs, had been on the Zoology faculty for several years, and another close friend, Peter Elliott, was an assistant football coach under the great Bud Wilkinson. One of my doctoral students at Michigan, John M. Dew, who was a native Oklahoman and an alumnus of University of Oklahoma, tipped them off that I might be in the hunt.

BOHNING: When did you leave Michigan?

SLIEPCEVICH: I left Michigan in January 1955 without harboring any regrets then, or ever since then. Michigan had been good to me and for me. It’s not often that a young professor has the opportunity to undertake projects, which put him in direct contact with noted scientists; I had occasions to meet with many of them. I particularly recall (as I mentioned before) a Nobel laureate, Peter Debye, was very supportive of our work in light scattering. Nobel laureate Percy W. Bridgman encouraged me in my work on predicting failure in tubes under high pressure and temperature and my approach to the open system of thermodynamics. Our work on attenuation of thermal radiation by dense dispersion, was of particular interest to another Nobel laureate, the brilliant astrophysicist, S. Chandrasekhar. However I never lost sight of the possibility that it really wasn't my work that was attracting this attention; but rather the perception that since it was being done at the University of Michigan, it had to be worthy. In contrast to Michigan, it wasn't long after I arrived in Oklahoma that the prominent atmosphere geophysicist, Lloyd V. Berkner, in a public address referred to this section of the country as the "intellectual wastelands." With two words he had condemned the entire educational process in this area. Since Berkner had been president of a consortium of eastern universities [Associated Universities, Inc.] and had headed U.S. scientific delegations to international conferences, he very likely was mouthing a perception shared by his associates.

By the first of February I was on board at the University of Oklahoma as Chairman and Professor of Chemical Engineering. Professor Richard L. Huntington, the real daddy of chemical engineering at OU, was about all that was left on the staff. Professor Lyle F. Albright had already accepted a position at Purdue beginning in that summer and Professor L. [Laurence] S. Reid had taken an extended leave to pursue his consulting work. Two doctoral candidates on the verge of completing their degrees, Joe Snider in chemistry and Francis Mark Townsend in chemical engineering, were available for part-time teaching that spring. After that, the staff situation looked grim because that just left Huntington and me to cover the rest of the undergraduate and graduate programs. Our student enrollment was comparable (more than half) to Michigan; in my first semester we had about fifty seniors in process. We also had about a dozen graduating seniors who had expressed an interest in doing a master's degree.

In my first discussion with President Cross he asked me to get the chemical engineering department—particularly recruiting more faculty—underway as soon as possible, and then he wanted me to turn my attention to the entire college of engineering. He created a new position, associate dean of engineering, who was to report directly to him and to be responsible for all curricula—both undergraduate and graduate—all faculty recruitment, and all research. The dean of engineering would retain responsibility for all of the routine administrative matters, student placement upon graduation, student records, and student organizations. President Cross wanted me to assume this added title of "Associate Dean" in time for the fall semester of 1955. Although I had a strong distaste for academic administration I realized I needed a "title" to get my job done. Besides, since most of the baggage that goes with administrative titles had been reassigned, I decided to give it my best shot.

Before I accepted the OU offer I advised Dr. Cross of my deep involvement in consulting for Continental Oil Company on petrochemicals and on liquefied natural gas and that I was not willing to give up either of them under any circumstances. His response was that he could foresee many beneficial aspects of this work accruing to the University, that he was basically in favor of faculty doing meaningful consulting work that would enhance their capabilities, and that as long as he was satisfied with the progress of my work at the University (about which he could stay abreast because I would be reporting to him) he had no problem with my desires. In return, I told him I would not leave him high and dry in the midst of an academic program development, and so long as he wanted me to remain at the University, I would. Since Dr. Cross had about twelve more years before his retirement, I realized the magnitude of my commitment; heretofore, I had rarely ever planned even months in advance other than for meeting job deadlines.

I realized that the first priority would have to be faculty recruitment. By the forthcoming fall semester there would only be two active Faculty in the entire College of Engineering (out of seventy-five) with doctoral degrees, Dr. Huntington and myself in chemical engineering. I was fully aware that to attract young Ph.D.s (we couldn't afford senior staff) we had to have a graduate program in place. However, the graduate college of the University would not authorize a doctoral program for a department having less than three full-time Ph.D.s on the staff. So here we were faced with the "chicken and egg" proposition, except for chemical engineering. Shortly after I arrived at OU I had convinced Mark Townsend who had just completed his Ph.D., while helping with our teaching load, to accept a tenure-track offer. That gave us three Ph.D.s, but all of them were in chemical engineering.

While at Michigan, both as a student and as a teacher, I realized how parochial or hidebound the academic disciplines really were. I had thought (or hoped) that a Doctor of Philosophy did not signify or require a narrow area of specialization in one discipline. Rather, I felt it should signify a breadth of knowledge in the natural (or moral or metaphysical—as the case might be) sciences. I recalled my early cogitations about completing my doctoral thesis under a mathematician, or a physicist or a chemist but was turned-off when I realized I would have to run completely through another obstacle course to satisfy stagnated departmental requirements. I also recalled the subterfuge (I was accused of that) I had to invoke in order to clear theses for my doctoral students related to light scattering, blood flow and kidney dialysis. Putting all these parameters into a pot, which I stirred vigorously, I conceived of a Ph.D. program in the Engineering Sciences (20) that was not affiliated with any particular department but was a college wide-program. I proposed that this program be administered by one faculty representative from each of chemistry, physics and mathematics and three representatives—recent hires with Ph.D.s—from the engineering college, besides myself as chairman of the committee (since I was the associate dean specifically responsible to the University Administration).

In the midst of my daily efforts to meet and balance my obligations to the University of Oklahoma and to my consulting clients, I somehow managed to maintain my sanity to allow the most important event of my adult life to transpire. On October 21, 1955 I married Cleo L. Whorton of Pryor (about one hundred sixty miles northeast of Norman). I had met Cleo three years earlier while I was working at Monsanto, and she was living in Belleville, Illinois. Most of my acquaintances immediately found a correlation between my leaving Michigan and moving to Oklahoma. On the contrary, our marriage would have taken place even if I had elected to stay in Michigan or to go elsewhere. The fact that Cleo has remained with me forty-three years (as of this writing) is a tribute to her endurance. For the first twenty-five years of our marriage I seemed to be consumed in my work, around the clock, seven days a week, including frequent travel for both consulting and a plethora of national academically related committees. Occasionally Cleo accompanied me, and whenever appropriate we included some of her young nieces and nephews (of which there were seventeen). Since Cleo's degree was in biology, she volunteered to revitalize the entomology museum in zoology and to assist in the herbarium in botany at the University. She also volunteered her service as a frequent substitute teacher in the local school system. Her hobby was, and still is, gardening. She always managed to stay busy, particularly when she was preparing a lengthy treatise on a menu-cookbook.

While we were in the process of getting the Ph.D. program activated, we started a wholesale revision of the curricula in engineering. Based on several studies under the aegis of the American Society for Engineering Education and the Engineers' Council for Professional Development it became abundantly clear what requirements had to be met by any engineering department seeking national accreditation. It was immediately transparent that none of our programs in engineering at that time could past muster. Therefore, we had to proceed without delay. There was not any hope of revising each major, independently, because we neither had the faculty nor the financial resources to execute such a move. We also were saddled with the problem of expanding graduate courses in every department to accommodate our projections. To kill two birds with one stone, we introduced a core, undergraduate curricula, which made about seventy-five of the curricula common to all engineering regardless of specialization. This core, of itself, guaranteed that every requirement for accreditation was met regardless of field of specialization.

In addition we—not by choice but by necessity—went to large lecture sections for the core courses supported by smaller recitation sections as needed. In this manner we could utilize our most knowledgeable faculty in a particular discipline. For example, one of the core courses was thermodynamics. Only two departments at that time had individuals who were particularly qualified to teach the subject. Therefore, they were put in charge of the thermodynamics core course, and they presented the lecture to classes of two hundred or more students majoring in the various disciplines of engineering. By playing this core curriculum gimmick to the hilt we were able to salvage enough teaching load to offer graduate courses in all of the disciplines.

BOHNING: How did the departments outside engineering react to these changes?

SLIEPCEVICH: Both our Ph.D. programs in the engineering sciences and our core curriculum in engineering were favorably received across the campus. Where previously the other colleges in the University had been standing-by and anxiously awaiting the eventual demise of the College of Engineering so that they could expropriate engineering funds for their own use, now engineering was receiving excellent cooperation across the board. I remember particularly how excited the humanities faculty was when we asked them to devise a program to satisfy the core requirements in this area. In the final analysis, and most importantly of all, the ultimate benefactors were the students; it was evident in their morale.

BOHNING: Was your doctoral program patterned after any program elsewhere?

SLIEPCEVICH: Not to my knowledge, in fact not even remotely, since we had other (than those mentioned previously) major departures from the conventional. In most institutions, students were required to pass special, comprehensive, written examinations covering their undergraduate and graduate work. In chemical engineering Michigan was using four, four-hour undergraduate examinations one week and another four, four-hour graduate examinations the following week. Although all of the faculty there extolled these tests with a passion I could not bring myself to believe that they served any useful purpose other than a disciplinary obstacle.

In place of these examinations, at Oklahoma we set forth a requirement that every candidate had to complete a study on two major comprehensive problems in fields completely unrelated to each other and also to the subsequent dissertation. The problems had to originate with the student, and they had to be original in nature. One problem was to be directed towards a basic research study complete with a thorough literature survey, precise statement of the problem, detailed outline of the experimental attack, data to be taken (and in some cases even carrying out the experiments), detailed development of the theory associated with the problem, and a probable method for interpreting and correlating the data. The other special problem had to be pragmatic and directed to the engineering design of a new process or device, substantiated by detailed economics. Both of these problems of course were to be written and defended on separate occasions by an oral examination before a faculty appointed by the graduate dean. The results were far more than I anticipated. Many of these special problems ended up as publications in journals or as successful proposals for sponsored research and development.

I had one other hang up. I also thought that the two-language requirement at Michigan (common to most schools) had become an outdated farce. I was all for scrapping it—and anything else that served only as an obstacle—but the graduate college at OU wasn't ready to concede—although in years to come it would.

BOHNING: Did this overhaul of both the undergraduate and graduate programs in engineering result in new academic programs being introduced?

SLIEPCEVICH: At least indirectly. In l959, I relinquished the chairmanship of chemical engineering to become chairman of General Engineering which was restructured—purely for administrative reasons—to oversee the common or core courses in engineering which we did not want to be identified specifically with the existing, traditional departments. Truthfully we felt this would erase the stigma of "service" courses, which had acquired an unpopular connotation across the country in engineering.

BOHNING: Did this move facilitate your carrying-out your duties as associate dean?

SLIEPCEVICH: In more ways than one!

BOHNING: How so?

SLIEPCEVICH: First of all it gave an administrative home to university-wide assets such as our newly-acquired nuclear reactor laboratory and academic computer facilities. By far, however, it provided the "device" (this word reflects to some degree the machinations that were implored) to introduce a program in meteorology. The Director of the University of Oklahoma Research Institute [OURI], Verne C. Kennedy, was always on the lookout for new government sources of funding for research and concluded that the discipline of atmospheric sciences was a sure winner. One example was the announcement from the National Science Foundation [NSF] stating that they were going to establish the National Center for Atmospheric Research [NCAR] and that proposals were being solicited. During the late 1940s, Kennedy had worked with me at Michigan on the V-2 rocket program for upper atmospheric winds and turbulence and he, like me, had acquired enough fascination for this science to be dangerous. [laughter]

Both Kennedy and I were convinced that our chances for getting this Center were virtually nil if we did not have an academic program specializing in this discipline. To make a long story short we sold—not in terms of money but solely on opportunity to build—Dr. Walter Saucier of Texas A & M to join us. Saucier's book on meteorology had wide adoptions so he was quite well known in the field. In addition, he was exceptionally well-funded—mostly by NSF—enough to support one postdoctoral student, the brilliant Dr. Yoshi Sasaki from Japan, and four doctoral candidates. Saucier was fascinated with the location of Norman in "Tornado Alley" and had great visions of how he could enrich his academic program with the proximate national severe storms laboratories in additional to NCAR. Within a matter of a few months, Saucier had moved his entire operation, students, equipment and funding, to Norman. He established residence in a dilapidated, army barracks on North Campus (formerly a Navy base).

We didn't succeed on NCAR, but even our greatest expectations for meteorology have not only been realized but also significantly exceeded. Today this academic program is the showpiece of the University, possibly even surpassing our noted History of Science collection as our intellectual diva. Norman is now the home for ten weather and climate entities with state, federal and university connections consisting of about seven hundred researchers, forecasters and students, which dump more than forty-five million dollars annually into the local economy.

BOHNING: Did you have particular difficulty in recruiting new faculty for engineering?

SLIEPCEVICH: Not anywhere as much as I had expected. Even though we were not generally competitive financially, we sold prospects on the opportunity to build literally from scratch, to implement their ideas and to participate in rescuing a culture from Berkner's intellectual wastelands. Surprisingly we were finding takers who had matriculated at the more prominent institutions and who after a few years of industrial experience were ready to return to academics.

BOHNING: You were now about five years removed from Michigan. Did you retain any contacts with Michigan or have any thoughts of returning there to work?

SLIEPCEVICH: Really, no. I left Michigan in January 1955. Late that fall I received a telephone call from Dean G. G. Brown at Michigan asking me if I was interested in returning to Michigan beginning with the next academic year, fall of 1956. His offer was unusually generous and he was reserving a place for me in the budget, which he was in the process of preparing. The following spring I received the written offer from the Department Chairman, D. L. Katz, which I respectfully declined. Nevertheless, through the 1950s and the 1960s I made occasional trips to Ann Arbor since I continued to collaborate with D. L. Katz and S. W. Churchill. They were not only my professional colleagues on several consulting projects but also my close friends.

BOHNING: With all your academic administrative responsibilities and continuing consulting activities were you able to maintain any level of graduate student research?

SLIEPCEVICH: In some respects, it even thrived. During my first summer in Norman, a prospective graduate student walked into my office, Robert J. Fanning. He was then working for Phillips in Borger, Texas, but he was debating his return to school for a Ph.D. Since he had been spending most of his time at Phillips working on instrumentation problems, and had grown very interested in this area, he was looking for a school where he could pursue related study. I told him I shared his interest based on my instrumentation and control experiences with the V-2 rocket program and my work at Monsanto, but I was never able to find adequate student interest at Michigan to initiate any thesis research. All I could offer Fanning at that time was a guarantee of a graduate fellowship. I also promised to try to find more substantial funding for his thesis research. Fanning arrived for the fall semester of 1955. During that academic year we worked diligently to develop a proposal for NSF which laid out a long-range program in considerable detail for studying system identification and dynamic response characteristics of process equipment. Our proposal work was somewhat unique in that we were going to combine our experimental studies on process systems with mathematical formulation of the dynamics. I was a strong believer that a doctoral dissertation had to include original experimental work.

NSF surprised us by picking up the whole tab for the first three years. It was my second NSF grant; my first one came at Michigan—just about the time I was leaving—to support my work in light scattering; that one was apparently only the second grant that the newly-established NSF had funded. In addition to NSF, Phillips Petroleum Company continued to support this program for several years until I relinquished it to one of our new faculty recruits, Dr. Michael McGuire who had completed a thesis in this field at Princeton under the renowned Professor Leon Lapidus.

During my tenure as director of this program (through 1965), nine students completed their Ph.D.s in chemical engineering in system identification and process dynamics and a comparable number (with majors in the physical sciences or other divisions of engineering). With full credit to the students, all of these theses were quite impressive. By way of example I cite Dave Haskins' thesis on invariance principle of control for chemical processes as indicative of the level to which we had progressed (21).

BOHNING: What about your work in high pressure?

SLIEPCEVICH: That, of course was my first love and consequently it was the first area I attempted to establish when I arrived in Oklahoma. Again my major benefactor was the National Science Foundation; in addition I had support from the Office of Naval Research, Continental Oil Company and Autoclave Engineers. With respect to the last-named company, I'd like to digress. In 1946 while I was working on my doctoral thesis, I purchased a high-pressure valve from a newly formed company, Autoclave Engineers. That purchase resulted in my meeting the founder and owner, Fred Gasche, who—out of curiosity—visited me in Ann Arbor. From that day-forward we established a very close personal friendship which initiated an uninterrupted consulting relationship I had with Autoclave for forty-five years; the last thirty included my serving on the Board of Directors. I observed the company business grow from about ten thousand dollars the first year to approaching one hundred million dollars my last year with them. Fred Gasche taught me a lot more about high-pressure design than I did him. He remains as one of the most remarkable individuals and treasured friends that I have ever known.

BOHNING: Were there any laboratory facilities for high-pressure research available at OU when you arrived?

SLIEPCEVICH: Laboratory space for research was very limited, particularly in chemical engineering. Since NSF was supporting my research quite substantially, they gave the University of Oklahoma a facilities grant to create more research space. To accommodate my work, I needed several thousand square feet, which simply were not available anywhere on campus. My only alternative was to convert one of the old barracks buildings on our North Campus (three miles from main campus) to house the barricaded cells for the high pressure studies and the pilot plant for the process dynamics work.

BOHNING: Didn't this removed location inconvenience you and your students?

SLIEPCEVICH: At first it did give us some concern, but I recall a comment that Professor Paul Chenea of Purdue, who was helping me with the stress analysis on the LNG ship's tanks, made upon visiting me at my dilapidated barracks. In attempting to look at the bright side, he commented, "At least you should be able to get a lot of work done being this far removed from the noise level and the Brownian motion on campus." As it turned out, Chenea's wry observation was prophetic; North Campus assured minimum interruptions and distraction and became the site of the largest concentration of academic research at the University.

BOHNING: It still would appear that your activities as associate dean unavoidably entailed a lot of time consuming paperwork. How did you manage?

SLIEPCEVICH: The secret is to surround oneself with competent, trustworthy people to whom you can delegate responsibility and then get out of their way. In January 1962, I was sharing an office suite and secretary with the chairman of our newly-found program in Metallurgical Engineering, Dr. William R. Upthegrove, whom I had recruited for this purpose. Our secretary had resigned so Bill Upthegrove volunteered to go through the standard university procedure for interviewing and hiring replacements. After looking over several applications, Bill picked Mrs. Billy Ann Brown. She had recently moved to Norman with her husband, Bob, who had been a practicing geologist but had decided to undertake graduate studies. They had three lovely, pre-school daughters. Billy Ann had previously earned a degree in business and had excellent qualifications for academic work, particularly since she had been a secretary to the President of Northwestern State College at Alva. I could elaborate endlessly, but I will simply state that she, more than any other co-worker, made the difference. Within a couple of years she became my alter ego. When I formed my private consulting firm, she became my office manager, overseeing all of the office employees, all the business and financial matters, internal auditing and taxes. At the same time she was the principal confidant and mother-in-residence for many of my graduate students and their families. After thirty-six years with me, she still has managed to maintain her sanity.

BOHNING: Didn't you leave academic administration in the early 1960s? Why?

SLIEPCEVICH: Dean Carson reached the mandatory retirement age in 1962 after serving the University commendably over four decades. In anticipation of this event, Dr. Cross and I had some discussion about this matter, and I made it clear to him that I had no interest whatsoever in being considered as Carson's replacement. In fact, I told him that the job of associate dean and the pressures that led to its creation originally were not operative any longer. The graduate program was underway, the undergraduate core curriculum was in place, the sponsored research volume was growing rapidly and all new faculty positions or replacements had been filled with Ph.D.s. We agreed that the new dean should not be fettered and should have the freedom to select his own staff and *modus operandi*.

When the new dean [Eugene Nordby] came on board, I resigned. Since Dr. Cross knew I wanted to divorce myself from all administrative functions, he found the ideal position for me. The University of Oklahoma had established, several years earlier, the position of Research Professor, which by definition excused the designee from serving on any university committees, teaching more than one course of his choosing per semester, and holding any academic administrative position. The research professor was expected to devote full-time to research, and therefore reported to the dean of graduate studies for the University rather than to an academic department. I couldn't have asked for more.

BOHNING: Somewhere in this time frame, you must have initiated the Flame Dynamics Laboratory. How did that evolve?

SLIEPCEVICH: One of my major responsibilities as the principal consultant to CONSTOCK was to assure that we would be able to obtain approvals from all of the regulatory agencies involved by assuring safety in operations. Liquefied natural gas was a very sensitive issue because the Cleveland disaster of 1944 was still vivid in everybody's memory. This issue was further complicated because it involved both land-based and marine agencies all over the world. Another inconsistency was that at one end of the spectrum was concern with the deleterious impact of high temperatures resulting from fires or explosions and on the other end cryogenic temperatures leading to failure in materials of construction or in excessive vapor dispersion.

BOHNING: Wasn't such information available somewhere or from someone?

SLIEPCEVICH: That was my hope, but I soon found out differently. My first pass was to study the literature available and accumulate pertinent data that I could then incorporate into calculations from which I could derive quantitative design specifications. I was truly amazed how much knowledge—even rudimentary—pertaining to fire safety was lacking. Consequently in order to convince regulatory authorities that we knew of which we spoke it was incumbent on us to develop and execute an extensive test and demonstration program because reliance on simple, laboratory tests would not be adequate. With the able assistance of one of Continental's engineers, Carl Schroeder, who had been assigned to work with me on LNG, we devised a program of field tests on a relatively large scale to learn about burning characteristics and extinguishment procedures for LNG and gasoline in earthen pits of up to forty feet in diameter. These tests were ultimately used to demonstrate to regulatory officials from all over the world that we had an adequate grasp of the safety precautions that had to be taken (22).

Since these field tests exposed how much fundamental understanding was lacking on such a vital topic (fire losses—lives and property—have been aptly described as an international disaster), I conceived a program to develop a working knowledge of these problem areas by a combination of laboratory experimentation and analytical interpretation. In revealing my plans to the recognized (including the self-propagated variety) experts in the field, I was promptly shot down. For example, my visions for quantifying the effect of wind on flames was greeted with the conviction from an acclaimed fluid mechanicist on fire research that, "this process was fraught with so many variables it could never be solved." With the ultimate authority on radiative heat transfer from flames, I was admonished about my contention that the spectral emission characteristics of a flame combined with the spectral absorptivity of the combustible was a dominant parameter in quantifying ignition criteria was not tenable because the effect was at best second order and therefore could be ignored.

Rather than being dissuaded by this reception I became determined to pursue my instincts. The result was my establishment of a Flame Dynamics Laboratory, which over the next two decades would result in nineteen doctoral dissertations, a comparable number of masters theses and over one hundred major reports and journal publications. With respect to the disparaging responses I had endured regarding the effect of wind on flames and spectral characteristics I was unequivocally vindicated (23, 24, 25).

BOHNING: Did you receive external funding for the Flame Dynamics Laboratory?

SLIEPCEVICH: As you probably know, research, which requires relatively expensive laboratory facilities and operating costs, rarely flourishes in a University without outside help.

BOHNING: Was your support from the government or private industry?

SLIEPCEVICH: Both, but eventually the major funding source was the federal government. Our first grant was for twenty-five thousand dollars, spaced over a three-year period, beginning in January 1964, from the National Bureau of Standards Fire Protection division under the directorship of Dr. Alan Robertson. He was aware of our early work on the effect of wind on flames and was sufficiently impressed to initiate the first contact with me. Just about that time, one of my colleagues on the electrical engineering faculty, Professor James Palmer, who had been doing some target analysis studies for the military, and I responded to a request for a proposal from the Chemical Research and Development Laboratory at Edgewood Arsenal on basic concepts in flame weapons. We were a long shot, here, because we were competing against several major corporations who had been doing related work for the military for some time. Here we experienced "ignorance is bliss." The review panel thought our proposal was unique and intriguing. Its novelty was worth a major contract, amounting to seven hundred fifty thousand dollars over three years. In addition to this contract we eventually received a succession of grants (as opposed to contracts) from this same agency to pursue fundamental studies of our choosing on fire related research.

Not long after we completed this flame weapons project, we received another major contract in response to our RFP. This time it was the Department of Transportation on the escape worthiness of vehicles, a major component of which was the flammability of automotive vehicle interiors and fuel systems. This project was initiated in June 1969 and carried through for three years at an annual funding level of about two hundred fifty thousand dollars. This project, like its predecessors, provided much support for graduate students.

BOHNING: How do you rate your activities in the 1960s with your previous experiences?

SLIEPCEVICH: I suppose you can say I experienced my own brand of the turbulent 1960s but in a much different (and hopefully more productive) way.

BOHNING: How did your consulting work fare during this period?

SLIEPCEVICH: As of February 1963 it took a different format. I was getting involved in a lot more work than I could handle myself so I had to employ others to help me. Shortly after we had demonstrated that liquefied natural gas could be transported by tanker, the LNG industry literally exploded around the world. Up through 1960 the only, active participant was CONSTOCK, the joint venture between Continental Oil Company and Union Stockyards of Chicago. Up to this point CONSTOCK had an investment of over twenty five million dollars in the venture. Royal Dutch Shell, wanting to get into the business, bought a forty- percent interest in CONSTOCK whereupon the name of the organization was changed to CONCH. Part of this acquisition agreement contained an understanding that I would continue to serve as a principal consultant during the transition, which involved moving the corporate offices from New York to London. Since this agreement obviously restricted me from working with any other companies on LNG, I requested a release, which was eventually granted in 1963. In anticipation of this event, I decided to form my own consulting engineering firm, which I incorporated as University Engineers, on February 28, 1963.

BOHNING: Did your company restrict its activities to liquefied natural gas?

SLIEPCEVICH: A substantial part of the work in the earlier years was, with much of it related to the design of complete fire protection systems for liquefied natural gas and petrochemical plants. In connection with this work, we became deeply involved in detailed design of storage and send out facilities and siting of terminals.

BOHNING: I gather from your list of publications that 1963 was also the year that you got yourself embroiled in some polemics regarding irreversible thermodynamics.

SLIEPCEVICH: I cringe every time I think of it or somebody brings it up. Dating back to my days at Michigan, shortly after [S. R.] DeGroot's book (26) on irreversible thermodynamics appeared in 1951, I began formulating my own thoughts on the subject. What bothered me was, not this book, but the rash of literature that followed it. Claims of a fourth law (or some other equivalent) of thermodynamics and the necessity for dipping into the realms of microscopic thermodynamics—a paradox in itself—to rescue a theorem of microscopic reversibility left me with pangs of bewilderment dancing through my head. From my viewpoint the theorem was not necessary to justify the equality of the reciprocal relations since I could accomplish the same result simply by invoking the fundamental precept of the second law that the entropy production must be an exact differential. By combining this fact with the phenomenological linear equation, the equivalence of the second order, cross partials (equivalent to the reciprocal relations) follows immediately. I used to discuss very briefly my approach (as well as the other) in my graduate class in thermodynamics and I was careful to emphasize that my ideas were anything but universal. Unfortunately, I let my students talk me into publishing my approach to test the waters (27, 28, 29). My worst nightmares as to what the consequences would be turned out to be pleasant dreams by comparison. I was butchered, cut, quartered, and ground universally. I choose "universally" because those that came forth publicly denounced me, whereas those who agreed with me (and there were some) chose to speak with me only off the record. I'm reasonably thick-skinned, but when some of my critics started to attack me and question even my moral integrity, I almost blew it.

BOHNING: Any other developments during the 1960s?

SLIEPCEVICH: Probably the most interesting one of all was our development of a unique freezing process for desalinating seawater. The impetus came from some early funding problems we had on the flame weapons project. Apparently, during the course of a reorganization of the Army Chemical Research and Development Laboratories in Edgewood, the division responsible for our flame weapons project got lost in the shuffle. As a result, we were told to put a hold on our work for three to six months until the Army could get this oversight resolved. Since I had already assembled a staff of post docs to work on this project, and in doing so I had made a three-year commitment to them, I started looking around desperately for new funding sources. I had some backlog of consulting work in my newly formed company, but it was only enough to take care of my disfranchised staff of five for about three or four months.

I recalled that earlier in the year one of my acquaintances who was working as a process engineer for the Office of Saline Water (OSW) told me that his division would be amenable to supporting some development work on a freezing process providing it was a novel one that hadn't surfaced before. I had done some work on reverse osmosis with one of my graduate students at Michigan so I did have a little familiarity with desalination in general. Since then I had mentally toyed with utilizing the fact that the melting point of ice decreases with increasing pressure for some useful purpose besides cocktails (ice floating on water).

In this state of mind (or frenzy) I began to evolve some thoughts on freeze desalination with cocktails and the pieces of a process began to come together. I decided to bounce these ideas off one of my post docs who could be caught in the financial squeeze if Edgewood didn't get their act together. Hadi [T.] Hashemi was one of my recent doctoral students; he had worked more closely with me on the liquefied natural gas project than any other of my staff. He had a brilliant analytical mind that could visualize and grasp immediately the practical applications. I discussed my ideas with him for about an hour and asked him to give some more thought. I recall leaving him about 6:30 p.m. The next morning, when I arrived at my office at 8:00 a.m., I found some papers on my desk. Hadi had developed a conceptual process flow sheet based on the fact that the melting point of ice increases with pressure, whereas for practically all other substances the reverse is true. After making some more back of the envelope calculations, I was ready to go to the Office of Saline Water.

To their credit, OSW advised me that they could give me some very modest support initially, but in so doing I would lose all proprietary rights to the process. They suggested I seek private support if I wanted to retain possession. My first thought was William Wood Prince, who had initiated the LNG project on his own; I had worked closely with him over the past ten years and I knew he would give me a quick response. Mr. Prince was the only CEO I ever knew who didn't rely on a staff or committee to evaluate new opportunities. He was a committee of one; after all, he was spending his own money. He was a Princeton graduate with a degree in finance, but he had a good conception of the technical world.

The following week I visited with Mr. Prince in Chicago. Fortunately, he was surprisingly familiar with desalination and its potential. Inside a couple of hours he had authorized me to get started immediately. He shunned my first idea to spend about six months and fifty thousand dollars as a first milestone in the development. Instead, he tripled my monetary request and told me to have a go/no go answer within a year.

The old saying, "Necessity is the mother of invention," had entered my life. I was in a likely, forthcoming financial bind and an invention saved the day. In terms of the degree of innovation that was required, this desalination work wins hands down in my experience (30, 31).

BOHNING: How so?

SLIEPCEVICH: Every step in our desalination process required inventing equipment to accomplish transformations that had never been practiced before: a <u>countercurrent</u>, horizontal crystallizer involving two liquid phases, their corresponding equilibrium solid phases, and containing a horizontal agitator that operated close to its cavitation point (we were warned by experts it was impossible); a hydrodynamically-balanced ice washing tower (others had tried this principle and had not succeeded); an energy exchange engine that performed the combined duties of a liquid pump (to 200 atm); an expander which recovered pressure energy from liquid-solid slurries at an efficiency of 99 percent (engineers who saw it perform still insisted it was a myth); an electrostatic coalescer operating at 200 atm using a-c current (rather than dc) in the presence of a colloidal suspension composed of two liquid and one solid phase; and a completely-automated, instrumented panel to operate the plant by itself (30, 31). From the equivalent of test-tube size experiments in the laboratory, we went directly to the design and construction of a demonstration plant in Norman, Oklahoma to produce seventy-five thousand gallons per day of potable water (equivalent to producing two hundred million pounds of product per year) from one hundred fifty thousand gallons per day of sea water. (The product water and concentrated brine were continuously recombined to maintain a seawater source.)

By 1974, we had succeeded in demonstrating a plant that could operate essentially unattended and produce potable water from seawater at a lower cost than any other desalination process available. Unfortunately, we ran into two obstacles. The optimistic projections by international commerce as to the market for desalination did not materialize; in fact it was almost in a period of retrenchment in the mid-1970s. The other obstacle, and the most serious one, was that our plant was so unusual, both in the nature of the processing required and complete novelty of practically all of the equipment—particularly the exchange engines upon which the overall efficiency of separation was totally dependent—scared customers away. By this time, Mr. Prince had invested over three million dollars in the development, and he was—understandingly—not willing to proceed further without a partner who would pickup the financial burden for marketing the process. We had many inquiries, but ultimately no takers. Nobody was willing to take the risk.

The bottom line was, we experienced a huge technical success but a complete marketing failure. By contrast our LNG project, which had its share of difficult technical obstacles to overcome—but not so vexatious as the desalination project—ultimately became an unbelievable marketing success (measured in terms of hundreds of billions of dollars in annual business volume). As Mr. Prince said to me after the desalination project was mothballed, “You win some and you lose some!”

It is incumbent at this point to note on both the LNG and desalination projects that the individual assigned by the sponsor (W. W. Prince) to oversee my work was John A. Murphy. I could dwell endlessly on extolling his virtues such as his level of technical comprehension, business acumen, motivational skills, enthusiastic outlook, boundless energy and compassionate bearing. He was one-of-a-kind; they threw away the mold when they made him. His untimely death in 1978 was a deep, personal loss for me and to all that worked with him.

BOHNING: By this time were you out of high-pressure research?

SLIEPCEVICH: On the contrary, I managed to find some new fish to fry.

Initially we were dedicated to our studies on the partial oxidation of methane at pressures up to 13000 atm and temperatures of 425º C (30) which predictably digressed in less than a decade to an in-vogue study of reaction mechanisms (33). Having the audacity to postulate several hundred reaction pathways, assign numerical valves for kinetic parameters, which may not be known to within several orders of magnitude, and solving the resulting non-linear equations reminds me of an exercise in futility. At best it serves the purpose of a computer looking for a problem to manipulate.

In a separate facility, we had undertaken continuous tubular kinetic studies of reactions over a pressure range of 0.4 to 137 atm (34). The ultimate goal was to obtain a quantitative measure of the effect of pressure on the reaction velocity constant. Another one of our students (35) investigated reactions which simultaneously were thermal (homogenous) and catalytic (heterogeneous).

In addition, R. L. Brown, a graduate student in geology, collaborated with me in 1964 to initiate a program on the hydrogen reduction of pure metals from their solutions. His work was continued by a continuous procession of four master's and four doctoral students in chemical engineering over the period of twenty years (36).

In our high-pressure laboratory we also carried out studies on the rheological properties of materials. This work enjoyed support from the National Science Foundation and the National Aeronautics and Space Administration [NASA]. We designed and built a viscometer for studies up to 15000 atm, and shear rates up to 5 x 10^6 sec^{-1}; these values are representative of the contact pressures and shear rates encountered in many lubrication applications in high-speed machinery.

Related to this rheological program was the facility we designed and built to study creep rates (continuous deformation under load) of frozen soils in connection with some consulting work we were doing for the Army Cold Regions Laboratory on the construction of landing fields for aircraft in the Arctic Circle. This creep equipment was later modified to carry out a rheological study on polymers.

My principal investigator for these rheological and viscoelastic studies was Robert G. Rein. In addition he managed to do an in depth experimental study of fires in enclosures, such as vehicle interiors, and an analytical study of radiation view factors applicable to fires. However, his earliest work with me was in 1963-64 when for one of his special problems (which I described earlier as part of the qualifying examinations for the doctoral program) he undertook an experimental study to quantify the effect of solid state dislocations on catalytic activity and ultimately debunked this prevailing myth (37). I also recall this paper as being selected as the best paper of the year for the *Journal of the Electrochemical Society*. To speak of Dr. Rein as simply versatile (and unorthodox) in work habits would be an understatement; he is a different breed.

BOHNING: I note from your publication list that in the early 1970s you seemed to have turned your attention to the subject of energy.

SLIEPCEVICH: I'm sure I wasn't the only one. The much publicized energy crisis in the United States was in vogue. The long lines at the gasoline pumps were a constant reminder of the Middle East embargo and our vulnerability. My paper with Jerry Lott in 1972, attempted to dispel many erroneous pronouncements not only in the public domain but also among supposedly learned elite (38). Our principal pitch in this paper was to justify the need for making a detailed net-energy analysis before rushing to a decision on energy alternatives. A follow-up paper on this subject of energy was my plea for action, not words (39); its genesis was accidental.

In May 1974 I was asked to substitute for the concluding dinner speaker at the annual gas conditioning conference held at the University of Oklahoma. Laurence Reid, who was the perennial chairman of this conference, called me in desperation. He advised me he had to go to press that very day for the conference and he wanted to list the name of the speaker for the banquet (five days away) and the title of the speech. I asked him what he wanted me to speak on, and when he didn't come up with anything specific, I suggested he pick a title and I would try to speak to it—jokingly of course. A day later a printed program was delivered to me, identifying the title of my speech as, "Twigs, Corn Cobs and Buffalo Chips." I wasn't about to go back to Laurence and plead my case (which he probably thought I would). So I simply assumed he wanted me to talk about energy alternatives. The usual practice in the past had been for the speaker to pick a serious topic of interest to the gas people but to deliver it in a relatively entertaining manner. After a marathon of effort lasting three days and two sleepless nights, with the aid of my trusty slide rule (I'm not joking), and a battery of frantically composed free-hand graphs and tables from which I prepared overheads, I produced what was to become the initial draft of my paper, "Conservation not Conversation" (39).

I had no intention of publishing a paper, but the editor of *Hydrocarbon Processing* who was in the audience kept after me until I produced a draft. Of course, I had to delete the humor and get serious about a very serious subject. That paper changed my entire outlook on the future of civilization. Although I had been aware of the deleterious potential of exponential growth—certainly there had been several, well-documented publications on this subject in the contemporary literature. My comprehension of the forebodings must have been, up to this point, only skin deep because I never felt them until I had slugged (somewhat laboriously—remember the slide rule) through the detailed calculations and graphing exercises. Far from the exhilaration I generally experienced with new realizations, I must confess that I was overcome with a level of concern or hopelessness, which, down deep, has never left me to this day. Even though I realized that the severe consequences would not plague my lifetime, it wasn't a legacy with which I could be content. I now understood what Georgescu Roegan, the 20th century economist without a peer, was feeling when in addressing the energy problem he said, "Alas, mankind would rather die in his penthouse than go back to living in a cave." Needless to say many of my compatriots in the petroleum industry—who had been a major part of my meal ticket for consulting work—concluded I had snapped and had become fuzzyheaded. Conservation was a dirty word to them. As a result, I felt the pinch in my pocketbook—so to speak.

BOHNING: I note from your publications that during the 1980s you had a dozen or so publications that sound more mathematical than empirical. Had you by this time forsaken the laboratory?

SLIEPCEVICH: Maybe "forsaken" isn't the precise word, but I plead guilty to about half of my publications during the 1980s falling in your categorization.

BOHNING: Any explanation for this shift in emphasis?

SLIEPCEVICH: Probably a sign of the times like, "Everybody's doing it so why can't I?" Truthfully it wasn't anything I had planned. In reality, typical of my entire professional career, it just happened when I happened to be there too without any ulterior motive. Nevertheless, there was an evolutionary process involved.

During the early 1960s much of my time was devoted to coming to grips with the LNG safety issues. Of concern to everyone who was contemplating the operation of an LNG facility was the consequence of an inadvertent release of liquid natural gas onto the ground. It was already well known and accepted that the liquid at -160º C, would vaporize rapidly to generate a vapor cloud that could travel downwind great distances. In the process, if the advancing vapors encountered an ignition source at the point where the concentration of gas was within its flammable range (5 to 15 percent) the vapors would ignite and burn back to the source where a major fire could result.

Because at that time we had very little quantitative information on LNG boiling phenomena, we initiated a graduate research program to obtain the pertinent data. In specially-designed equipment we were able to measure nucleate and film boiling of methane and other light hydrocarbons between atmospheric pressure and critical pressure. Of course, by definition boiling ceases at the critical point. Efton Park initiated this program as his doctoral thesis and was followed by C. T. Sciance who did a comprehensive and commendable study (38). His data put us in a better position to undertake a mathematical analysis of the vapor dispersion problem. H. T. Hashemi had originally developed a methodology for area source dispersion in an earlier consulting project that we had on the dispersion of ammonia vapors from a spil, which we presented at an American Gas Association Conference (41).

BOHNING: So far you have talked about LNG vapor dispersions in the atmosphere igniting, but it seems you have deliberately avoided the word explosion. As I recall from living in Cleveland at the time of the Cleveland fire, there were also a series of explosions that caused considerable damage.

SLIEPCEVICH: You are absolutely correct, but I have to qualify your inference. Natural gas vapors cannot explode in the open. They can explode, however, if confined to enclosed spaces like rooms, roof eves, sewers et cetera, and that's really what happened in Cleveland. Up to this point we have been talking exclusively about chemical explosions wherein a rapid oxidation occurs. However, there is another type of explosion to which liquefied natural gas is susceptible and that is a purely physical event. Such explosions transpire in a matter of milliseconds (as compared to microseconds for chemical explosions) and the energy-release is orders of magnitude smaller. Nevertheless, the force of these explosions is still sufficient to damage or destroy near-by structures. Similar explosions with damaging results had long since been experienced on occasions when Kraft paper smelt, molten metals or molten slag are dumped into water for quenching.

I first experienced these physical explosions in the mid-1950s when we were initially testing our barge-mounted LNG liquefaction facility in the Louisiana bayous. We observed that upon dumping LNG onto the water, we would hear crackles and pops, some of them were so severe that they shattered glass windows in our buildings on the nearby shore. None of us really understood what was going-on, and we weren't particularly concerned since the pops seemed to be relatively harmless and there was not any evidence of a combustion-type reaction taking place. However, in 1970 the U.S. Bureau of Mines in carrying out some studies on LNG for the U.S. Coast Guard reported an explosion that shattered an aquarium in which they were studying LNG spills on water which raised substantial concerns in the LNG community.

At that time Professor D. L. Katz and I were serving on an advisory committee to the U.S. Coast Guard on marine transportation of hazardous cargoes. At their request we did an in depth study, the substance of which is summarized in a journal publications (42). We were able to identify the cause as a direct result of the LNG reaching a high level of superheating (about 50º C, which is near its theoretical superheat limit) before vaporizing. Both our theoretical (analytical) prediction and the experimental data already available were in excellent agreement. Subsequently, my consulting firm, University Engineers, Inc., did some further studies for our own account to quantify damage potentials (TNT equivalents) as a function of distance from the source (43).

BOHNING: Let me rephrase my earlier question now. How did this concern for LNG safety spawn your series of mathematical papers that you published in the 1980s?

SLIEPCEVICH: Since we had presented our original paper on vapor dispersion from LNG spills in 1969 (41) we continued to use the mathematical techniques that we had developed therein throughout the 1970s to establish safe separation distances for components in LNG terminals for clients all over the world. During the 1970s, however, a number of R and D type contracting agencies, with sponsorship from both government agencies and private companies, were working feverishly to develop more rigorous analytical methods to attack the vapor dispersion problem, particularly since new computer capabilities were skyrocketing. One of the most elaborate computational models, which had cost the private sponsoring organization several million dollars, was being touted as the standard by which all other models were to be evaluated. To put this cost into perspective, back in the early 1960s University Engineers had invested on their own account less than one hundred man hours to develop their model. Since I had some reservations regarding this model—including the initial formulation—I decided it was time—after a hiatus of more than a decade—to take another shot at the problem.

I asked one of my graduate students, Faruk Civan, who was particularly adept in mathematics and computer programming, to make some preliminary comparative evaluations of the various vapor dispersion models that had surfaced. Since this was an area in which my consulting company had been actively involved from it inception, my decision was to fund the study internally. To make a long story short, Faruk Civan got so deeply involved and interested in the work he decided to use it as a basis for his Ph.D. dissertation (44).

Civan's principal contribution was the extension of the method of differential quadrature, which the well-known Richard Bellman had demonstrated in 1972 as an effective technique for solving single, initial valve problems. Civan generalized, expanded and demonstrated this technique to solving complicated problems of simultaneous mass, momentum and energy transfer. Shortly after completing his thesis Civan and I decided to revisit a series of problems with which I had been confronted in the past and for which I had to settle for simplifying assumptions coupled with still laborious numerical calculations. Differential quadrature solutions of these old problems constituted the substance of a dozen papers with Dr. Civan during the 1980s that you noticed in my list of publications.

BOHNING: Did you continue to participate in sponsored research grants or conduct some major consulting work during the 1980s?

SLIEPCEVICH: The answer is yes for both categories. With respect to the former, ever since my first exposure to screening smokes in 1942, I had been involved almost continuously in some aspect of aerosol dispersions. In the mid-1980s in response to a RFP I was awarded a contract by the Chemical Research and Development Center at Aberdeen Proving Ground to undertake an investigation of the substitution of diesel fuel fog oil to generate screening smokes in the battlefield. This study involved experimental work in our chemical laboratories and our wind tunnel in combination with thermodynamic analyses related to phase diagrams, two-phase flow and superheating—a really interesting project.

The two major consulting projects that possessed me during the 1980s were with Dow Chemical Company and Owens-Corning Fiberglas. The Dow work was primarily related to flammability problems that they had encountered with their Styrofoam (primarily in agricultural structures) and Polyurethane (in LNG applications) which had exposed them to a substantial level of liabilities. This work was both analytical and experimental. The latter ranged from bench-scale tests in our laboratory to large-scale field tests at our proving grounds.

The Owens-Corning consulting project was directed to the design of a novel LNG marine tanker, which utilized a fiberglass-based material that had both superior insulating and structural properties. Most of the cryogenic testing was conducted in the Owens-Corning Fiberglas (OCF) Laboratories in Granville, Ohio. My responsibilities were to provide assistance to the OCF engineering staff in devising the test programs, analyzing and interpreting the data, developing structural designs and details for the cryogenic components of the proposed marine tanker and making continuously-updated economic projections. Apart from this project I also participated in some of their new product development work. In this connection I first became directly involved in their research on the enhancement of chemical reactions in the presence of an electric field. Subsequently, when OCF decided to drop further development of this process, all of the experimental facilities associated with the electric field work—which represented a substantial investment in their development of unique equipment—was donated to the University of Oklahoma so that we could continue this type of work on our own. This field of study has now become a major—and quite promising—field of research for my successors in our chemical engineering department at the University of Oklahoma.

BOHNING: Did you ever test the waters again in thermodynamics after your being openly repudiated by your peers on your opinions of the reciprocal relations and microscopic reversibility?

SLIEPCEVICH: Not until I couldn't hold it anymore! Let me explain. In 1986 I was asked to make a presentation at the Oklahoma Academy of Sciences Annual Meeting. My subject area was to be on some aspect of thermodynamics which high school students, who had been invited to the meeting, could comprehend. Rather than present a watered-down, broad-brush overview of the first and second laws with the customary examples, I decided to focus on one elementary concept, reference states, which they could grasp and to demonstrate how misleadingly simple it could be. In the process of presenting examples I did mention, without going into details, how some of the great thermodynamicists of the past and current ones as well have stumbled on this concept.

The reason I selected the subject of reference states is because at that time I was seething with a thermodynamic bee in my bonnet. During the early 1980s I had run across two major blunders in prestigious journals (*Annual Review of Fluid Mechanics* and *Chemical Engineering Science*) arising from an inadequate comprehension of the concept of energy. This same oversight appears frequently in textbook problems in thermodynamics. Even the great J. Willard Gibbs could be guilty of this same treason in the equation he referred to as the defining equation for chemical potential.

Since presentations at the Academy meetings are generally published in a proceedings, I had the option to prepare a manuscript. However, I did not feel that the watered-down presentation I made was worthy of publication. Furthermore, I wasn't convinced that a paper on reference states had much merit; on the other hand I had previously corresponded with the authors of the aforementioned blunders, but I was not able to obtain any retractions. In addition, when I sent copies of these questionable papers to a number of my colleagues asking for their opinions on the validity, I concluded from the responses that the confusion was not isolated. Consequently I sent my file on the subject with a preliminary draft of my manuscript to the inimitable Professor Joseph Kestin of Brown University, requesting his opinion. As was usual with him, I received a prompt, detailed response. His bottom line was, "Publish; the subject matter is not trivial!" I did (45).

BOHNING: I'm surprised you haven't written a book on thermodynamics because I detect a level of excitement in your voice when you talk about it.

SLIEPCEVICH: In my earlier days I made a couple of aborted attempts. Once with Professor Joe Martin at Michigan and about twenty years later with Professors John Powers and Walter Ewbank while they were still on our staff at Oklahoma. In both cases we had advanced to what could be called the first preliminary draft, but somehow (mostly my fault) we just didn't get it done. My problem was that I was always over my head in a plethora of research and consulting projects that were far more exciting than writing a book. In some respects, I regret that I did not push harder to finish the thermo book with Powers and Ewbank at Oklahoma. We did not get favorable comments from our projected publisher's reviewers. We weren't surprised because our book was anything but conventional. As you probably know practically all textbooks on thermodynamics follow a long established pattern; we chose to be different, not for the sake of being different, but to get our message across. The most gratifying comment was that three professors who had taught thermo for years confessed they had never understood thermo until they read our manuscript.

BOHNING: You officially retired at the end of 1990-1991 academic year, but from the looks of the clutter on your desk and the notations on the blackboard, it doesn't appear that you have.

SLIEPCEVICH: My retirement was not only official in 1991, but it was also mandatory. That happened to be the last year that this policy remained in effect. By federal law it had to be revoked thereafter.

BOHNING: Any major projects since retirement?

SLIEPCEVICH: A year before I retired, I received in response to an RFP a contract from the Air Force to conceive and demonstrate a process for producing high purity nitrogen tetroxide, which is used as the oxidant for hydrazine fuel on long-term space flights. Because we were successful the Air Force awarded us a second contract to design, construct and operate a skid-mounted plant for producing ten thousand pounds per year, enough to supply the annual demand of spacecraft-quality nitrogen tetroxide via our ammonia oxidation process. Auxiliary to this plant was the development and implementation of an automated data acquisition and instrumentation for automated process control and the development, design and construction of an abatement system for the plant effluents. Early in 1993, having completed our work, we loaded our skid-mounted plant on two, huge flat-bed trucks and shipped it to the NASA facilities at White Sands where it was going to be put into operation to supply nitrogen tetroxide. The critical element in this project was a novel design of the tubular reactor which I had originally conceived at Michigan and which Autoclave Engineers, Inc. built and donated to me in 1955 after I arrived at Oklahoma to conduct research on it (46). Despite the potentially hazardous nature of this process and its products, I thoroughly enjoyed it because it constituted the practice of chemical engineering in its ultimate form. Select the chemistry, conceive the continuous process, design the equipment, build it (in this particular project every piece of process equipment had to be built from scratch because of its uniqueness or size) and then operate it. I recall the concern of some of my colleagues in the early days that I was too involved in physics, or mathematics or aerospace engineering or biomedicine or whatever.

Having made these statements of commitment, I hesitate to add that my most recent project of substance was a contract that Professor Sherril D. Christian of chemistry and I had with the Office of Naval Research, addressing the problem of ozone depletion specifically due to chemical fire extinguishing agents like the halons. When we proposed on this project initially we soon discovered we were not part of the inner circle of contractors who had been pursuing and acquiring government funding for work in this area for a number of years. In essence we were interlopers. The fact that we proposed to combine physical and chemical fire extinguishing agents to take advantage of a synergistic effect was greeted to some degree with ridicule because it was almost unanimously held that synergism would not exist. Under the circumstances however, a decision was made to give us a shot by awarding us with a six-month contract to confirm by experiments that synergism did in fact exist. This initial contract also specified that if we were successful the rest of our contract for the succeeding eighteen-months would be fully funded. In short, our twenty-four-month effort established beyond any doubt the predictions we had made (47). The unfortunate part was that our work clearly demonstrated that to date an ideal substitute for the halons had not surfaced; in fact it did not look encouraging that it ever would. The reception we received on our work from the community of contractors, as well as the funding agencies, was without exception <u>dead</u> <u>silence</u>. Although we have tried, almost desperately, to solicit comments, nobody responded. We haven't even received the courtesy of a simple acknowledgment of ever having received our inquiry. <u>Professional courtesy does not exist where political correctness is at stake</u>.

BOHNING: What is the present status of your laboratories?

SLIEPCEVICH: The high pressure laboratory that I had does not exist as such anymore, similarly for the process control and cryogenic heat transfer facilities. However, the chemical engineering department has a very active research program underway in chemical kinetics and catalysis, some of which is a continuation of studies I had initiated. My screening-smoke or aerosol work cannot be anymore since our large fire research wind tunnel on North Campus was bulldozed to make space available for other projected activities. What remains of my flame dynamics work has been consolidated, fortunately, with the Combustion Laboratory under the capable stewardship of Professor Sub Gollahalli of the mechanical engineering department. Over the past twenty-five years Dr. Gollahalli has been of inestimable value to me in all of my work related to fire technology. He is much more knowledgeable of my area of flame dynamics than I am about his area of combustion.

BOHNING: One paper on your list of publications caught my attention since it obviously has a religious connotation whereas all the rest appear to be technical. What was that all about?

SLIEPCEVICH: You obviously are referring to my paper "The Serbian Spirit as a Legacy to America," which appeared in the North American publication, *The Diocesan Observer* (48). I was asked by the Serbian priest of the largest Serbian congregation in America, located in Gary, Indiana, to be the speaker at their annual celebration and observance of the Serbian patron saint's day. By request my speech was directed to the youth in the congregation, most of who were three to four generations removed from ancestors born in Balkans.

BOHNING: Were you always active in the Serbian Orthodox Church?

SLIEPCEVICH: On the contrary, I probably had not attended a Serbian Orthodox Church Service more than a dozen, or so, times in my whole life. There was not a Serbian Orthodox Church in my hometown of Anaconda, Montana. Since my folks believed that we should have some formal religious training, my two sisters and I attended the Episcopal Church, which from the standpoint of the liturgy more closely resembled the Serbian Orthodox practice than any of the other Protestant denominations.

BOHNING: Before we wrap up, are there some closing thoughts you would like to express?

SLIEPCEVICH: As I look back over my professional career, I don't think I made a difference, but I tried. If by any stretch of the imagination I have, I truly believe it was due to the element of luck—being in the right place at the right time! Of course to take advantage of such situations one cannot be hampered by the baggage of an agenda or planned timetables for achieving notoriety or collecting awards. From the standpoint of being a true scientist I admit failure because I never possessed sufficient discipline to keep banging-away in a relatively narrow (not to imply lack of importance or far-reaching consequences) or well-defined subject area. In today's parlance I suppose I could be diagnosed as having ADD [Attention Deficit Disorder] because I always welcomed the opportunity to get into new—and often completely foreign—subject areas. The ever present opportunity to challenge the unknown (at least to me) consumed me. Along the way I met and worked with almost a complete spectrum of personalities: in the final analysis, those people really made the difference. I'll conclude by paraphrasing an old proverb—my unwitting motto: Jack of all trades. Master of none. Doesn't garner accolades. But provides a lot more fun.

[END OF INTERVIEW]

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