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Chemical Engineering Education: A Gallimaufry of Thoughts

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Abstract

To discuss various facets of chemical engineering education, I proceed step by step through my own education and career. In this way, I touch on various points concerning the operation of the educational system that may be of interest to others.

THE EARLY YEARS

As a youngster, I attended elementary and junior high school in Fort Dodge, Iowa, a town small enough that we could walk to school. My family then moved to Washington, D.C., where I attended Central High School. Our living room was filled with books, and I was particularly fascinated by my dad's Spanish and German books and by my mother's French and Latin books. I thus grew up with a natural curiosity about foreign languages that has never left me.

When it was time for college, I wanted to study foreign languages, but my father promptly vetoed that idea and said that I would study engineering "because you can always get a good job if you study that" (he was a very successful civil engineer). He further said that chemical engineering (ChE) was the newest and most difficult branch of engineering and that I should major in ChE. Thus was the matter settled.

UNDERGRADUATE YEARS

In the fall of 1941 at age 17, I started at the University of Maryland, although I had been accepted at several other institutions. My parents thought I was not mature enough to move far from home. Maryland's ChE department had just been accredited, and it was small and rather limited. However, I had some excellent teachers in inorganic chemistry (Charles White), organic chemistry (Nathan Drake), and mathematics (Tobias Dantzig). The latter was unusually good, and six of us were privileged to be in his analytic geometry honors section. In 1930, he had published *Number: The Language of Science* (1), which I still find a joy to read.

In December 1941, the president of the United States was heard over the public-address system in the dining hall with his famous "day of infamy" speech, declaring that we were at war. Required hours for the Reserve Officers' Training Corps (ROTC) were doubled immediately, and the university switched to a three-semesters-per-year schedule. Shortly thereafter, I enrolled in the Advanced ROTC program. We learned about military life, military history, first aid, physical fitness, and military tactics. I am convinced that ROTC belongs on college campuses and that two years should probably be required.

Meanwhile, we progressed slowly through our chosen curricula. Inside the back cover of my unit operations book, I made a list of ways that I thought the book could be improved—even as an undergraduate, I was apparently thinking like a future teacher and textbook author. Or was I just a precocious brat?

In December 1942, after finishing my sophomore year, I enlisted in the U.S. Army. In June, I was sent to basic training at Camp Sibert in Alabama and found myself in a company with a swarm of students from MIT, including Alan Michaels and David Himmelblau, who later became ChE professors. Then we were sent back to our respective universities for approximately one semester. By that time, J.M. Smith (who later became a famous ChE textbook author) was at the University of Maryland, and I thoroughly enjoyed his classes. In February 1943, I went on to Officer Candidate School at Edgewood Arsenal, Maryland, where we became officers in 17 weeks. I was then assigned to the 90th Chemical Mortar Battalion and went on to Europe with that unit. I was the only officer in my company who knew both French and German, and this proved to be very useful. Our battalion saw combat all the way from the Belgian-German border to the German-Austrian border. This army experience helped to prepare me for a teaching career because I learned how to interact with all sorts of people and how to size up difficult situations.

After World War II, we were required to stay in the Army for a while, and I took a course in differential equations and a course in German through the United States Armed Forces Institute (USAFI), which was administered in Madison, Wisconsin. Both courses were of value later.

Discharged from the army in April 1946, I worked in a biochemistry laboratory in the U.S. Department of Agriculture until September. I learned a number of important analytical and laboratory skills there: Kjeldahl analysis, lyophilization, titration, testing of pullorum disease antigens, etc. I also had time to review some undergraduate subjects prior to returning to college.

I had already decided not to return to the University of Maryland because I had learned that departments elsewhere had more to offer. I ended up going to the University of Illinois, where I found that the competition was much more challenging. I got a part-time job in the Physics Department working for one of Professor Kerst's underlings, calculating particle trajectories in an accelerator—not particularly interesting, but it provided some extra income.

In the organic chemistry lab, I developed a mysterious contact allergy and spent three weeks in the hospital working with a dermatologist to figure out what chemical was affecting me. He finally deduced that *o*-chlorotoluene was the culprit. At the end of the spring semester, the organic chemistry laboratory was completely scrubbed down and the *o*-chlorotoluene experiment was removed; I was told to come back in the summer and repeat the course. The experience taught me much about contact allergies, their diagnosis, and their treatment.

GRADUATE SCHOOL

At that time, the University of Illinois had a policy that those who did undergraduate work at Urbana had to go elsewhere for graduate school. After talking to friends and faculty members, I decided to attend the University of Wisconsin and work for Joe Hirschfelder in the Chemistry Department. A strict taskmaster, he required his students to take three or four courses per semester and also be at his institute every afternoon as well as on Saturdays. It was an exciting place to be because he attracted all sorts of interesting visitors. Graduate students got to meet most of the influential thinkers in theoretical physics, including S.R. de Groot, J. de Boer, J.G. Kirkwood, I. Prigogine, J.E. Mayer, and many more. Joe was not a particularly smooth lecturer, but his classes were always exciting because he knew many of the pioneers in the fields of quantum mechanics, statistical mechanics, and other topics. He had earned a double PhD with Eugene Wigner as his adviser in physics and Henry Eyring in chemistry. His lectures were peppered with statements such as "I asked Oppie (Oppenheimer) about that the other day . . . ," "Jack Kirkwood told me that . . . ," etc.

While in Madison, I developed a passion for wilderness canoeing, which continued until several years ago. This did get me into trouble, however. Just before the preliminary exams, I went on a two-week canoe trip instead of studying. The result was that I failed the exams and had to take remedial course work.

In research, I worked on two main projects. The first was the calculation of the transport properties of dilute gases and mixtures, using a Lennard-Jones 6–12 potential and the theory of Chapman & Enskog (2). The second was the evaluation of the third virial coefficient, using the same potential function (3). This work familiarized me with the art of computing, various mathematical source materials, and how my work could be used by engineers. We had frequent joint seminars with the combustion group in the Mechanical Engineering Department. After three years in graduate school, I received my PhD. Joe believed that after three years, a student should go out and have other experiences and do something useful. I think that, nowadays, our graduate students spend too much time in school.

In the spring of 1950, Olaf Hougen (Chairman of the Chemical Engineering Department) asked Joe Hirschfelder and Charles ("Chuck") F. Curtiss to give a course in transport phenomena for chemical engineering students—the first course on that subject at the University of Wisconsin. It covered the equations of change, their derivation from molecular theory, and their application to gas flow systems and detonations.

POSTDOCTORAL WORK IN THE NETHERLANDS

By June 1950, Joe had decided to write a book summarizing his various research work on the properties of fluids, and he invited Chuck Curtiss, Ellen Spotz, and me to join him. During the summer we made some plans and started writing. Then, in September, I left for the University of Amsterdam on a Fulbright fellowship to work with Professor Jan de Boer, director of the Instituut voor Theoretische Physica. My year in Amsterdam was a whopping success scientifically, linguistically, and culturally. I worked on several scientific problems, started a serious study of the Dutch language, and went to many concerts at the Concertgebouw (which was just around the corner from where I lived). I traveled all over the Netherlands, Denmark, Norway, and Sweden with Fenner Douglass (a Fulbright professor from Oberlin) looking at baroque pipe organs. I became acquainted with Professor Frits Zernike, whose daughter lived in the same pension that I did; Professor Zernike received the Nobel Prize a few years later for his pioneering work on phase-contrast microscopy. During the Christmas vacation, I traveled to Belgium, France, Italy, Switzerland, and Austria with Alex Kotch (later associate chairman of the University of Wisconsin Chemistry Department) and Bill Hallo (later a professor and Sumerian scholar at Yale). At the end of the trip, I joined my World War II company commander, then stationed in Heidelberg, and we retraced much of our battalion's trajectory during the war. At Remagen we were even able to find the foxholes we had dug on the east side of the Rhine River.

While I was in Amsterdam, I wrote several papers with Jan de Boer (4) on quantum effects in transport phenomena and with Jan van Kranendonk (5) on pressure-induced infrared absorption. Joe subsequently asked me to coauthor two chapters on quantum effects with Jan de Boer for our forthcoming book (6).

I thoroughly enjoyed working with Jan, and he taught me a lot about writing and teaching. For example, he was very insistent that, in sequences of equations, groupings of variables be carefully maintained so that they were easier to follow. He said "Het oog wil ook wat," which means roughly "Your eye also wants a treat." When I told him that I found his lectures incredibly easy to follow, he showed me his lecture notes. On the pages facing his theoretical derivations, he had blackboard diagrams. There he had mapped out exactly where various equations, diagrams, and graphs would appear on the board and when they could be erased. He clearly took lecturing very seriously. That year in Amsterdam influenced the rest of my career in many ways, and the friendships that I made in the Netherlands have survived and flourished through the years.

BACK IN MADISON AND WORKING ON MOLECULAR THEORY OF GASES AND LIQUIDS

The following year (1951–1952) I was back in Madison to finish working on the book that came to be known as MTGL (6). I got into trouble with Joe at the beginning because I wanted to prepare an outline of the book, reorder the chapters, and standardize the notation throughout the book. I felt quite strongly that this was necessary for preparing the manuscript for a very large book (approximately 1200 pages). After some gentle persuasion, Joe finally agreed, and the revision of the manuscript began in earnest. We were able to finish in one year. That was a year of very hard work. In August 1952, Joe, Chuck, and I took three copies of the manuscript down to the train station, and Joe asked the freight office how much it would cost to have someone accompany the package all the way to the publisher (John Wiley & Sons) in New York. Then we celebrated with a final milkshake. (By that time Ellen had dropped out of the project for family-related reasons.)

TEACHING AT CORNELL UNIVERSITY

The morning after we sent our book manuscript to the publisher, I left to become an assistant professor of chemistry at Cornell. I had been informed that I would be teaching a section of freshman chemistry and a course in quantum chemistry. When I arrived in Ithaca, I was told that, because of a death on the faculty, I would be teaching quantitative analysis (my least favorite subject as an undergraduate—I had barely eked out a C in the course!). Fortunately Don Cooke, who had joined the faculty earlier, explained why quantitative analysis was such an important and exciting subject. With Don's help, I was able to get through two semesters of quantitative analysis—and actually enjoyed it!

On April 1, 1953, I received a telegram from Olaf Hougen, back in Madison, offering me a job as a project associate in chemical engineering. I thought it was an April Fool's Day joke and did not reply until I received a phone call from Joe telling me that it was serious. Now I had a dilemma. I was really quite happy at Cornell and had made some very good friends. I also had practice privileges on the Sage Chapel Organ, on which I was working on the Prelude and Fugue in E-flat Major, the Schübler chorales, and other works of J.S. Bach. Also, I had not yet sampled all the great hiking places near Ithaca. Why should I leave?

I made a chart with two columns labeled University of Wisconsin and Cornell; down the side I listed all the factors I should consider along with weighting factors. When I filled in all the numbers, Cornell won. Then I fiddled with the weighting factors and tried again with the same result. After even more fiddling and no change in the results, I finally realized that, deep down inside, I really wanted to return to the Midwest. So I told the department chairman, Frank Long, of my decision, and he was quite perplexed. At Cornell at that time, chemists and chemical engineers were barely on speaking terms. I pointed out to Frank that at U.W. the two departments had many close ties and that the relations were quite amicable (which seemed inconceivable to him).

AN INTERLUDE AT DUPONT

In June 1953, I saw Cornell vanish in the rearview mirror of my 1941 Chevrolet as I proceeded southward to Wilmington to spend the summer at DuPont. I was assigned to the Polychemicals Department, where my immediate boss was an MIT graduate, Henry Linton, whom I had known at Officer Candidate School. Henry told me that I was going to be a rheologist. He could tell by the look on my face that I had no idea what he was talking about. During the summer, I learned that polymer rheology was quite an exciting subject with many unsolved problems. I worked on viscous heating in polymer extrusion (7). No one there at that time realized that there was a term in the energy equation that described viscous dissipation heating (6; equations 11.1–4). The problem was easy to formulate using the standard equations of transport phenomena. But the chemical engineering curricula at that time did not include a course in transport phenomena, and it became apparent that it should.

IN THE ChE DEPARTMENT AT THE UNIVERSITY OF WISCONSIN

I returned to Madison in time to take a two-week trip to Canada before the fall term. Olaf had told me earlier that I would be teaching the graduate thermodynamics course that fall and that he wanted to see more statistical mechanics introduced into the course. I was really looking forward to that. However, because Bob Marshall had just been named associate dean of engineering, I was asked to teach the course in fluid dynamics that Bob had planned to initiate.

I decided to start by reviewing what the students had learned in unit operations: the overall mass, momentum, and energy balances. Then there was something called the Bernoulli equation. Everything I read in the engineering books on this equation seemed confusing. Badger & McCabe's book (8, p. 23) said that it was "only a special case of the more general law of the conservation of energy." Walker et al. (9, p. 46) stated that "an adequate appreciation of the underlying significance of Bernoulli's theorem cannot be had except in the light of the second law." So what was I supposed to tell my class? That it was related to the first law of thermodynamics or the second law?

I had already shown how to obtain the unsteady-state mass balance by integrating the equation of continuity over a macroscopic system, the unsteady-state momentum balance by integrating the equation of motion, and the unsteady-state energy balance by integrating the energy equation. It seemed to me that this Bernoulli equation should be obtained in a similar way.

I soon found that by forming the dot product of the local velocity with the equation of motion, one could generate an equation for mechanical energy, which on integration yielded an unsteady-state generalization of the engineers' Bernoulli equation. A literature search showed that this had not been done before; hence I published a short paper showing how to proceed from the "equations of change" to the "time-dependent macroscopic balances" (10), and this would later appear in BSL (BSL is the students' abbreviation for *Transport Phenomena* by Bird, Stewart, and Lightfoot; 11). Later I published a better discussion of this subject (12; see also section 7.8 of the second edition of BSL). This is a small example of how teaching and research are coupled.

The second semester, I was assigned the course in diffusional operations using Sherwood & Pigford's book (13). Because much of this material was new to me, I had to work through a lot of the basic background material needed for the applications. This was not readily available anywhere, so I prepared a review article (14), which established the notation that would be used later in BSL (11) and presented a number of illustrative problems as well. I took great care with the definition of diffusivity, the frames of reference for the fluxes, and the distinctions between mass and molar units. Most authors seemed to me to have paid little attention to these points.

In September 1957, I was asked to participate in a meeting for engineering deans on the latest ideas for teaching and research. My assignment was to discuss the teaching of mass, momentum, and energy transfer in fluid systems. My presentation actually became a sales talk for a one-semester undergraduate course in transport phenomena according to **Table 1** (15).

My talk emphasized these ideas:

1. In one course, students should see the connections between the transport of the three entities (momentum, energy, mass) and the transport at the three levels (molecular, microscopic, macroscopic)—all in consistent notation. Students should be told that, more often than not, two or three of the transport phenomena occur simultaneously in both industrial and biological systems.

Table 1 A one-semester undergraduate transport phenomena course

	Entities Transported		
Level ↓	**Momentum**	**Energy**	**Mass**
Molecular	Viscosity	Thermal conductivity	Diffusivity
Microscopic	Equation of motion	Equation of energy	Equation of species diffusion
Macroscopic	Macroscopic momentum balance and mechanical energy balance	Macroscopic energy balance	Macroscopic species mass balance

2. Students should understand the similarities and differences among the items in the three main columns. The physics describing the transport of the three entities should be made clear, and, when possible, similarities in the mathematical descriptions should be emphasized.
3. The connections among the three levels of description are equally important. Students should realize that one can go from the molecular level to the microscopic level and also from the microscopic level to the macroscopic level. They should know how one develops the macroscopic mechanical energy balance from the equation of motion, without invoking irrelevant thermodynamic arguments.
4. In addition, under momentum transport, I wanted to introduce students to some simple non-Newtonian models for describing polymeric fluids and thick suspensions. Under energy transport, I wanted to show how the energy equation in terms of the temperature is derived and how the viscous dissipation term arises. Under mass transport, I wanted to define the diffusivity carefully and to explain the role of various reference frames.

THE DEVELOPMENT OF A COURSE ON TRANSPORT PHENOMENA

When I returned to the Madison campus in 1953, Ed Lightfoot had just arrived. He had completed the five-year bachelor of chemical engineering course at Cornell, followed by the PhD program at the same university. Then he worked for the Charles Pfizer Company in Brooklyn, New York. In 1956, we were joined by Warren Stewart, who, after earning his BS and MS at the University of Wisconsin, received his ScD at MIT. Following that, he worked at Sinclair Research Laboratories for approximately five years. Our backgrounds were quite different, but that made us an excellent team for what was soon to follow.

Somewhat earlier, it had been decided that a Department of Nuclear Engineering should be formed at the University of Wisconsin. Bob Marshall was chairman of the planning committee, and I was asked to serve on it. When it was time to propose a Nuclear Engineering curriculum, the committee decided that a course in basic transport phenomena would be needed. I was asked to propose such a course to the Physical Sciences Divisional Committee, whose approval was needed. The committee sent a note to the ChE Department with the query: “Why don’t you have a course like this in your curriculum?” This forced the ChE Department to consider the matter. After much discussion, the department decided by a vote of 5 to 4 to institute the course with the proviso that I would prepare a set of notes during the summer of 1957 so that the course could be instituted in the fall. I indicated my willingness to do this, and, within a few minutes, Warren and Ed expressed their enthusiasm for participating in a joint venture.

During the first half of August 1957, I wrote the text for chapters 1 to 12 to establish the style and level of the textbook and then took off for a two-week canoe trip in Canada. After my return to Madison, assignments were made for all three authors to prepare the remaining 10 chapters. Then began a frantic semester of writing chapters, making up problems, preparing figures, mimeographing chapters, and meeting classes. I held a two-hour session every week to instruct the other faculty members on how best to teach the material. All three authors had other teaching duties and were also supervising research. Olaf Hougen gathered the three of us together one day and told us that we were embarking on a very important project and that it would be appropriate for us to neglect some of our other responsibilities. We appreciated his words of advice and encouragement.

Clearly the team of BSL was “rushing in where angels fear to tread.” Not one of us was truly prepared to undertake our task. Ed and I had been on the faculty for only four years and Warren for just one. In the twenty-first century, such an activity would be considered preposterous—a sure recipe for academic suicide. But life was very different in the 1950s, when teaching and textbook writing were encouraged. Furthermore, Olaf Hougen had been a great teacher and a very successful book writer, and all three of us were greatly inspired by his wonderful accomplishments.

By the end of the fall semester of 1957, we were exhausted. During the spring term Warren and Ed created more problems and illustrative examples to make the book more useful for teaching; this was a demanding assignment, which they tackled energetically. Their years in industry had prepared them well for thinking up interesting and practical brain twisters.

I could not participate in this activity because I had already arranged to spend the spring and summer in Delft (the Netherlands) on Fulbright and Guggenheim grants. I was attracted there by the presence of Hans Kramers, who had been teaching transport phenomena since 1956 and had developed a set of lecture notes (16). I enjoyed interacting with Hans very much, and we had many discussions about how to teach and use the material. Also, I had the chance to see the fascinating experimental setups in his laboratory, many of which dealt with various aspects of mass transfer. During the spring term, I gave the transport phenomena course in Delft and prepared a set of mimeographed notes, *Transportverschijnselen in Stromende Media* (17).

In the summer of 1958, our publisher (John Wiley & Sons) put out a preliminary edition of our book called *Notes on Transport Phenomena*. This was used on a trial basis by J.E. Powers (University of Oklahoma), J. Dranoff (Northwestern University), E. Weger (Johns Hopkins University), and K.M. Watson (Illinois Institute of Technology). These professors and their students wrote extensive commentaries on the provisional textbook, which we took very seriously. One undergraduate wrote, "Never use the expression 'It is obvious that . . .'." It was Professor K.M. Watson who suggested putting subscript labels on each problem to indicate the degree of difficulty; we complied.

THE PUBLICATION OF BSL

Between 1958 and 1960 we completely rewrote the text, resulting in many improvements, and the chapters were read aloud with all three authors present; in this way, we hoped to make the text homogeneous and smooth reading. In the fall of 1960, the book *Transport Phenomena* (11) was finally published. There were two editions: the red book, priced at $11.00, for students; and the green book, priced at $13.00, for industrial practitioners. It was fun to tell our students about the secret messages in the book. In the preface, reading the first letters of each sentence gives: "This book is dedicated to O.A. Hougen." In the postface, reading the initial letters of the paragraphs gives "On Wisconsin"—our school song.

The book went through 62 printings and is now in its second edition (2002) with a revised second edition (2007). All three of us went on to publish books in our own specialties: In 1974, Ed published *Transport Phenomena in Living Systems* (18), which was the first book to combine the topics of biomedicine and transport phenomena. In 1977, my colleagues Armstrong, Hassager, and Curtiss and I published *Dynamics of Polymeric Liquids* (19, 20), which included both continuum and molecular approaches. Finally in 2008, Warren's *Computer-Aided Modeling of Reactive Systems* (21) appeared posthumously (thanks to Prof. J.B. Rawlings's final editing).

At the same time that the department instituted the lecture course, we also established a laboratory course. For this our colleague Edwin J. Crosby (1925–1991) developed a very fine set of laboratory experiments as well as a laboratory manual (22). He wanted the experiments to be valuable teaching tools and to be student-proof. He included experiments on the measurement of transport properties, measurement of profiles (velocity, temperature, and concentrations), determination of interphase transfer coefficients, and the use of the macroscopic balances. By performing the experiments, the students could get a better physical feeling for the subject. These experiments proved to be so successful that he collaborated with a company to reproduce the experiments and make them commercially available to other institutions. Unfortunately, Ed never received appropriate recognition for his work on the laboratory development.

What was the effect of our experiment on teaching transport phenomena? This can be partially answered by examining the proliferation of transport phenomena books since our initial publication. In 1961, a book appeared from the Mechanical Engineering Department at MIT by Rohsenow & Choi (23), who stated in their preface that they had been teaching this material for the preceding 15 years! In 1962, Bennett & Myers's textbook (24) from the ChE Department at Yale appeared. After that, dozens of books appeared on transport phenomena in chemical engineering, all the way from transport phenomena "lite" to transport phenomena "very heavy." There are also specialty books that cover applications to biology, interfaces, colloids, multiphase systems, chemical reactor design, crystal growth, fuel cells, ceramics, porous media, fibrous materials, cardiovascular systems, low gravity systems, food processing, combustion systems, materials processing, and polymer processing. There are other books devoted to the mathematical and numerical solutions to transport problems. Clearly the subject of transport phenomena is now widely recognized as a useful applied science topic in many fields.

By January 1998, I was anxious to prepare a second edition of BSL. My coauthors were somewhat less enthusiastic, but they gradually joined in and made some fine contributions. The book (published in 2002) was almost entirely rewritten with three chapters added: 0, 8, and 24.

A NETHERLANDIC INTERLUDE

When I returned from Delft in the fall of 1958, I was pleasantly surprised that the German Department had hired William Z. Shetter to teach Dutch. Bill had spent a year in the Netherlands and had just published an elementary grammar book (25). Bill and I became good friends, and before long we were talking about the need for a Dutch reader that would present annotated selections from the writings of current Dutch authors. A book like this did not exist. We began collecting materials and editing short stories and poems. After a few years of swapping ideas, we were ready to go into print. Our book, *Een Goed Begin* (26), was quite successful and went into a second edition eight years later.

Bill moved on to Bryn Mawr College and from there to the University of Indiana, having established himself as one of the leading Dutch teachers in the United States. Later we collaborated on a second literary reader, *Reading Dutch* (27). I enjoyed this project even more than the first one. For me it was an escape from the world of equations, graphs, experiments, and theories.

Interestingly, I had the full support of the dean of engineering and the chairman of chemical engineering to indulge in this kind of extracurricular activity, something that would not have happened at some universities. Frankly, I feel that developing a second academic career should be encouraged. A true university should to some extent be a playground for scholarly pursuits and not a high-pressure cooker for research publications (which it now seems to be). I have found it quite refreshing to rub elbows with colleagues in foreign language departments. Their way of thinking and their connection with human beings are quite different from those of scientists. We can learn a lot from them.

TEACHING IN JAPAN

In the fall of 1961, I was invited to participate in the twenty-fifth anniversary of the founding of the Society of Chemical Engineers of Japan. Earlier that year, Bob West of our Chemistry Department had been to a similar meeting, and he had given his talk in Japanese by having a Japanese student translate his lecture and transcribe it in Roman letters. Not to be outdone, I decided to do the same thing. Reiji Mezaki was the first Japanese student in our department after World War II, and he expressed his willingness to help me. The talk went moderately well, but I was quite nervous as I stood before hundreds of people reciting my lecture and wondering whether it was comprehensible.

As a result, I was invited to teach transport phenomena as a Fulbright professor the following year, 1962–1963, at Kyoto and Nagoya Universities. It was a wonderful experience, and I made numerous friends and acquaintances. I also took Japanese lessons twice a week so that, by the end of the year, I could stumble around moderately well with one of the most difficult languages in the world. I was able to visit numerous universities, research institutes, and industries while I was in Japan.

AN ADMINISTRATIVE INTERLUDE

From 1964 to 1968, I served as departmental chairman. The first two years were reasonably pleasant, and I was able to carry a full teaching and research load and still handle administrative matters. It was during that period that I set up an international committee to solicit funds for establishing the O.A. Hougen Visiting Professorship to bring outstanding scholars to our department.

However, in the last two years, the campus was plagued with riots and the nastiness of the Vietnam War era, and I had to spend a good deal of time on departmental safety. Many of the protests, sometimes violent, occurred near the Chemical Engineering Department offices, laboratories, and classrooms because the engineering placement offices and interview rooms were located literally next door. It was also necessary to spend time with the students to ensure that any legitimate complaints were heard and appropriately addressed.

I had agreed to function as chairman for a four-year period, and when the period was up I was only too happy to relinquish the job and return to scholarly pursuits. Up to that time, my students had been working on problems in diffusion and also polymer fluid dynamics from a continuum point of view. I decided to shift my focus to the molecular aspects of polymer fluid dynamics.

POLYMER FLUID DYNAMICS AND KINETIC THEORY

After the chairmanship, I spent a month in Japan talking to polymer scientists who were familiar with the molecular theories of polymers. While there, I collected reprints of key papers in the field, and then I went to Hawaii (Waikiki Beach) and spent a month swimming, hiking, and studying kinetic theory. At month's end, I had planned what I wanted to do for the next several years.

The first project was to summarize the work in the literature on the dumbbell models, the simplest models for elastic and rigid polymer molecules. This resulted in a review article (28) coauthored with Hal Warner (a graduate student) and Colin Evans (a postdoctoral student from the United Kingdom). Later this work was extended by Bob Armstrong (29, 30). At approximately the same time, Ole Hassager was beginning to explore more complex molecular models (31, 32). One of the most successful dumbbell models that we studied was the Warner or FENE (finitely-extensible-nonlinear-elastic) dumbbell and a simplification called the FENE-P dumbbell (using the Peterlin assumption); the latter can mimic many of the rheological properties of polymeric liquids fairly well (33).

All this time, I was developing a course in the continuum theory of polymer flow as well as coteaching a course on polymer kinetic theory with Chuck Curtiss of the Chemistry Department. There was, at that time, no textbook that covered both the continuum and the molecular aspects of polymeric fluids. Before Bob Armstrong and Ole Hassager finished their PhD work, they came to my office one day and said that they would like to write a book with me covering this material! I tried to explain to them that this would involve an enormous amount of work, but they would not be dissuaded. This resulted in the two-volume textbook *Dynamics of Polymeric Liquids* (or DPL) with Chuck Curtiss participating in Volume 2 (19, 20).

For approximately two decades, I served as a consultant to Union Carbide in Bound Brook, New Jersey. It was interesting that many of the questions that people were asking me bordered on kinetic theory. Researchers at Union Carbide were being asked to make connections between the rheological properties of the polymers and their structure. They wanted to know how molecular weight distribution would affect the viscosity and normal stresses. They were interested in how mixing two different polymers would affect the processability in various situations. These questions could not be answered easily, and many still cannot be answered; fundamental research at the molecular level has simply not been done yet.

In 1994, two years after I retired, I was invited to return to Delft and teach a semester course on the kinetic theory of polymers in the Mechanical Engineering Department. Because the course was going to attract people from other universities as well as industry, they asked me to lecture for four hours on Tuesdays so that the noncampus participants would not lose so much time in travel. Reluctantly I agreed to this. Lecturing in a foreign language for two hours at a stretch is very wearing, but doing that twice in one day is extremely so—particularly because one has to chat during lunch with the participants.

While lecturing, I began to get ideas about what should be done next in kinetic theory. All of our effort had been concentrated on the stress tensor and the equation of motion. But no one had made a similar study of thermal conductivity and the energy equation, and as far as I knew no attempt had been made to study diffusion in multicomponent mixtures. By the time I left Delft, I was convinced that this would be a good postretirement project. Back in Madison, I broached the subject with Chuck Curtiss, who was rather lukewarm to the idea initially. But after I returned from a Canadian canoe trip, he had already begun to work on the energy equation and the thermal conductivity! During the fall term, while I was lecturing at the university in Louvain-la-Neuve in Belgium, Chuck and I continued working by email; during the next two years, we developed a kinetic theory of polymers that has the same general structure as the kinetic theory of gases but is much more complex because of the extension of the polymer molecules in space. This work resulted in a review article (34) in which we laid the foundations for studying all three transport phenomena (mass, momentum, and energy) from a common starting point using bead-spring models of any degree of complexity. In subsequent publications, we made a start at this activity (35–41). But much remains to be done, both in theory and experiment.

We found that there should be a chain-chain interaction contribution for both the stress tensor and the heat-flux vector when dealing with undiluted polymers. Our theory is thus in conflict with the network theories of Lodge (42) and others, and with the tube and slip-link theories of Doi & Edwards (43) and subsequent workers, who do not include a chain-chain interaction term in their expressions for the stress tensor. There may be some reason that such a contribution is negligible compared to the term that accounts for the tension in individual polymer chains, and this certainly merits further investigation. Schieber (44) has developed a successful slip-link model for undiluted polymers that seems to describe polymer rheology remarkably well, but he too has ignored the chain-chain interaction.

With regard to thermal conductivity, we found (38) that tightening up the springs in Fraenkel bead-spring dumbbells (dumbbells that have a rigid rod and a Hookean spring in series) so that they nearly become rigid rods has only a small effect on the rheological properties but results in an enormous increase in the thermal conductivity. Therefore, in modeling the polymers as bead-spring chains, this effect needs to be taken into account in thermal conductivity calculations.

In addition, in the systems studied in References 34–41, we found that many cross effects appear, such as the effect of velocity gradients on the heat and mass fluxes. Such cross-effects are not allowed in the thermodynamics of irreversible processes (according to the Curie principle). These effects have not been measured experimentally, and it would be interesting to see if the effects are small or large.

FURTHER LINGUISTIC ADVENTURES

After being in Japan for a year and taking language lessons, I was discouraged because I still could not read technical papers or books in Japanese. I finally concluded that I had not mastered an appropriate set of kan-ji (these are the 2000 or so Chinese characters needed for reading and writing Japanese). I therefore undertook a project of doing a kan-ji frequency count in a general physics book. I then tabulated the 300 most important kan-ji for technical reading. About the time I finished that task, Ed Daub joined the faculty of the College of Engineering at Wisconsin. Ed had a BS and MS from our department, but, after graduating he went to Union Theological Seminary to prepare himself for the ministry. He then served as a minister in Japan and later took on the additional task of teaching chemical engineering (in Japanese) at Doshisha University. We decided to collaborate on a Japanese technical reader because nothing was available in that area.

This turned out to be a very fruitful collaboration. In 1975, working with Professor Nobuo Inoue of the Science University of Tokyo, we published *Comprehending Technical Japanese* (CTJ), the first entry in its field (45). This book was written for the reader who had studied one or two years of elementary Japanese and was therefore familiar with the grammar. Later we decided that we could be more effective if we prepared a grammar book that presented only those aspects of the language that are needed for technical reading. This resulted in a second book (46), *Basic Technical Japanese* (BTJ), which is the only book of its kind. We later published a few supplements to BTJ for people working in areas with very active current research. I chose to write one on polymer science and engineering (47) together with Sigmund Floyd, who had been a graduate student in our Chemical Engineering Department. He had experienced the Japanese school system and earned his BS at the Tokyo Institute of Technology; thus he was truly bilingual as well as very bright.

Still later, in 2009—just for fun—I wrote another book called *Ichi, Ni, San: Adventures with Japanese Numbers* (48) in collaboration with Reiji Mezaki. This modest little book describes the various ways that numbers are used in Japanese; some are quite different from English usage.

A STINT AS DEPARTMENT HISTORIAN

As a high school student, my least favorite subject, by far, was history. In college, I avoided courses in history, and thereafter I never read any history books. I came to regret this very much during my three stays in the Netherlands and also my sojourn in Japan.

However, as our departmental centennial year approached (2005), I felt that there should be some kind of written record for our first 100 years. Olaf Hougen had written the first draft of a history up to approximately 1965, but he labeled it incomplete and unsuitable for publication. Having just finished the second edition of *Transport Phenomena*, I decided that I should take up the task where Olaf had left off.

I found that writing history is fun, but it is not easy. It is quite frustrating to get facts, dates, and people's names all correct. More often than not, records are incomplete. It is difficult to understand personal interactions, prejudices, and behavior, as well as how ideas are developed. Thus, writing a history is tantamount to solving one mystery after another! And it involves research—knowing where to find things and whom to ask for information. Even more difficult is deciding what to include and what to omit. In addition, a history book is more lively and meaningful if photographs can be included—and where do you find those?

I encountered all of these problems. Of course, it was a godsend to have Olaf's rough draft of the first six decades of departmental activities. So I plunged in and decided how to carry the history forward through the last four decades in the spirit of his writing. I learned a great deal that I should have known earlier: details about professors' lives and activities, accomplishments of the first faculty members in the department, interactions between the department and various deans of the college, the role of the wives and children of faculty members, and personal problems of some of the faculty and staff—all these things helped explain how the department had evolved. I could feel the excitement of our founding chairman, Charles F. Burgess, as he created the department and exhorted his faculty to take part in national meetings and to publish their research. I could feel the pain that Olaf Hougen must have experienced when Ken Watson decided not to collaborate further on the revision of *Chemical Process Principles*. I could feel the sense of pride that Roland Ragatz must have had when the department was running smoothly because of his meticulous attention to administrative details. By the time I finished *100 Years of Chemical Engineering at the University of Wisconsin* (49), I had renewed admiration for those who preceded me, and I understood better the many difficulties they had to overcome. I also now have great respect for historians and the assignments they tackle!

THOUGHTS ON CONTEMPORARY EDUCATIONAL PROBLEMS

In some ways, university education today is much better than it was back in 1941, when I started college. The professors are generally better educated, the laboratories are better equipped, searching for information is considerably more efficient, and contacts between researchers at different institutions are easier. On the other hand, some problems today are, in my view, quite serious—problems that should be addressed and corrected. I mention just a few below. Several were discussed in an article I wrote thirteen years ago (50), which generated a flood of emails, letters, and phone calls (both in agreement and in disagreement); no other publication of mine has attracted so much attention.

1. Grant proposal writing. A universal complaint seems to be that all faculty members expend an undue amount of effort writing research-funding proposals. Some say that they must write ten proposals in order to get one funded. What a colossal waste of time and energy! The deans of engineering and science should get together and work out a new scheme for research funding and see that it is implemented.
2. "Getting and spending we lay waste our powers." This is a line from one of Wordsworth's sonnets. Not only is it difficult to obtain funding, but one is also expected to write lengthy reports at various intervals explaining what has been accomplished. In addition, one is required to participate in the peer review process, which takes further time. The researcher ends up jumping through a variety of hoops just to please the granting agencies.
3. The pressure for conformity. Today's young professors understand from the beginning that they must publish or perish (cash or crash). On being hired, they put on their blinders and march forward: writing proposals, publishing, attending meetings, and then doing it all over again. Instead of being challenged by scientific curiosity, they are challenged by financial hurdles. Is this appropriate for an institution of higher learning? There seems to be one, and only one, route to tenure. We should remember that J. Willard Gibbs did postdoctoral study for three years in Europe and then returned to Yale University; he published virtually

nothing there until he wrote his highly original articles on thermodynamics—well after receiving tenure.

4. Loss of the collegial spirit. A true university should have faculty members who have broad interests outside their own field and take an interest in the activities of other departments. When I first came to the University of Wisconsin, there were many luncheon and dinner clubs. These were groups of professors from a variety of departments who met once a month to hear one of their members talk about his or her research or other scholarly activities. This fostered a spirit of collegiality and strengthened the bonds between departments. These luncheon and dinner clubs have now completely disappeared, and the university is poorer as a result.
5. Need for book writing. Because of the rush for funds and the pressure for publishing results, book writing—whether textbooks or research monographs—seems to have taken a back seat. First-rate textbooks are essential for undergraduate and graduate teaching. They should be written by top-notch academics who are active in research, scholars who can anticipate the needs of the next 20 years. Nowadays many of our textbooks are written by mediocre professors with limited research experience.
6. Universities are being run too much by nonprofessors. At U.W. we have had a tradition that, when a vacancy occurs in a high administrative post, a search-and-screen committee presents a slate of candidates to the board of regents or chancellor. Fifty years ago, the search committee consisted of deans, departmental chairmen, and top-flight scholars. This committee understood very well what kind of person was needed to fill the vacancy and where such a person could be found on the campus or elsewhere. Nowadays, a search committee must contain students, secretaries, low-level administrators, basketball coaches, etc. These people do not understand their assignment or how to tackle it. They lack the crucial contacts on and off the campus for getting accurate information on likely candidates. Whereas the committees of 50 years ago could operate quickly and efficiently, the committees of today waste untold hours and often come up with an inferior slate of candidates.
7. Curricular studies. Very little has been done in recent years to update curricula, probably because the professors are too busy working on grant applications. Some alternative course arrangements should be proposed and tested. For example, in chemistry, I think it would be preferable to start with organic chemistry and biochemistry (with compounds of just a few atoms such as C, S, O, H, N) and a lab that illustrates extraction, distillation, filtration, etc. (i.e., the unit operations). Next, one could teach inorganic chemistry and the periodic table, followed by physical chemistry, and finally analytical chemistry. I think the present freshman chemistry and sophomore physics smorgasbord courses may have outlived their usefulness.
8. University versus community colleges. I disagree with the idea that everyone should go to the university. Only properly prepared and motivated students should be admitted. The university should be reserved for training the leaders in various fields. Others should go to community colleges or trade schools.
9. Foreign language study. We hear a lot about the fact that we are in a global economy. However, most Americans are not equipped to participate fully in overseas ventures. There should be a foreign language requirement for entry into the university and then additional study in undergraduate curricula.
10. Annual salaries. Professors should be paid for the entire year, rather than for two semesters. They should not be asked to get all or part of their salaries from outside grants. It should be recognized that professors are asked to perform many functions for which they are not

properly compensated: serving professional societies, refereeing manuscripts for journals, refereeing grant proposals, and serving on university and government committees.

11. Honors sections. Professors should spend more time with the very best students. In honors sections of some courses, gifted students should receive special instruction. No doubt cries of elitism will be heard, but they should be ignored. Are there objections when athletic departments spend a disproportionate amount of time with the most talented athletes and take less interest in the "stumblebums"?

CONCLUDING COMMENTS

Looking back over my academic career, what activities have had the biggest influence on my own career?

- Starting out in chemical engineering and ending up in chemical engineering, with forays into chemistry and physics, proved to be quite lucky. The extra training in the pure sciences enabled me to make a unique contribution to chemical engineering because I had a different perspective on teaching than those whose training had been exclusively in one field. Students should understand that it is often beneficial to switch fields in midstream.
- Book writing was an activity that I thoroughly enjoyed. Every writing project that I worked on resulted in a book that filled a need or charted a new direction. These books were not palimpsests but writings with some element of freshness. They would not have been possible without coauthors who were having as much fun as I was. Coauthors with vastly different backgrounds, education, and prejudices are the best kind to have. The more disagreements you have along the way, the better the final book will be—provided that you can take criticism and seek compromise.
- Having a second career can be beneficial. In my case, studying other languages has enabled me to visit many places in the world, interact with scholars elsewhere, and develop interest in the history and customs of the people. It has also given me a sense of curiosity about the students that we have in our department who hail from faraway lands and the problems they encounter in the United States.
- I firmly believe that it is important to have an interest in some kind of athletic activity. For me, it was wilderness canoeing. During my career I have taken about 75 two-week canoe trips, mainly in Canada. The most elaborate one was a three-week trip down the Coppermine River in the Northwest Territory all the way to the Arctic Ocean. Once a trip is planned, you are obliged to go, to leave your work behind, and to let your eyeballs get spherical again. I think it is also important to have some kind of artistic outlet. In my case it has been keyboard music, both performing and composing. There is something satisfying about creating and learning to appreciate other people's creations. Whether it be music, art, theater, literature, or ballet, I think everyone should have the luxury of enjoying a creative diversion.

The paragraphs above summarize my recipe for a happy and productive career, but I realize that one recipe will not fit all. My worry is that current pressures on faculty members for conformity will prevent professors of the future from realizing their talents to the fullest and may inhibit their development.

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Errata

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