

## HISTORY OF THE CHEMICAL ENGINEERING DEPARTMENT

At North Carolina State University

James K Ferrell

The Department of Chemical Engineering at North Carolina State University was organized as a part of the School of Engineering in September 1924, when the Board of Trustees of what was then North Carolina State College of Agriculture and Engineering, authorized the formation of the Department. At that time the Dean of Engineering was Dr. W. C. Riddick, for whom Riddick Engineering Laboratories Building was named.

For just a little information about Dr. Riddick, the following quotation is from the program for the dedication of Riddick Engineering Laboratories building on April 27, 1951.

WALLACE CARL RIDDICK - the man who has served State College in more capacities and for a longer time than any other person.

Dr. Riddick was professor, vice-president, and then president of State College, in each position contributing his vast experience, wisdom, and strength to the demands of the jobs before him. As president, he successfully guided the College through the turbulent period of the First World War and its aftermath. Under his guidance, the College made some of its greatest progress.

Dr. Riddick was also a pioneer in the engineering profession in North Carolina and contributed tremendously to the development of engineering education in this state. He resigned as president of the College in order to become the first dean of the School of Engineering, when it was organized in 1923.

Our department here at N. C. State was formed fairly early in the history of chemical engineering. Chemical engineering education in the United States is now, however, well over 100 years old. A centennial convocation was held at MIT on October 7 and 8, 1988, celebrating the fact that Professor Lewis M. Norton started a program in chemical engineering education in 1888 at MIT, the first one in the United States.

To add another perspective to this history, Dr. Warren L. McCabe, who served on our faculty from 1964 to 1972, received his B.S. degree in Chemical Engineering from the University of Michigan in 1922, and his Ph.D. in 1928. Ed Schoenborn, who served as department head from 1945 to 1966, received his B.S. degree from The Ohio State University in 1932 and his Ph.D. in 1941. The Chemical Heritage Foundation has published a listing of major events in the history of Chemical Engineering "The First Century of Chemical Engineering, A Timeline of Discoveries and Achievements". The following are excerpts from that publication:

**1882** A course in "Chemical Technology" is offered at University College, London.

**1885** Henry E. Armstrong offers a course in "chemical engineering" at Central College (later Imperial College, London).

- 1888** Lewis M. Norton starts a new curriculum at Massachusetts Institute of Technology (MIT): Course X, Chemical Engineering.
- 1891** MIT awards a bachelor's of science in chemical engineering to William Page Bryant and six other candidates.
- 1892** A bachelor's program in chemical engineering is established at the University of Pennsylvania.
- 1905** The University of Wisconsin awards the first Ph.D. in chemical engineering to Oliver Patterson Watts.
- 1908** The American Institute of Chemical Engineers ( AIChE) is founded.

In the years just prior to the formation of the Chemical Engineering Department, N. C. State College was still a very young institution, and the development of engineering education was just beginning. Dr. Wallace Carl Riddick was president of the College, and in his report to the governor of North Carolina for the 1920-21 year, President Riddick noted that things were just getting back to normal after the war years, meaning World War One, at that time. Also noted in that report was the fact that Dr. E. E. Randolph was elected Assistant Professor of Chemistry.

In 1920, North Carolina State College was made up of 27 departments, all apparently reporting directly to the President. The departments included Chemistry, Civil Engineering, Electrical Engineering, Mathematics, Mechanical Engineering, Physics, Textiles, and, as an interesting note, a department called Veterinary. The faculty was made up of 22 full professors, 7 associate professors, 8 assistant professors, and 35 instructors; the enrollment was 1,055 students in 1920, and 1,124 in 1921. The salary of the president had just been raised from \$4,500 to \$6,000, and the tone of his report that year was one of considerable optimism.

During the period 1920 to 1924, State College was reorganized to form the Schools of Agriculture, Engineering, Science and Business, Textiles, and a Graduate School. Textiles was originally included in Engineering, but was moved to a separate school the following year. Dr. Riddick resigned as President to become Dean of the new School of Engineering and Dr. E. C. Brooks became president of State College. When the Chemical Engineering Department was formed in September, 1924, Dr. E. E. Randolph, who had joined N. C. State College in 1920 as Assistant Professor of Chemistry, was appointed Head of the new department.

During the 1932-33 year both the School of Engineering and the Department of Chemical Engineering wrote reports to Dr. Brooks on the status of the School and Department. Dr. Riddick's report was addressed to Vice President E. C. Brooks. In it he reported that the School occupied space in five buildings amounting to 130,000 square feet and having a value to replace of one million dollars. He noted that the School was made up of 34 faculty above the rank of instructor with one Ph.D. and one LL.D. The Ph.D. must have been Dr. Randolph.

Dr. Randolph titled his report "Data Concerning the Chemical Engineering Department of State College of the University". In the report he states that "The Chemical Engineering

Department was authorized in February, 1924, in time for registration of students for the second semester of 1924. Twenty two students had been prepared so that they registered as Chemical Engineering students of whom two graduated that year." He also states "Dr. W. C. Riddick, President of State College . . . in 1920 engaged E. E. Randolph to teach physical chemistry and to develop a Chemical Engineering Department at State. Then for several summers I did regular graduate work at MIT and took their regular experience training at different large plants of chemical industries in their Practice School of Chemical Engineering." This pretty well leaves it up to us to decide whether the department beginning was in 1920 or 1924.

Apparently when the department was formed it included authorization to offer both the BS and the MS degree in Chemical Engineering. The first MS degree was awarded in 1928.

Dr. Randolph was born in Charlotte, North Carolina, July 22, 1878, and was educated at Sardis Academy and the University of North Carolina where he received his A.B., A.M., and Ph.D. degrees. While his degrees from UNC were not in chemical engineering, he later did graduate work in chemical engineering at MIT. He served as Department Head until his retirement in 1945, and died at his home in Raleigh on November 10, 1954.

At the time the Chemical Engineering Department was formed, the School of Engineering consisted of departments of Architectural, Chemical, Civil, Electrical, and Mechanical Engineering. The departments of Chemistry and Physics were in the School of Science and Business; Mathematics was not listed anywhere, but apparently reported to the Dean of Engineering.

The department was originally located in Page Hall, but was moved after a few years to Winston Hall. For the first five years, Dr. Randolph was the only faculty member in the department and taught all of the courses and laboratories. In 1926 the enrollment in the department was 36 undergraduate and 5 graduate students, and had grown to 127 by 1930. In his report of 1928 Dr. Randolph noted that the demand for chemical engineers in North Carolina exceeded the supply. He also made a strong plea for an instructor to help with the teaching, for space for constructing chemical engineering equipment, and for an office for himself and the instructor.

Some idea of what things were like in those days can be gotten from the 1929 annual report of the college, which devoted two pages to chemical engineering.

Curriculum in chemical engineering seems to meet the desires of a considerable number of earnest young men who wish to prepare themselves for the profession. Much good work has been done under Professor Randolph's direction to provide a suitable chemical engineering laboratory. Three stone-top desks have been installed and provided with water, gas, live steam, alternating and direct current – high and low voltage – compressed air, and vacuum. Semi-plant scale equipment is being designed and built rapidly, in line with resources and industries of the State. Funds have been provided through the Engineering Experiment Station for making a complete cotton-seed oil refinery. This equipment was designed by the department and has all essential units and control system of large refineries.

A destructive distillation outfit has been built with a capacity of distilling 100 pounds of coal or oil shale at a run. . . . A large down-draught kiln has been constructed . . . A complete model water purification plant has been designed and built with a capacity of 1,000 gallons of pure, clear, and soft water of zero hardness.

. . . A complete bleachery for peanuts has been built. This bleachery is of wood with a rotating drum provided with live steam and chlorine connections. The student who largely constructed this equipment has perfected a method whereby discolored nuts may be made marketable, solving a serious agricultural problem.

. . . The ten graduates in this department have been placed for the next year with the following companies: Feldspar Milling Co., Aluminum Company of America, Champion Fiber Company, DuPont Company, Burlington Rayon Company, Proctor Gamble Company, and American Enka (Rayon) Company. One will return to State College as a fellow.

All of the above was done with just one faculty member and several undergraduate students. Dr. Randolph put an estimated value of \$3,500 on the equipment, and with a little imagination, you should be able to picture what the lab looked like. Dr. Randolph wrote lengthy reports and managed to make things look pretty good. In this respect, he was as much a master of the annual report as the department heads who followed him.

Most of what is known about the department during the 1930's and early 1940's is listed in Appendix A, and little exists to describe what happened during the World War II years. Shortly after the war, however, and with the return of many veterans, things begin to change. Dr. John Harold Lampe, B.S., N.E., and Doctor of Electrical Engineering from Johns Hopkins University, was appointed Dean of the School of Engineering in 1945. Since most of the department heads in the school were of retirement age, Dean Lampe was able to bring in new department heads, take advantage of the post-war atmosphere, and give the School of Engineering a real boost. In 1945 Professor Randolph retired and Dr. Edward M. Schoenborn, who had received his Chemical Engineering Degrees from The Ohio State University, was recruited from the University of Delaware and appointed Department Head.

## APPENDIX A

### Recollections of Dr. James K Ferrell 1999

The following few pages reflect some personal views of my early years at the University of Missouri and at North Carolina State University.

I started at the University of Missouri at Columbia in the fall of 1941. At that time the major College of Engineering in Missouri was at Columbia. The College at Rolla, Missouri was the Missouri School of Mines, and as far as I know, had no chemical engineering. My first two years as a student at MU, as it was known then, were not too successful. For some reason I didn't know how to study and my grades were just adequate to stay in school. Part of the reason for this was that World War II was underway and it was clear that I was not going to be able to finish a degree anyway. I was in an accelerated ROTC program and was called to active duty in June of 1943. After three years in the army, I returned to school in August 1946. For some reason, I now found that I knew how to study and liked to study and make good grades; that I enjoyed the competition for grades and that I liked to take examinations. I made all A's and finished at the top of my class. I wish I knew how to tell students how to do this, but I am really not sure I know how I made the change.

I received a BS degree in Chemical Engineering from the University of Missouri in 1948 and an MS degree in 1949. At that time a good starting salary for a BS Chemical Engineer was \$3,600 per year. I had decided that if I could find a way to afford it (I was married and had two children) I would enter a Ph.D. program. My advisor at Missouri, Dr. Frank Oldham, knew that Ed Schoenborn had recently become Department Head at N. C. State College and that a Ph.D. program had recently been approved there. At that time Schoenborn was fairly well known, mostly for his work with Allan Colburn at the University of Delaware and at DuPont. Both Frank Oldham and the Dean of Engineering, Dr. Harry Curtis, contacted Schoenborn, and I eventually got an offer from him for a position as fulltime Instructor of Chemical Engineering at a salary of \$2,800. The year 1949 was one of those post World War II years when North Carolina had a budget surplus, and before I arrived in Raleigh, my salary had been raised to \$3,100 for nine months. I felt pretty good about this since it was about what my classmates were getting in industry.

When I got here in 1949, the Chemical Engineering Department was located in Winston Hall. The facilities were pretty bad: all offices had two or more faculty in them, there was very little research, and the classrooms were dark and uncomfortable. The student laboratories were in the basement, and as I now remember things, it was dark, damp, and had some of the oldest and dirtiest equipment I had ever seen. Someone had refurbished and polished up an old batch distillation unit, however, and the gleaming copper kettle seemed to provide most of the light for the room.

If I had known enough to be disappointed, I would have been. However, things were really not as bad as they seemed. The undergraduate program had been recently accredited, the

Ph.D. program had just been approved, and there were about dozen graduate students enrolled and several were getting started on their research. I found a very friendly faculty with high morale, Riddick Engineering Laboratories building was almost finished, and things actually looked pretty good. The department held a party for the class of 1949; Appendix B gives copies of the invitation to the party from Ed Schoenborn in the form of a poem, and the responses of several of the class members, also in the form of poems. Several of these students were beginning graduate students when I arrived. Appendix B shows something of the general good spirits of the department at that time.

At the beginning of the fall semester of 1949, the faculty was:

1. Edward M. Schoenborn, Professor and Head. Ed had been brought in by Dean Harold Lampe in 1945 to replace Dr. Randolph who had been head since the beginning of time.
2. Kenneth O. Beatty, Associate Professor. Ken was one of the first new faculty Ed brought in. Ken received his BS and MS from Lehigh University and had either very recently finished, or was in the process of finishing, his PhD degree from the University of Michigan working under Don Katz.
3. Phillip P. Pike, Associate Professor. Phil was also a new addition to the faculty and was in the process of finishing his PhD degree at the University of Minnesota. He had his BS and MS from MIT.
4. Donald S. Arnold, Instructor, in the process of finishing his PhD degree at Ohio State. Both BS and MS were from Ohio State. He did finish his degree and was made Assistant Professor sometime during 1950.
5. Russell F. Hazelton, Associate Professor. BS from Wayne State, MS and PhD from the University of Michigan.
6. Richard Bright, Assistant Professor. BS and MS from Iowa State University.
7. J. Frank Seely, Assistant Professor. BS and MS from N. C. State College.
8. Eugene B. Finch, Instructor, working on a PhD degree at N. C. State.
9. James K. Ferrell, Instructor, working on a PhD degree at N. C. State.

I was in the dual role of graduate student and faculty and so I got a picture of things from both sides. It was clear that Schoenborn, Beatty and Pike were the senior faculty and the people who were going to be important. Russ Hazelton was also senior in rank, but somehow didn't seem to be in the same group. Frank Seely, while not very senior in rank, was even then easily identified as the person who knew everything about the department and the college.

For the fall quarter (NCSU was on the quarter system at that time) I signed up for two courses: one in mathematics taught by Dr. Bob Bullick (Functions of a Complex Variable) and one with Ed Schoenborn (Diffusional Operations). I was assigned to teach a section of Unit Operations with Phil Pike and two sections of the Senior Unit Operations Lab with Dick Bright. For the winter quarter I took the first course in Operational Mathematics with Dr. John Cell and a course in spectroscopy in the Physics Department. Both Dr. Cell and Dr. Bullick were excellent teachers and my contact with them certainly increased my interest in applied mathematics. John Cell became a good friend and was on my graduate committee. During the 1949-50 academic year and the summer of 1950, I managed to complete 25 quarter hours of course work.

One of the requirements for graduation in Chemical Engineering at that time was a week-long field trip to visit chemical companies, scheduled for the week between the winter and spring quarters. Don Arnold had done all the scheduling and planning for the 1950 trip and I was asked to accompany him as a second faculty member. For this event we chartered a Trailways bus and took the entire senior class to New York. The bus departed from Winston Hall on a Saturday evening and we drove all night to New York City. In addition to the students, Don Arnold, and me, there was also lots of beer. Frequent stops were necessary, and since the crowd was too large for any filling station to accommodate, these stops were usually by the side of the road with the entire class lined up on the shoulder relieving themselves in the ditch. (There were no women students in that class.)

The trip was financed by charging the students a fee to pay for the bus and hotel rooms, and for other expenses. The entire class stayed in a hotel just around the corner from Times Square. The arrangements with the hotel included a free room for Don and me. The hotel was excellent, and in 1950, Times Square was a very nice place to be in New York. Each morning of that week, the bus picked us up and drove us, usually into New Jersey, where we visited a chemical plant during the morning and had lunch furnished by the company. We visited a second plant during the afternoon and returned to the hotel for the evening.

One of the memories I have of that class is that soon after arriving in New York the students made an agreement with a bar near the hotel that if they did all their drinking in that bar, every third drink would be on the house. I thought it outstanding that a bunch of boys from North Carolina could make that kind of agreement with a New York bartender.

Late on Friday afternoon the bus departed New York and arrived in Raleigh sometime Saturday morning. There was not much beer on the bus for the trip home. I, for one, was exhausted. Don and I made the same trip, with the same bus driver, who now seemed almost a member of the class, with the class of 1951 the following year. As I remember it was almost a copy of the first trip and the two have sort of run together in my mind. Whatever else these field trips accomplished, they certainly gave the class a good look at the chemical industry and developed a very strong class spirit. I don't remember when the field trip requirement was eliminated, but I don't think New York was visited again.

During the summer of 1950, the department moved into the new Riddick Engineering Laboratories Building. At the time of the move the Chemical Engineering Department occupied the entire east wing of the building, the front of the building (the north side) was almost all classrooms, the Dean's office was in a suite at the west end of the second floor, and the Engineering Research and Industrial Engineering Departments were in the west wing.

As a part-time summer job I helped with the move. In what is still a typical style for NCSU, the building project had run out of money and none of the offices was furnished, only about half of the laboratories had new lab furniture, and the equipment budget for the building covered only about half of what had been planned. A lot of the stuff was moved from Winston Hall to Riddick in a trailer owned by Frank Seely and pulled by his car. In addition to office furniture and equipment, we moved a large number of very old lab benches from Winston and

installed them in the labs in Riddick. I think the last of these was finally thrown out sometime in the seventies.

If anyone is interested, I still have one of the old lab benches in my basement at home, carried there, as they say in North Carolina, in Frank Seely's trailer. In the fifties and sixties the Department could not have managed without Frank Seely's trailer. Speaking of Frank Seely's trailer, I noticed yesterday (October 21, 1993) that it is sitting in the front yard of his house with a "for sale" sign on it. The Department ought to buy it for the archives.

During the first half of the 1950's, the Department had finished the move into Riddick; the faculty, the graduate program, and the facilities were excellent; Beatty, Pike, Schoenborn, and Hazelton all had active research programs; a Ph.D. program had been approved; and the Department was in far better shape than any other ChE Department in the Southeast. There were far more applications for admission to the graduate program than the Department had the ability to support, and we pretty well had the ability to pick the best.

The 1953-54 departmental annual report contained a section on kinds and costs of research. The following is quoted from that report:

Departmental research during the past year involved doctoral dissertations, masters theses and projects. One doctoral thesis was completed and two are in progress. Six master's theses have been completed and five are in progress. Fifteen senior projects have been completed.

Cost of such research cannot be determined since no budget is provided for this work as such. Sponsored research projects of a basic nature include the following:

1. The Measurement of Thermal Properties in Non-Metallic Materials. \$7,779.
2. Radioactive Tracer Technique. \$24,000.
3. High Temperature Thermal Conductivity. \$2,500.
4. The Performance of Contactors for Liquid-Liquid Extraction. \$21,000
5. Plate Efficiencies on Fractionating Columns. \$23,000.

The dollar amounts for these research projects do not seem very impressive to us today, but they were all large projects at the time and sufficient to purchase good equipment and support a large number of students. For example, the \$24,000 listed for the Radioactive Tracer Technique Project supported Ken Beatty, Billy Richardson, me, and several students.

At that time, 1954, with a good graduate program and lots of research, the Department seemed to be moving rapidly toward real prominence, at least in the Southeast. Somehow the opportunity to accomplish this slipped away. The other departments in the neighborhood improved their facilities, developed strong faculties and graduate programs, and became strong competitors for top graduate students. During the last half of the decade of the 50's, the department seemed to stagnate, and by the early 60's was no longer in the running for the best in the Southeast. There is certainly a lesson to be learned from this period of departmental history.

For my Ph.D. program I minored in both mathematics and physics. The negative part of this was that I had to take examinations for the minor in both. The positive side was that nuclear



physics, quantum mechanics and applied mathematics complemented each other very well and I learned a lot of mathematics, which I really enjoyed. I developed strong ties with Dr. John Cell in mathematics, and with Dr. Buck Menius and Dr. Ray Murray in Physics. Also, since the Physics Department, which was then in the School of Engineering, was building the first nuclear reactor on a college campus, I had the opportunity to participate in that.

This first reactor was a "water boiler" reactor which was made of a spherical vessel filled with an aqueous solution of dissolved uranium fuel (uranile nitrate, I think). Such a reactor operates at a very low power, but generates a pretty good neutron flux and can do some useful things. During the nuclear reaction in such an environment, some water is dissociated into hydrogen and oxygen. To avoid the accumulation of this explosive mixture, something had to be included in the reactor design to take care of it. I spent some time on this problem. Mr. Charles Winslow, a new graduate student from VPI, and I built an experimental catalytic converter to recombine the hydrogen and oxygen and carried out some experiments with the apparatus. Charlie used the results for his master's thesis and I went on to other things for my Ph.D. dissertation.

I picked for a thesis problem an idea conceived by Ken Beatty. The idea was to measure the velocity distribution of a fluid very near the wall by using a radioactive tracer. The project was supported by the Air Force, Wright Air Development Center, and was sufficient to support several people; Ken Beatty, Frances "Billie" Richardson, Harold Lamonds, Jim Pearson, me, and several others. We built a nice experimental apparatus for the fluid flow part of the problem, and some very advanced radiation counting instrumentation was designed and built by Hap Lamonds, who had also designed and built most of the instrumentation for the nuclear reactor.

My contribution was to use a dye instead of a radioactive tracer and a spectrophotometer instead of radiation counting instruments. In either case, the mathematics problem of radial diffusion in laminar flow required the solution of a non-linear partial differential equation. I struggled with this problem for several years, got partial solutions, but never a complete solution. The university had just acquired an IBM 650 computer and I attempted to make use of it for the solution, but soon discovered that it was impossible for that computer. I finished my thesis in 1954, presented a paper at a Kansas City meeting of the ACS, and published it in Industrial and Engineering Chemistry.

I was appointed an Assistant Professor in the fall of 1953, and since I did not yet have the PhD, this appointment was certainly not according to University policy. Ed Schoenborn did it anyway, another indication of how times have changed, and I received my Ph.D. in the spring of 1954.

For the fall semester of 1954, the department decided to try an experiment and teach all of the junior courses at once in a sort of an integrated single course. I didn't participate in this so I am not sure how it came out, but it was dropped and never tried again, so I guess it was deemed a failure. The reason I missed this great experiment was that Ed Schoenborn loaned me to the Mathematics Department for the semester.

I don't remember exactly how this came about, but the Mathematics Department was overwhelmed with students and Chemical Engineering had faculty to spare. I must have agreed to it or Ed wouldn't have done it, but I didn't know what I was getting into. I was assigned to teach two sections of algebra and trigonometry, one section of calculus, and one section of make-up solid geometry. Except for the solid geometry, the sections were large, at least 50 students each, and the algebra and trig sections met five times per week, some on Saturday. I guess this arrangement of loaning faculty was relatively easy since the Mathematics Department was in the School of Engineering at the time.

At that time, the way the Mathematics Department worked, all of the course outlines had been fixed by a committee; the material to be covered in each class was specified and daily homework assignments (lots of problems were assigned as homework and were turned in every day) and exams, were all predetermined. By the end of the first month of classes I had accumulated so many ungraded homework papers I could hardly get in my office. I quickly adopted the technique used by all the other math teachers of checking off the homework papers as turned in, grading none of them, and handing them back with a red check mark on the front. Jim Pearson was in one of these classes, and at least he survived the experience. So did I.

I especially remember the solid geometry classes and my trying to explain some of the theorems by waving my hands in the air trying to describe spheres with planes intersecting them and other interesting things. I had never taken a course in solid geometry and didn't have time to do much preparation, so my ideas had to be cooked up on the fly. I remember watching, with my solid geometry class from a window in Riddick, as Hurricane Hazel passed through Raleigh. Probably one of my better class sessions.

I continued to work with Billie Richardson and Ken Beatty on various aspects of tracer and dye displacement techniques and wrote a proposal and got a grant from the Army Research Office on using the technique to measure diffusion coefficients. We built a new apparatus for this work, but I left before the measurements were finished. Billie took over the project and finished it up.

I left the Department in the fall of 1956 and went to work for the Nuclear Division of the Martin Co. in Baltimore, Maryland. At that time the Martin Co. was engaged in the design and manufacture of airplanes and missiles and had just formed a new nuclear division. The goal of the Division was to be the Briggs and Stratton of the nuclear power reactor field and to build small power reactors for military, space and other such applications. I thought this a great idea. The Division had been formed in a hurry by recruiting people from all over the place and had a mixture of very good engineers and very bad ones. Within one week of my arrival, I was made a group leader and found myself in charge of about 15 engineers. I had a very good time there, made some lasting friendships, and got more good engineering and management experience than I could have gotten in ten years anywhere else.

As a group leader at the Martin Co., I hired a young engineer, Mr. Albert Carnesale, who had just graduated from Cooper Union and seemed a very bright young man. We became very good friends in addition to working together. Al came to NCSU and got his Ph.D. in Nuclear Engineering under my supervision. He was on the N.E. faculty here for a time, left here for a

faculty position at Harvard University, became Dean of the John F. Kennedy School of Government, then Provost at Harvard, and is now the Chancellor at UCLA.

The Nuclear Division at the Martin Co. did build a couple of very successful reactors, but it quickly became clear that the Division, for a number of reasons, was not going to make it. After two years of very interesting and rewarding work, I left the Martin Co. sinking ship and went to work for the Nuclear Division of the Babcock and Wilcox Co. in Lynchburg Virginia. After a couple of years, it became clear that the prospect for commercial power reactors was no better than for small military reactors. In January of 1961, I returned to N. C. State University as Professor of Chemical Engineering, without ever having to worry about things like reappointment, promotion, or tenure.

I did make some good friends and contacts at B&W however. Don Bylund came to NCSU and got his Ph.D. with me and ended up as the top person in R&D at the Milliken Co. Ron Scroggins left B&W and went with the Atomic Energy Commission and was a good source of research funding for me for a number of years.

When I returned to the Department, except for a few differences in faculty and graduate students, nothing had changed. I found the laboratories just as I had left them with everything still in the same place and no new research underway. I think Ken Beatty had one small funded research project, but that was about it. I did find several new graduate students in instructor positions, however, and that was good. Tommy Godbold, now Professor at Vanderbilt, Don Martin, now professor of Computer Science here, and John McGee, former Department Head and now Professor at Tennessee Tech, were just beginning. The Department was, however, certainly in the doldrums.

The Department also had some vacant positions, and during 1961 and 1962, interviewed several candidates for them. As a result, two new faculty were added; Dr. Dave Marsland joined the department from the DuPont Company in 1961 and Dr. Ed Stahel received his Ph.D. from Ohio State and joined the department in 1962. While neither Dave nor Ed managed to get any immediate research funding, the presence of these two new faculty members gave the Department a needed shot in the arm.

I managed to talk two of the graduates of the class of 1961, Joe Privott and Milton Williams, into entering our Ph.D. program and working with me. on their research. Joe finished his degree and is now President of Novus International, Inc. Milton finished the problem I started for my Ph.D. research by finally solving the diffusion equation for the dye displacement problem. He did this by a combination of LaPlace transform, numerical solution of the resulting differential equation and numerical inversion of the transformed equation. All this was done on an IBM 1410 computer, which was pretty good work. It turned out that the technique could measure diffusion coefficients but not very well. Anyhow, the problem was finished as far as I was concerned.

When I returned to the Department in 1961, I was dismayed to find that the University had almost no useful computer facilities, and I started trying to promote the idea that the College and the Department ought to do something about it. Don Martin had had some analog computer

experience at South Carolina and together we started trying to build an analog computer facility for the Department. Dean Lampe was very sympathetic to the idea and provided some monetary support. He also supported a proposal we made to the National Science Foundation. I recall that when our first proposal was not fully funded he commented, "Well that outfit always did think too small".

During 1962 Dean Lampe retired and was replaced by Dr. Ralph Fadum who had been Head of Civil Engineering. Just before he retired Dean Lampe told me he certainly was not going to leave any of the hard earned discretionary money he had accumulated to Ralph Fadum, and he gave Don Martin and me enough to complete a very fine analog computer facility for the college and to acquire an IBM 1620 digital computer. Since it was located in Chemical Engineering, most of the use was by us. Don Martin has written some of his recollections of the computer facilities development in engineering and this is included as Appendix C.

During the search process for a new Dean of Engineering, Don Katz at the University of Michigan was recommended for the position by Ken Beatty. Dr. Katz did visit the campus several times for interviews and discussions, but not take the position. As always in cases like this I occasionally wonder what the College would have been like if he had become Dean.

At the age of 65, Dr. Warren L. McCabe retired from the Polytechnic Institute of Brooklyn where he was Administrative Dean, and he and his wife Lillian moved to Chapel Hill. Ed Schoenborn and Warren had long been friends and Ed seized the opportunity and appointed Warren as a half-time Visiting Professor of Chemical Engineering, starting in the fall of 1964. Warren immediately started an active research program in crystallization, picking it up where he had left it years ago. He got support for the research from NSF and several corporations and started another academic career. Dr. Norvin Clontz was one of his first Ph.D. graduates.

Sometime during the years of 1962 to 1964, with research in the College of Engineering at a very low level, various committees had recommended that the Dean create a position for and recruit an Associate Dean of Engineering for Research. This finally came to pass and Dr. Henry B. Smith was hired. Henry was also given a faculty position in the Department of Chemical Engineering, although he was never active as a faculty member. Henry was an alumnus of the department, a classmate of Frank Seely from the class of 1938, and was Vice President for Research and Development for General Foods when he joined the University.

In addition to the Department, other parts of the University were also in the doldrums. Sometime in about 1954, the University had acquired an IBM 650 computer which was located and managed by the Statistics Department, but was available to the entire university. I thought this a big step forward, and since I was trying to solve boundary value problems, I made a serious effort to learn to use this computer, Programs had to be written in machine language and the total memory was 2000 locations on a magnetic drum. It was fun, but not very useful for the problems I was interested in, so I soon gave it up. When I returned in 1961, the University still had the IBM 650, although it was replaced sometime in 1962 by an IBM 1410.

When I joined the Martin Co. they had several IBM 701's (soon replaced by IBM 704's) and a new language, FORTRAN, for programming. I quickly became a serious computer user.

Babcock and Wilcox also had good computing equipment. When I returned to N. C. State University I was very disappointed to learn that the only computer available was still the IBM 650.

In 1964, higher education in North Carolina, which included the Consolidated University (The University of North Carolina, North Carolina State College, and the Woman's College) and the other colleges in the State, was managed by a Board of Higher Education. The Board had come to realize the growing importance of computing and appointed a committee of the Board to make recommendations for computing in the University system. I became chairman of this committee and spent a lot of time during the course of the year on this report. While Duke University was not a part of the University system, they were included in the findings of the committee. The committee found the computing facilities at all three & the Triangle universities to be inadequate. It also recognized that none of the three had the resources to provide adequate computing without significant help. A major recommendation of the committee was that the Triangle universities pool resources and invest in a large computer facility to serve all three, making use of remote computer access, a technology then in its infancy.

The Board acted on the recommendation, and asked the leaders of the three universities to appoint a joint committee to take action on this recommendation. A committee consisting of two representatives from each of the three was appointed and reported to the president or chancellors of each, Dr. John T. Caldwell at NCSU, Dr. Douglas M. Knight at Duke, Dr. Paul F. Sharp at UNC, plus the president of the Consolidated University, Dr. William Friday.

Dr. Fred Brooks, who had been a leader at IBM in the development of the System 360 computers and the operating system for them, had just joined the University at Chapel Hill as Head of a new Computer Science Department. He was the lead member of the committee from Chapel Hill. Dr. Tom Gallie of the Mathematics Department at Duke, also a computer scientist, was the lead member from Duke. Dr. David Mason, Head of Statistics, and I represented NCSU. I served as chairman of this committee. The committee set to work immediately to write specifications for a computer and draft a proposal to NSF for support.

The final concept was for a computer to be located in the Research Triangle Park, accessed remotely from locations on the three campuses, and managed by an independent corporation owned by the three universities. The concept was successfully sold to the three universities and to NSF. Space for the facility was provided by the North Carolina Board of Science and Technology; the Triangle Universities Computation Center (TUCC), a nonprofit corporation, was formed; an IBM System/360, Model 75 was chosen for the computer; and NSF awarded a grant of almost a million dollars per year to the activity. I took a leave of absence from NCSU to become president of TUCC.

It took TUCC a couple of years to work out all of the bugs and get the performance up to expectations. After that it served the three universities very well for a number of years, but like almost all computer centers it failed to change with the times, became almost useless for university computing, and finally folded up and went out of business.

At TUCC I had another fantastic year of unique experience, and returned to NCSU as Head of the Chemical Engineering Department in July 1966. My appointment as Head is another interesting story of how things were done in 1966.

Some time in the spring of 1966 Ed Schoenborn told me that he was stepping down as Head at the end of June. I informed Dean Ralph Fadum that I was interested in the job. He appointed a committee chaired by Warren McCabe. I know the committee met several times, but I never talked to the committee, or interviewed anyone. I was appointed sometime in June.

During Ed Schoenborn's last year he had gotten the two instructor positions in the department upgraded to assistant professor positions. In addition, the University had just received a National Science Foundation Science Development Grant. This grant provided two senior faculty positions to the department, one in materials and one in mechanics, two support positions, and some equipment money. My first action as Department Head was to help talk Vivian Stannett into taking the senior faculty position in materials in the department.

Vivian accepted our offer sometime in June 1966, and during the fall semester of that year we started working on a laboratory (Riddick 314) for him. With Vivian's help we also recruited Hal Hopfenberg, mostly by letter and cable since he was still in Viet Nam, for one of the assistant professor positions. Both Hal and Vivian started with the department in February 1967.

It would be difficult to overstate the effect that these two new faculty members had on the Department. Even before he actually arrived on campus, Vivian had started to make a big difference in the departmental research program, and by the spring of 1967, he had students, post-doctoral students, research grants, and a very active research program that gave the appearance of having been in place for several years. Some of the laboratories on the third floor of Riddick became almost instant hazards.

Vivian has the very great talent of working with people, and he very quickly developed joint projects with both Hal Hopfenberg and Ed Stahel. As mentioned earlier, during the summer of 1966, the research program was at a very low level, and I was determined to do something about this. Ed Schoenborn, in addition to being Department Head was also the graduate administrator for the department. I asked Ed Stahel to be graduate administrator and together we set out to increase graduate enrollment. Since we now had the resources to support more students, an increase in graduate enrollment was possible, although at the time the new students were mostly international students. As a result, almost overnight, the Department began to look like an active research organization.

Hal Hopfenberg made an equally important change in the Department. At the time, the attention paid to teaching was also at a low ebb. I don't think the teaching was really bad, but it also wasn't good. Very little attention was given to teaching and it was almost never discussed. Hal was our first really good teacher. Not only was he a good teacher, but also he set a new standard for teaching, and I think everyone improved. Not only was Hal a good teacher, he was also a good researcher.

Since Hal was from MIT, I assumed that he could take over our transport phenomena courses and bring them up to par. He informed me immediately that he had never studied transport phenomena and could not possibly teach the course. My opinion of MIT underwent a big change. Hal started by teaching ChE 205, (material and energy balances) which had a reputation among the students as one of our worst courses. He changed it, again almost overnight, into one of our best.

Hal has written some of his recollections of his days in the Chemical Engineering Department, which are included here as Appendix D.

Vivian and Hal were recruited and signed up almost before I got started as Department Head, and without much input from the departmental faculty. My goal as a new Department Head was to forget the past few years, bring in some new faculty, and make a fresh start beginning in 1966. I felt I had a good start on this and immediately started working to recruit the senior professor in mechanics. Again without very much input from the faculty, Dr. Len Austin, Professor of Fuel Science at Pennsylvania State University, accepted our offer and joined the faculty in the summer of 1967. Len had a Ph.D. in Fuel Science and was a friend and professional colleague of Robin Gardner. Robin was an alumnus of the Department, (BS and MS degrees) and had joined the University with a joint appointment in nuclear and chemical engineering in 1966.

Sometime around the end of the spring 1967 semester, the Department held a retreat at the Harbor Island House near Wrightsville Beach, N.C., a large house that had been donated to the University by Mr. Walker Martin. It was available for faculty activities and was an ideal place for retreats. The College of Engineering Executive Committee met there for a retreat for a number of years.

The departmental retreat was attended by almost all of the faculty. Len Austin attended even though he had not yet formally joined the department. As I recall, we gathered on Friday afternoon, had dinner at one of the local places, and started our meeting on Saturday. One of the first problems with the meeting was that at breakfast Hal Hopfenberg had acquired some tomato juice and hot sauce and we started the formal faculty meeting with a couple of rounds of bloody marys. Much rowdy and unruly discussion dominated the morning session, and I don't remember that we accomplished very much.

We did discuss some important issues during the afternoon, had an occasional lively debate over when to start the cocktail hour, (Vivian voting for now and others holding out for no sooner than four) finally had a social hour, and went out for dinner. Sometime Sunday morning, thoroughly hung over, we returned to Raleigh. I thought the retreat a success since we did have some serious discussion and good time.

For many reasons, Dr. Austin did not work out as a senior faculty member of our department. There was difficulty on both sides, but I had clearly made a mistake in hiring him without sufficient thought and without faculty input. The Department and I both got lucky and Len resigned before the end of his first year and returned to Penn State. I am sure that if we had followed a normal procedure for recruiting faculty, we would not have hired Dr. Austin.

Having made a very fortunate recovery from that mistake, we started recruiting for both the senior faculty position in mechanics and for a new junior faculty position. At the time, I think, we all agreed that what the department needed was young faculty at the assistant professor level rather than senior faculty, and we concentrated our recruiting at this level. The result was that Rich Felder and Ron Rousseau both joined the Department at the beginning of the 1969-70 academic year. Rich coming from Brookhaven National Laboratory and Ron from a just completed Ph.D. program at LSU. As an example of how things get diluted by time, I doubt that Rich Felder even remembers that he was hired into an NSF position in the field of mechanics. In fact, by the time he arrived on campus, there was very-little emphasis on the NSF positions and those hired under that program were rapidly becoming just NCSU faculty, which was the intent of the NSF Science Development Program.

I should make a brief comment here on the NSF Science Development Program. The program was intended to develop centers of excellence in science and engineering at a number of universities such as N.C. State. The majority of the grant to NCSU went to the College of Engineering and was to develop centers of excellence in materials and mechanics. The grant was of enormous value to the Department of Chemical Engineering. We received funds for two senior faculty positions, one in materials and one in mechanics, two support staff positions, and a fairly generous amount for equipment. The grant came at a very important time for us and gave the department a big boost. Since the majority of the grant money was for salaries, the commitment of the State and of the University was to continue these salaries past the end of the grant. The State did appropriate money for half of the salaries, but defaulted on the other half. As a result, when Ed Schoenborn retired in 1974, the department lost his position.

Both Ron and Rich made extremely valuable contributions to the Department. I think Ron's most important contribution was his ability to adapt quickly to the needs of the Department and to work with others on research. He took the responsibility for teaching our mass transfer operations courses and worked with Warren McCabe on crystallization research, with me on several research projects, and with Rich Felder on writing their very important textbook. Ron was also a good friend to everyone and contributed greatly to the environment, which made the department a wonderful place to work.

Rich Felder's contributions, which still continue, were and are of lasting importance. I don't think Rich had any significant experience in teaching, but he very rapidly became the best teacher in the department, surpassing Hal Hopfenberg in student evaluations. I don't know how he did this, but it must have been a combination of effort, of caring how the teaching was done, and of some native talent. Rich's dedication to engineering teaching is now well known and is the emphasis of the current phase of his career, making his contributions important to the entire world of engineering education. In addition to his teaching, Rich devoted much of his first years to research, collaborating with me and others, some of whom were outside the Department.

I don't remember exactly when I discovered that Ron and Rich were working together on a new text for our first course in Chemical Engineering, but when I saw some of the draft material for the first chapter, I knew that if they could manage to finish it, it would be a winner. They did finish the book "Elementary Principles of Chemical Processes" and it was published by



Wiley in 1978, and a second edition in 1986. It has become the most widely used chemical engineering textbook in print, and has been selected by roughly three quarters of the chemical engineering departments in the United States. Rich has written some of his recollections on the writing of this book, Appendix E.

With the addition of Vivian, Hal, Rich, and Ron, the Department had no vacant positions and we made no further additions for several years. When I consider the things I managed to accomplish as Department Head, it seems clear to me that hiring these four faculty members was the most important. I think I knew then, and I do know now, that the most important thing a department head can do toward departmental development is to bring in the right people and then try to create an atmosphere where they can develop into outstanding faculty.

At about this same time, 1970-1971, Warren McCabe wrote two papers of historical importance and which are included as appendices. "The Origins of the McCabe-Thiele Diagram" was written for presentation at Seminar 32, "History of Chemical Engineering," at the Denver, Colorado meeting of the AIChE in September, 1970, (Appendix F), and "Where Do We Go From Here" was presented at the Southern Regional Conference of the AIChE Student Chapters held at NCSU, April 2, 1971 (Appendix G).

During the year of 1971, the University carried out an extensive self-study in connection with the ten-year accreditation by the Southern Association of Colleges and Schools. As a part of the departmental self-study, Ed Schoenborn wrote a brief account of the recent history of the College Engineering and of the Department of Chemical Engineering. The entire section of the self-study, entitled "Where did we Come From", written by Dr. Schoenborn is given in Appendix H.

Speaking of Ed Schoenborn, sometime around the time he retired, I remember his stating what was apparently one of his rules of living, which became known as Schoenborn's first law. Ed was not a real heavy drinker, or an alcoholic, or anything like that, but he did frequently overdo things, and often had a hangover. He also had frequent colds and other ailments and sometimes didn't feel very well for legitimate reasons. His law says "that if you don't feel better by lunch, then it's not a hangover".

I had intended to include a lot more of something like an official history of the department, but it is now the fall of 1999, and I am going to print this as it is.

## APPENDIX B

### THE SENIOR CLASS PARTY OF 1949

The department gave a party for the class of 1949. Ed Schoenborn invited the class with a poem and many of the class members responded with poems. Ed's invitation poem and a sample of the responses are given below:

May 12, 1949

Dear Forty-niner,

O come and with us congregate  
On Wednesday eve of June the eight  
To bid goodbye to N. C. State  
And end of studies celebrate.

At half-past six in Winston Hall  
Our unit op. lab sheds its pall;  
So present be when sounds the call:  
Of "Come to supper, one and all."

In sending you this invitation  
To our farewell celebration,  
I express appreciation  
For your fine cooperation.

We've had our ups and downs, I vie,  
But to this fact must testify  
The class of '49 ranks high  
By any norm one can apply.

With class and labs now at an end  
Slide rules forget and then - unbend.  
So if you find you can attend  
R.S.V.P. I hope you'll send.

Cordially,

Ed Schoenborn

(Poeta nascitur, non fit.)

An invitation must have been sent to the Chancellor. The following reply is from Mrs. John William Harrelson, 1903 Hillsboro Street, Raleigh, North Carolina.

To pretend that the Harrelsons understand all about plastic  
Would be uttering a statement entirely drastic,  
But, as to having supper with a "poeta nascitur, non fit"  
And his graduating students, we don't mind a bit!  
So - expect us on June 8th in your lab,  
And we'll be happy to join your chemical confab!

From one the students, Doover Ketchie,

Dearest adorable old Doc:  
Whose head is as bare as a Rock.  
How about a job? Even on a Dock,  
To get my watch out of Hock.

Next June 12th, I espy.  
With high hopes to make before I Die,  
To be a salaried person in July,  
And with a wife, Multiply.

To school four years to educate  
Off to Meredith to cultivate  
On to 2604 Dover to assassinate!  
And with you to celebrate.

(Me y usted esta unas Helluva Poets)

Note: 2604 was the address of Professor Phil Pike.

And finally a reply from Charlie Plank, now Professor Emeritus at The University of Louisville.

No poet am I  
But I'm not shy  
So here I go  
To give it a try.

And so I say to you, Doc,  
For your treat I'd gladly hock  
Perry, slide rule, charts and all,  
This I'd do without appal.

On June the eight  
I'll be glad to help celebrate  
The end of studies  
That here meant to annihilate.

With this line I begin to close  
This epistle, which so irritates the nose.  
But you, Doc, have no gripe at reading this tripe  
For I say your ode was equally as ripe.

Hopefully (to say the least),

Charlie Plank

## APPENDIX C

### COMPUTING IN THE COLLEGE OF ENGINEERING

Dr. Donald C. Martin  
March 1996

Here are my recollections of the history of computing as we developed academic facilities for the School of Engineering in the dark ages. Some dates may be slightly in error, but I believe the chronology is correct.

I came to NCSU in September 1960 as an Instructor working toward the PhD in Chemical Engineering. I believe it was the year 1962 you returned to NCSU that we wrote the NSF proposal to procure two analog computers to establish a teaching facility for the department. At that time, the only computer on the campus was an IBM 650 operated and housed by the Statistics Department, primarily for research. To the best of my knowledge, there was no other computing facility on the campus primarily for student use. When the proposal was funded at half the request, Dean Lampe generously supplemented the grant so that we could purchase both computers and set up the teaching laboratory in Riddick Hall. We decided that the facility should not be restricted to Chemical Engineering students, so we created the Engineering School computer facility. Dean Lampe, and later Dean Fadum, provided the operating funds for student labor, maintenance, etc. My own experience was building a Heathkit vacuum tube analog computer at South Carolina and using it for my Masters thesis. Since I was the only one on the faculty with any experience in analog computation, you placed me in charge of the laboratory. (Maybe it was just because no one else wanted the job) I remember the many committee discussions we had as to whether to buy an analog or digital computer for the laboratory.

I am sure you remember in those early years, no one was sure whether the analog or the digital computers would be the most appropriate for engineering problem solving. When the IBM 650 was introduced, the president of IBM publicly stated that there might be a market for 50 to 100 machines in the country. At that time, the analog technology appeared to be more useful for solving engineering problems because of its real time differential equation solving capability. So, right or wrong, we forged ahead with the analog laboratory. This was the first computer-teaching laboratory on the campus and the first solid-state computer. (I believe the computing center IBM 1410 may have been about the same time, but I need to check with them). This was the first time analog computer simulation courses were taught in the engineering school, and I believe, in the state.

Within about a year, we added a digital computer to the laboratory, an IBM 1620. This was the first digital computer in the school of engineering. It was a solid-state computer designed primarily for process control applications. To illustrate the state of the art during this time period, this machine had 2K random access memory, a card reader/punch and old IBM model B typewriter as the only input and output. It did, however, have a FORTRAN compiler. Programs were written and keypunched on cards. Because of the limited memory, the compiler, a deck of about 800 cards, was read into the computer followed by the user program cards. The computer compiled the program and punched a deck of cards as the machine-readable program, errors and

all. If there were any errors, you threw the cards away and started all over. In the unlikely event there were no errors, you read the cards back into the computer and prayed that there were no logic errors in your program. If so, you started all over. The remarkable result of this acquisition was the number of students and faculty who learned FORTRAN in spite of the difficulties and kept the system busy day and night.

About two years later, the 1620 was replaced by an IBM 1130. This was a significant improvement in that it had 8K of memory and a removable 1/2 megabyte disk. The primary input was still from punched cards, but there was no longer any necessity for a cardpunch output. Since we could accommodate many more students, we made the first use of television to teach computer programming. Because of the demand, I made a series of FORTRAN tapes on a reel-to-reel tape recorder and trained bright undergraduate students to monitor and help other students learn programming. They held teaching sessions every day of the week using the TV tapes as background, reinforcing them with problem sessions. We were able to introduce thousands of students and faculty to digital computation over the next few years. The computer science department was not in existence until 1969.

One of the interesting features of the 1130 was the storage access channel. This was a precursor to the DMA or Direct Memory Access features used for high-speed disk and display access on today's computers. Since the best feature of the digital computer was number crunching and the best analog feature was high-speed simulation of differential equations, we decided to interface the two computers and have a hybrid computation facility. With one electronic technician and a number of very bright students, we developed the first hybrid simulation facility in the state. I received a \$210K grant from NSF for the express purpose of developing an undergraduate simulation laboratory. This doesn't seem like a significant grant by today's standards, but remember that students were only paid \$1.50 per hour in those days. The system developed had 17 student terminals and could display and store simulation outputs for each student in about 20 milliseconds. This simulation system received national recognition and I assisted in installing a similar system in Sweden.

In 1970 (about) I received a \$300K grant for personnel and equipment to develop a campus wide data acquisition system. At the time, the campus was using the Triangle Universities Computing Center (TUCC) for essentially all academic and research computing. There were almost no interactive terminals. Data and programs needed to be punched on cards and transmitted to TUCC for analysis. Output was then directed back to one of several printers on campus. My idea was to connect the analog or digital output from the research device to a central minicomputer, convert it to punched card images and send it directly to TUCC over telephone lines for processing. There would no longer be any need to spend hours punching cards manually and the results could be analyzed and returned expeditiously. Results could be sent back to the researchers laboratory if he had a printer available. Because this significantly expanded the scope of the engineering school laboratory, I convinced the university administration to create the University Systems Analysis and Control Center (USACC), which included the school-computing laboratory. The system we designed utilized the first minicomputer on the campus – an IBM System 7. This system had extensive analog and digital input and output channels as well as serial communications features necessary to send data to and from TUCC. Again

employing undergraduate students, we were able to solve this problem for many researchers on the campus, including those in physics, chemistry, nuclear and chemical engineering.

In 1974, I was appointed head of the new Computer Science department, but continued to direct the operation of USAAC. In 1975, Larry Monteith, then head of Electrical Engineering, and I wrote an NSF proposal to obtain the first of the new breed of scientific computers, the DEC VAX 11/780. This was the first cooperative venture between Computer Science and Electrical Engineering and also the first such computer on the campus. This computer was effectively used by researchers from many departments and USACC eventually became the University Graphics Center.

In 1975, along with Jim Powell. I instituted the first computer outreach program to high schools in North Carolina. We installed analog and digital computers in an old library bookmobile, and hired Joyce Hatch to run the program. She conducted demonstrations and training sessions at many high schools throughout the state. The rapid growth of computer science and engineering in the late 70's and early 80's completely overloaded computer access for undergraduates. With a special grant from the governor and legislature, I developed the first computer-teaching laboratory, which utilized multi-user microprocessors. Each 68000 microprocessor system supported 5 users and had an attached printer. The complete system supported 60 simultaneous users. We wrote the operating system software and a simulator so that students could use the system in assembler language courses. It also supported FORTRAN and Pascal for beginning courses in computer science and engineering. With the advent of reliable networking and reasonably priced Unix workstations, the system was replaced in the late 80's.

## APPENDIX D

### Recollections of Dr. Harold B. Hopfenberg

In the fall of 1965, a rakish, 48 year-old distinguished lecturer was on an American Chemical Society tour of New England. The US Army Natick Laboratories was on the list of stops. The lecturer was greeted at the labs by a young first lieutenant who had been assigned to be the briefing officer for Natick Labs, providing visitors to the labs with an overview of the lab's mission, resources, and activities.

The lecturer was Vivian Stannett and the briefing officer was Hal Hopfenberg. Thirty years later, Stannett and Hopfenberg would have co-authored 55 papers in the general area of polymer science; all based upon their 29-year cooperative effort within the Department of Chemical Engineering at North Carolina State University. But before the career of cooperation began, Hopfenberg would spend most of 1966 in Viet Nam as a captain in the US Army and Stannett would complete his tenure as Associate Director of the Research Triangle Institute's Camille Dreyfus Laboratory. Stannett and Hopfenberg hit it off as colleagues and a warm and productive relationship, begun in the improbable surroundings of the briefing room of the US Army Natick Laboratories, was formalized as Stannett actively recruited Hopfenberg from Viet Nam to join the effort to build an extensive and ambitious polymer science program within the Chemical Engineering Department. They arrived in the Riddick Laboratories simultaneously on February 1, 1967.

Stannett's impact upon the department was immediate and explosive. Within months, laboratories were renovated, equipment was built, and grant applications were awarded. Students and post-doctoral fellows from around the world were recruited and each, in turn, arrived at the Raleigh-Durham Airport needing an apartment, furniture, frying pans, and a used car. Within two years, Stannett's network of international scholars would include representatives from Scotland, Ireland, and England (Stannett, an Englishman, distinguishes between the historic components of the UK), Japan, Italy, France, Sweden, the Soviet Union, Poland, Holland, Rumania, and Korea. Frying pans and used cars were exchanged between succeeding generations of post-docs and a stable steady state was achieved with approximately 25 Stannett research associates in residence at any time.

More than simply people, programs, recognition, activity, and papers, Stannett contributed a viewpoint of humorous and unbridled optimism as well as support for his colleagues and students. If a student had nine areas of weakness and one area of strength, Stannett could only recognize the area of strength and would expose the student to challenges consistent with his or her strength. If a student had an experimental green thumb then the student would live in the laboratory and never be confused by theory. If a student were clumsy in the laboratory, and facile with calculations, the student would live at his desk with pencil and paper. Remediation had no place in Stannett's vocabulary. When departmental crises arose, Stannett could always see, feel, believe and articulate, the validity of the opposing points of view and make all the parties to the controversy feel good about themselves and each other.



Ed Stahel had joined the department in the early sixties, and Ed had the outlook, skills, and heritage of a Swiss watchmaker. Stannett involved Stahel in the large program on radiation-induced polymerization, funded by the Atomic Energy commission. Ed led the efforts related to the design and construction of pilot plants for radiation-induced, emulsion polymerization processes. Each of Ed's pilot plants was large, perfectly functioning, Swiss watches. Ed's death, at age 56 in 1989, shocked and saddened the department and left a void of caring humanity which any department needs.

Hopfenberg received his three degrees from the Department of Chemical Engineering at Massachusetts Institute of Technology. But MIT's view of chemical engineering in the 50's and early 60's was quite a bit different than the quantitative focus adopted by Minnesota, Wisconsin, Berkeley, and other leading departments. MIT, consistent with the founding of the profession, focused on applied chemistry. The approach was descriptive and conceptual rather than mathematical. An MIT graduate of the late 50's was not required to take a course in Transport Phenomena and, therefore, when Ferrell asked Hopfenberg to teach Transport Phenomena during Hopfenberg's first semester at State, Hopfenberg responded, "he couldn't pass it, let alone teach it." Hopfenberg received his Ph.D. under the direction of Alan Michaels in the general area of transport phenomena in polymers, belying his lack of formal training in Transport Phenomena. Ultimately, Hopfenberg's research would involve reverse osmosis, ultrafiltration, gas separation, generalized membrane phenomena, solvent crazing, stress cracking, devolatilization, non-Fickian diffusion, and aging phenomena in polymeric glasses. These seemingly diverse explorations, all based, in fact, upon transport phenomena in and through polymers, would attract the interest of industrial laboratories worldwide, and he developed a large and international group of consulting clients. At the peak of their combined activity, Stannett and Hopfenberg would serve more than twenty continuing, industrial, consulting clients.

As the program grew, Stannett and Hopfenberg were able to attract Bill Koros to NC State in 1977 to begin his extraordinary career. Koros stayed at State for six years and then returned to the Department of Chemical Engineering at the University of Texas where he received his Ph.D. under the direction of Professor Donald R. Paul, who, had graduated from NC State in 1961. While at State, Koros contributed enormously to both the substance and recognition of the polymer program. Koros is currently the BF Goodrich Professor of Chemical Engineering and head of the department at Texas. He is the editor of both the Journal of Membrane Science and the proceedings of the North American Membrane Society. The insight of his research and the magnitude of ambitious research program are internationally recognized in the fullest sense.

While Stannett was receiving recognition in the form of national and international prizes honoring his research contributions, Hopfenberg developed a strong teaching program within NC State, receiving three Outstanding Teaching awards from the University between 1969 and 1975. Ultimately, this early pattern persisted and Stannett won virtually every national prize even remotely connected to his work, culminating in his election to the National Academy of Engineering in 1995. Hopfenberg would find his rewards within the University, in the form of the opportunity to serve in an extraordinary string of administrative positions beginning with his selection as Department Head in July 1980. As of 1995, he had served as Associate Dean and Special Assistant to the Chancellor, Executive Assistant to the Chancellor, Athletics Director, Vice Chancellor for Institutional Advancement, and, as of July 1992, as the first Director of the

William R. Kenan, Jr. Institute for Engineering, Technology & Science which was endowed by a \$20 million grant from the William R. Kenan, Jr. Charitable Trust, the largest gift ever given to NC State. When Dean Larry K. Monteith introduced Hopfenberg to the department as its new Head he remarked that the department had a new "head" but, as importantly, it would maintain its old and sympathetic "heart" in the person of Frank Seely, who would put off retirement for a few years to guide the new department head. Frank's guidance was invaluable and his contribution to students, colleagues, department heads, and Deans was irreplaceable.

During Hopfenberg's seven-year tenure as Department Head, he emphasized recruiting of people and resources. He led the recruiting efforts, which attracted Mike Overcash, Peter Kilpatrick, Hubert Winston, Dave Ollis, Ruben Carbonell, Carol Hall, Steve Peretti, Benny Freeman, Christine Grant, and his distinguished thesis adviser from MIT, Alan Michaels, to the department. The first seven years of the 1980's were wonderful years for higher education in North Carolina. Budgets expanded rapidly and for the most part, opportunity for the department was limited more by imagination and intramural resource allocation than by overall resource availability. The department acquired significantly more space, and maybe a slightly larger slice of the institutional pie, than the other department heads thought appropriate. Labs, offices, and common spaces were renovated. A timely investment in the rapidly growing area of biotechnology was handsomely rewarded and the department made its mark both inside and outside of the University in this important new area of focus. Recruiting graduate students as well as talented faculty became a new priority and the department, for the first time, looked to other universities as the prime source of student talent. The department provided leadership resulting in the formation of the Southeastern Regional Fellowship Program in chemical engineering. This program, coupled with significant support from industry through related programs such as the DuPont Fellowship program, resulted in the development of strengthened networks, which were useful for recruiting human talent, and related recognition of the department. The department invested heavily in communications materials such as award winning, professionally produced brochures and flyers to tell the department's story of newfound confidence and growth.

During the 1980's, the North Carolina legislature provided special funding and new positions for recruiting and retention of women, minorities, and faculty with defined programmatic interests in concert with university initiatives such as biotechnology. In addition, the legislature provided handsome resources to recruit faculty, not currently at the University, who were truly distinguished, internationally recognized, card-carrying members of one of the National Academies. With the generous resources allocated to these programs, the department added positions, which permitted the recruiting of Ollis, Hall, Winston, Michaels, and Grant.

Inflation and, therefore, raises were typically large during the 1980's. Special salary increases in the range of 15 percent, quite apart temporally and conceptually from the July 1 annual salary increases, would be legislated for the "top performers" within academic departments. It was a time for department heads to look good. Resources and opportunity flowed in the 80's and, if life's lessons teach the cycling of times, the difficulties of the 1990's should not have come as a complete surprise.

## APPENDIX E

### ORIGINS OF ELEMENTARY PRINCIPLES OF CHEMICAL PROCESSES

Dr. Richard M. Felder  
June 1998

I joined the N.C. State University faculty in July of 1969, and Ron Rousseau joined in August. In 1972 Alan Lesure of John Wiley & Sons proposed to Ron that he write a stoichiometry book. Ron thought it sounded like a fine idea and said sure, and when he later asked me if I'd like to come in on it with him, I thought it might be fun and said sure. Although I was slightly older and considerably wiser than Ron and had that critical extra month of academic experience, neither of us knew anything about writing textbooks, and by the time we began to understand what we had committed to we had both invested too much time and energy to back out.

The first edition of Elementary Principles of Chemical Processes made its appearance in the spring of 1978. The book has prospered since then-it's been used by well over 100 departments in the United States and an indeterminate number of other departments in the rest of the world. Its success is not something we could have anticipated when we were writing it; in fact, we doubted that we'd ever finish it, among other reasons because it seemed almost certain that one of us would first murder the other. We managed to restrain ourselves, however, and we're still friends 29 years later as we struggle to finish the third edition, which we're determined to do sometime before Halley's comet returns, although we've given our editor a slightly earlier date to mollify him.

Here is a chronology of the book's history, as closely as I can recall it. If we ever publish this I'll get Ron's input, but since this is being done at the last minute I'll trust my recall of events ... besides, he'd probably just hack up my writing, like he always does.

1972. RWR starts work on A First Course in Chemical Engineering (working title), and is soon joined by RMF.

1973. We write an outline and a sample chapter and send it to Wiley for review. The reviews are mixed. The chemical engineering editor at the time, Thurman Poston, encourages us to continue, but says that we'll need to get more chapters reviewed before he can issue a contract. We continue writing.

In the fall the students in CHE 205 get drafts of the first few chapters as their course text. They discover about 25 mistakes per page and any number of glitches in the problems. The course ends before we ever get to energy balances. (I hope they eventually learned them somewhere.)

1974. We send about five chapter manuscripts off for review and get back two good reviews and one assuring the publisher that the book has few redeeming features and no future. Post

declares that he needs to see still more chapters before offering us a contract. We reply that perhaps it's time for us to open up discussions with McGraw-Hill. We get a contract by return mail.

One of the reviews is so well done, with so many constructive suggestions, that we persuade Post to line up the reviewer to critique the rest of the manuscript for us. The reviewer turns out to be a pair of professors from Iowa State John Stevens and Dick Seagrave. We owe a great deal of the book's success to the input we got from those two, even though we still bitterly resent the fact that no matter how many chapter-end problems we wrote they always wanted more.

1975-1977. We continue to write chapter drafts, class-test them, find glitches, write new drafts, class-test them, and so on, until in the spring of 1977 we declare the manuscript perfect and send it off to Wiley. Much of 1977 is spent proofreading galleys and pages and writing a solution manual. We are extremely careful about the proofreading and are quite sure that the book will be almost error-free.

Before we send the manuscript off, we set out to select a permanent title. Neither of us has any brilliant ideas, and so what we do is write the words Basic, Fundamental, Elementary, Introduction, Methods, Principles, Elements, Chemical, Engineering, Material and Energy Balances, and Processes on pieces of paper and shuffle them into every combination that makes any sense at all. When we get to Elementary Principles of Chemical Processes we look at each other, shrug, say "Good as anything else, I guess" (or something equally enthusiastic), and make that the title.

1978. The first printing appears. In the fall we use the book in its published form for the first time and offer the students 25 for each previously undiscovered typo or mistake they come up with, in the unlikely event that any survived our proofing. We stop counting after 200.

1980. By its third year the text has taken over most of the market. By the third printing we are offering \$1 for each new mistake and are not getting many takers.

1980-1998. The text continues to enjoy success. A Spanish translation, the second edition (1986) a Chinese edition, and an International edition all appear. We live happily ever after.

The only other point that might be of interest concerns who did what. We generally don't discuss this, but in honor of the occasion I'll reveal it. Problems containing weird characters with unpronounceable names are mostly RMF. The case studies are all RWR. The rest of the book bounced back and forth between us so much that it's impossible to ascribe any of it to either author, except that any remaining mistakes are Ron's.

## APPENDIX F

### THE ORIGINS OF THE McCABE-THIELE DIAGRAM

Dr. Warren L. McCabe  
September 1, 1970

Paper for presentation at Seminar 32, "History of Chemical Engineering," Denver, Colorado, meeting of AIChE, and, Instituto Mexicano de Ingenieros Quimicos, September 1, 1970.

Sidney Kirkpatrick has just pointed out that historical events occur when people interact with their times. This paper is an attempt to show how the McCabe-Thiele distillation diagram evolved from such a series of interactions. From scanty records and fallible memory, the evolution of the method is described from the standpoint of that individual who was the fortunate beneficiary of the influences brought to bear upon him under the circumstances of his environment.

The first circumstance was the experience given to a young future chemical engineer in the year immediately following his graduation from high school in Bay City, Michigan. The year was 1917-18, which was, of course, the year of our participation in World War I.

As a high school graduate, considering my vocational options, and already strongly attracted by science and engineering, I was motivated toward the choice of chemical engineering by the public interest aroused by the crisis of the shortage of dyes, drugs, and fine chemicals that was the effect of the British blockade of Germany. At that time, a close family friend, Mr. C. T. Clark, was available for some top-notch career guidance. Clark was a Division Manager of the Dupont Company. He was responsible for the operation of two hard-wood distillation plants, one in my hometown, Bay City, and the other in Grayling, Michigan, about 100 miles further north. Clark was an unusual man. He was an experienced knowledgeable, and dedicated chemical engineer. He lost no time in endorsing my preliminary idea about chemical engineering. I then approached him for a summer job in the Bay City plant; he offered me a year's job in the Grayling plant. One of his major points was that Grayling was the proud possessor of some of the world's most advanced distillation equipment, in the form of a multi-column system made in France by the Barbet Company of Boston.

I accepted Clark's offer. The Grayling plant was all that he claimed it to be. The wood distillation plant of that era was rich in processing equipment, and all of this was of much interest to me. But the six tall copper columns of the Barbet system were my major delight. The system produced pure methanol, refined methanol - acetone azeotrope, and two side streams, one of light oils and the other, a crude allyl alcohol mixture. It had sections for treating the streams with acid and alkali. It discarded a well-exhausted waste. Except for manual control and primitive instrumentation, it would pass muster today as a modern bubble-cap system.

Winter nights in North Michigan are long and cold. I used many of them to prepare, from the actual blueprints of the plant, a complete flow diagram, showing all columns, heat exchange equipment, piping, pumps, and controls, with appropriate color coding for pipe lines.

Also the plant management had difficulty in finding sufficient operators to keep the plant running under wartime pressure for production. My boss was a young chemist from the University of Virginia, Mr. H. Clay Hodgson. Several times the two of us were pressed into service to operate the Barbet system. More than once I had the experience of starting up, and then lining out the unit. Armed only with high school courses in physics and chemistry, I could not, of course, understand just what was going on in those big columns, but the experience gave me the strongest possible motivation for finding out.

The preprofessional year at Grayling, where I picked up background and familiarity with a sizeable fraction of what we now call unit operation equipment, with a special involvement in distillation, was one of the most influential years I can remember.

The second important circumstance was my five years at the University of Michigan. Here, I had other notable experiences. One of the professors of Mechanics at Ann Arbor was Mr. John Airey. Again I was influenced by an unusual man. Airey was a student of Osborne Reynolds, at the University of London, and through Airey's influence I can claim the rather tenuous relationship of being a kind of technical grandson of Reynolds! Airey's teaching was decades ahead of the usual method of instruction in engineering of the day. He had a firm grasp on the basic idea of what we now call engineering science. The word "model" was, of course, not used in the early twenties, but Airey had an equivalent approach, and held firm to scientific fundamentals in all that he taught. More to the point was a technique that he used for a graphical method for following the forces through a hinged, statically determinate truss. One started with a reaction at one end, drew lines parallel to the directions of the members, and proceeded to the other end. All stresses and their directions are then read from this diagram.

Also at Ann Arbor, the evolving unit operation concept was avidly followed by Walter L. Badger, Eugene H. Leslie, and Edwin M. Baker, who alertly taught the material as it appeared, and from which I learned. Also, Leslie and Baker had a sound but complicated method of distilling column design, about which I was informed.

The third set of influences on the growth of the McCabe-Thiele method was the inspiration, encouragement, and support given to me by the galaxy of creative men in the Department of Chemical Engineering at MIT, where the distillation paper was written and edited, and from which it was published. Not only were there strong, indirect influences; there were specific aids as well. At MIT the fascination with distillation generated at Grayling, and the background added at Ann Arbor, surfaced in the form of a conviction in my mind that there must be a better way of analyzing the performance of distillation equipment than any of the complicated methods used at that time. Especially strong was my conviction that much simpler methods of teaching the subject to all chemical engineers should be forthcoming.

So, while thinking about this, I talked to Warren K. Lewis and described to him what I was up to. I told him that a step-by-step method analogous to the graphical technique for the pinned

trusses taught to me by Airey might be used for the distillation problem. Lewis thought a minute and said, in effect, "I think you are on the right track. The equations are linear in both situations. Keep at it."

Preliminary drafts of the paper were reviewed by C. S. Robinson, W. K. Lewis, and W. C. McAdams. All gave the paper searching and detailed reviews, and made constructive comments and suggestions, which greatly improved the presentation in the paper.

The fourth, final, and most important factor in the successful conclusion of these studies was my great good fortune in acquiring Ernest W. Thiele as a collaborator and co-author. Our Joint effort was a classic example of a creative effort of two young men of complementary personalities, methods of thinking and insights. Our collaboration came about in this way. Ernest and I had known each other quite well for some time before we joined forces. Soon after I had talked to Lewis, there came Ernest's oral doctoral examination. At that time Lewis told Ernest that I was active in working out a new method for distillation. Ernest also was much interested in this subject, and Lewis suggested that he, Ernest, talk with me. Just before that time I had conceived of the linear operating line and the stepwise induction. The operating line concept came more or less "out of the blue" when I was looking at the usual x-y diagram for ethanol-water appearing on p. 598 of the first edition of Walker, Lewis and McAdams "Principles of Chemical Engineering." It suddenly came to me that the equation

$$y_{n+1} = x_n O_n/V_{n+1} + Px_c/V_{n+1}$$

was, for the special case of constant molal overflow, simply a linear equation in  $x_n$  and  $y_{n+1}$  having a slope  $O/V$  and a y-axis intercept  $Px_c/V$ . Immediately I drew a typical line for assumed values of  $O$ ,  $P$  and  $x_c$ , on the figure in the book. This line still is there. The second concept, the step-wise construction between the equilibrium and operating lines immediately followed. When Ernest talked to me, I showed him these results. It generated in him considerable excitement (probably more than I had myself). Immediately, I asked him to work with me on the details and to join me as co-author.

The evolution of the final paper-was a true joint effort. Just what was "McCabe" and what "Thiele" was of but little importance at the time, and of no importance now. Certainly, neither of us expected that one of us would be talking about the origin of our work in Denver forty-five years later! The q-line and the construction on the two-feed column were worked out by Ernest. The rest was strictly "McCabe-Thiele."

As the paper approached completion, I spent considerable time on the bibliography. My search resource was the Widener Library at Harvard. I have never been ashamed of the result, as I believe that the list of references included all pertinent papers printed at the time. For example, I found the well-known (by now) papers by Ponchon and Savarit, and introduced them in our bibliography. Later Ernest wrote another classic paper, which brought these more general methods into the mainstream of American chemical engineering.

I presented a summary of the paper at Johns Hopkins University, Baltimore, at the spring 1925 meeting of the American Chemical Society. There were, I reflect, about fifty people there.

McAdams, Dr. E. D. Reis, and I came from Cambridge together. We were joined by Eger Murphree who at that time was employed at Syracuse, N. Y. Ernest, who had just started his life career with Standard of Indiana, came east from Whiting. All of these men were at the presentation of the paper. Soon after, Murphree published his classic paper on individual plate efficiencies, in which he used the graphical representation of a single plate as a basis.

The paper was submitted to Harrison Howe, editor of Industrial and Engineering Chemistry. It was reviewed (and very thorough, too) by the redoubtable W. A. (Ollie) Peters, Jr., who was one of the top industrial people in the distillation field. Considerable pressure was applied to us to shorten the paper, and a few rather insignificant paragraphs were deleted. Incidentally, all the figures except those for a worked-out example on ethanol-water, were drawn by Thiele, others were drawn by me. And that, insofar as I can remember it, is the story of the McCabe- Thiele diagram.



## APPENDIX G

### WHERE DO WE GO FROM HERE?

Dr. Warren L. McCabe  
April 2, 1971

Remarks given at the Southern Regional Conference of the A.I.Ch.E. Student Chapters, North Carolina State University, Raleigh, N.C. April 2, 1971.

Engineering has just ended its most romantic era. During the past two decades, good engineering jobs have been available, salaries and working conditions good, money plentiful, public opinion of technology benign. Engineering tasks have been amenable to mathematical modeling of purely physical situations, and all the drudgery was taken over by computers.

Suddenly, immediately after the successful completion of the most spectacular engineering effort of all time, this golden age ended with a suddenness that reminds me of the collapse of the euphoric Coolidge era in 1929. Once the human interest of the first moon shots was satisfied, people began to ask: "Just why did we do this, anyway?" It was recognized, paradoxically, that the space effort paralleled the rapid buildup of a set of severe social, racial, environmental, and conservational problems that were not being solved and which, superficially, seem to have originated as a consequence of the very success of our technology and from our ability to improve our material standard of living. The same engineers who were so successful in the space program have had the traumatic experience of being those most severely hurt by the change of mood.

It is reasonable to ask, not just what has happened, but also where do we go from here. What I have to suggest is one chemical engineer's attempt to answer the question.

If I have any business in hazarding an analysis of a question about the future, it is only that by the luck of the draw I happened to be born at the time I was-the last year of the nineteenth century. Actually, the nineteenth century world did not cease to exist on January 1, 1900, but persisted without discontinuity until World War I. It was irrevocably destroyed in Europe in 1914 and here in 1917. I was in the tenth grade in 1914 and I graduated from high school in 1917, so my formative years were essentially vintage Victorian. World War I was followed by a relentless succession of revolutionary changes in science and technology, and in political, economic, and social structures, which influenced all aspects of the interaction of man-with-man and man-with-planet earth. Our worst depression, four wars-two of them the most murderous and destructive in history, and one the stupidest-, the rise of malignant militaristic police states, the opening of South America, Asia, and Africa, the liquidation of colonial empires, the population explosion, the cold war, all have contributed to the production of more social entropy than any other time period of comparable length in history.

In living through such a series of changes and participating in them in a small way, one accumulates memories of events and people, and gets a certain perspective of the past that may be used in evaluating the present.

I am not trying to predict the future. This simply is not possible. The big historical changes come out of the blue, and cannot be foreseen. Some examples are: the discovery of miracle drugs, the discovery and reduction to practice of nuclear reactions, the development of big computers, the perfection of microwave transmission of information and entertainment, the applications of the transistor, and the discovery of the laser. What may be done is to try to sense the kind of challenge that is looming in the immediate future and to establish a frame of reference and an approach that has some promise of helping to meet the challenge. Meeting challenges is not new to the engineer. The history of our profession consists of challenges encountered and successfully met. The more important of them are:

1. Ground transportation, first by roads and canals, then by railways, and then by automobiles and superhighways.
2. Power generation from fossil fuels and engines and turbines for production of power.
3. Generation, transport at ion, and utilization of electrical energy, and the communication of information by currents and electromagnetic waves.
4. Synthesis of new materials fuels, chemicals, drugs, and the development of large-scale processes to make them.
5. Application of nuclear energy.
6. Development of the large computer, and the creation of mathematical models for the analysis of large systems.
7. Air and space transportation, from Kitty Hawk to the moon.

Personal experience illustrates the impact of unexpected discoveries. During World War II, I was involved in work in two of the Divisions of the National Defense Research Committee. One took me to the University of Chicago, and the other to the University of Pennsylvania. At Chicago, I was following work on defense against gas warfare. Right next to our group there was a highly secretive organization called the Metallurgy Laboratory. No one, of course, knew what went on there. In due course, the veil was lifted. It turned out that Fermi and his associates were building and testing the first self-sustaining nuclear pile, and Seaborg and his group were discovering plutonium and investigating its properties.

At Philadelphia, I was engaged in studies on the manufacture and use of liquid oxygen for various applications by the armed services. There was no secret about the fact that across the street a collection of some thousands of vacuum tubes was being assembled to the first electronic digital computer.

Obviously, nothing in previous experience gave any one an inkling of the development of either the atom bomb or the computer. We would now be living in a far different world if neither of these two events had occurred.

As a starting point for a look ahead, let me state and compare two definitions of engineering. The first is the definition of chemical engineering given in the Constitution of the A.I.Ch.E.

Chemical engineering is the application of principles of the sciences, together with the principles of economics and human relations, to fields that pertain to processes and process equipment in which matter is treated to effect a change in state, energy, or composition.

The second definition is an example of the kind that has been used historically for engineering generally. It is

The engineer develops, designs, and builds feasible and operable devices, structures, and systems all of predictable performance, cost, and effectiveness.

Although the two definitions overlap, they express different points of view. The first emphasizes the method and tools of the engineer; the second states the mission of engineering. In my opinion, the second is the more pertinent to the present. Just as any other calling or profession, engineering serves the body politic and must be prepared to dance to its tune. The public has no interest in how the task is performed. It cares only for the results. Whether the engineer uses advanced mathematics and sophisticated science or relies on experience and common sense is a matter of indifference to those who are supporting us. And this fact is important in plotting a course.

Considering present issues in the light of the past half-century of experience leads me to one conclusion. It is based on the complicated and highly non-linear makeup of the biological species to which we all belong. We call ourselves *Homo sapiens* or "man the wise". At times one can feel that the name is flattering. It is true that, when man has applied his hand and head to objective experimentation, careful study and observation of nature, and constructs and uses the great machinery of mathematics and science, he has advanced, over the millennia, from the crude stone axe of the cave man to the moon and back.

Along another dimension, however, the record of the interaction of *Homo sapiens* and planet earth is mixed. I refer to his emotional properties and behavior. Here the spectrum of his accomplishments and failures covers another wide span. Drawing upon his emotions, *homo sapiens* has fashioned his great religious, moral, and ethical systems; he has shown how benevolent are kindness, love, and consideration for nature and his fellow man; and he has created his masterpieces of literature, art, music and architecture. Antithetical to his wonderful emotional creations, as Mr. Hyde is to Dr. Jekyll, is man's horrid record in giving way to his other emotions, greed, envy, fear, anger, hatred, and cruelty. These have led him to violence culminating in the hideous history of man's inhumanity to man. His emotions cover the range from the gates of heaven into the depths of hell.

Until now, engineering challenges have been largely solvable by the control of the inanimate forces of nature with little or no involvement in the emotional hang-ups of man. There were no farmers with shotguns waiting for our astronauts when they landed on the moon, and nobody objected to their litter bugging while there. Even if possible, Shepard did not have to holler "fore" when he made his 7-iron shot.

The big change in the world of the engineer is that the problems now laid on his doorstep do involve emotional factors that must be taken into account. No longer can his problems be solved by the sole use of mathematical models, computers, and system analysis. Emotional variables are not subject to such objective quantifiable parameters. There are no mathematical models or computer software for either the creative emotions or for hate, greed, fear, or envy. The facts of life are becoming more obvious daily. No longer can the engineer ram a super-highway through a ghetto; nor plump a jet airport down in a game preserve; nor locate a chemical plant willy nilly adjacent to a residential area; nor fill the skies with a fleet of SST's. The emotional content of such situations shows in the uncritical rejection by a large and influential section of the body politic of all science and technology. In spite of such scornful rejection of rationality, which is itself an emotional act, I am certain that the constructive accomplishments of objective and rational methods have been of tremendous importance in the improvement of the conditions of human life, and I do not agree for one minute that benefits from such rationality have peaked out. It is equally clear that, although sciences and technology still are necessary, they now are far from sufficient.

What corollaries may be deduced from the broadening of the base of engineering into the emotional climate of man? Several seem clear:

1. The second definition of engineering, quoted above, appears more relevant than ever. It is true that the A.I.Ch.E. definition is saved from obsolescence by the phrase "human relations" contained in it, but I have reason to believe that it really refers to the human problems encountered in the management of human organizations, rather than to an expansion in the foundations of all engineering
2. The engineer is not a specialist in the emotional field. Fortunately, the introduction of a sizable core of curricular time to humanistic and social studies has anticipated the new trend, and emphasis and strengthening in this area are indicated. Nevertheless, the real practical specialist in the emotional field is the politician. Not all emotions are political, of course, but politics is largely emotional. In the past, the engineer has not exactly been an admirer of the practical politician, but from now on he must learn to respect and work with this important and interesting segment of the human race. Not only must the engineer rap with the politician; some engineers will have to become politicians, as in the past some engineers found it necessary to become scientists and applied mathematicians.
3. The portion of man's emotional spectrum that I refer to is, of course, the creative, moral, and constructive end. Herein lies the real challenge. There is no doubt that the alliance of science and technology (which is itself totally amoral) with the cancerous emotions of war has been horribly effective in its own terms, or that it has reached its ultimate limit in giving man the means of obliterating all life on the planet above the level of the cockroach. If only we can do as well at the other end of the emotional spectrum! I do not wish to suggest in any way that those who were responsible for the initiation and conduct of the program leading to the production and use of nuclear bombs were irresponsible people. In fact they included the most humanistic and brilliant scientists, engineers, political leaders, educational executives, and managerial talent we had. Ironically, the initial stimulus for the development was provided by a blue-ribbon group of physicists

including Albert Einstein, one of the gentlest and kindest of men. Their motive in urging work in the field of atom bombs was simple: the well-grounded fear that Nazi Germany would make atomic bombs before any one else and reduce the entire world to thralldom. The crowning irony appeared when the war ended and a mission hurried to Europe to see how far the Germans had gone. The mission found that the Nazis had done nothing with atomic energy.

4. Does our present situation mean that our own branch, chemical engineering, is now obsolete? Not at all. Consider one of the major problem complexes in the public eye: conservation and pollution. The chemical engineer, as described in the A.I.Ch.E. definition, converts raw materials into useful products, and he does so in an optimum manner. What is wrong with this? It simply is that processing as now done stops too soon. The criterion of optimization currently used is to maximize rate of return on investment, and to stop when the optimum has been reached. This method probably will have to be modified. I doubt that it will be tolerable in the future to skim the cream from a valuable raw material and literally throw the residue out the window. Both sound conservation and environmental integrity are violated by the oversimplified method now used.

The cure for this is clear; more processing, not less. The materials now discarded must be recycled, reclaimed, or converted to additional useful products not necessarily of maximum profitability as measured by convention cost accounting. Residues may be discarded only when reduced to a harmless state that will not hurt nature or men.

The emotional input to this problem appears in the allocation of costs. Clearly, the expense of conservation and pollution control is to be great. It could be as much as 50 billion dollars per year, about five percent of our gross national product. The battle lines to establish who is to foot the bill are now forming.

Two powerful tools are available for solving the pollution-conservation problem. One is our trillion-dollar economy and the other our great technological capability. To this must be added the will to pay the necessary price. The worst approach would be to try to solve the difficulties by a purely emotional approach and to start by destroying the means available to do the job.

One other memory comes to mind. In the middle thirties I attended a meeting of the A.I.Ch.E. in Pittsburgh. The principle speaker was a famous editorial writer and commentator. His opening remark was how pleased he was, on passing a steel mill on his way to the banquet, to see smoke issuing from the stacks. He stated how wonderful it was to see the evidence that men at last were going back to work. Such a statement now sounds queer: but at that time it was understandable and everyone there agreed with. People who had not seen a pay check for three years were eager to trade off absolute air purity for food for their families. Ten years later, during World War II, when Pittsburgh was a, great engine for producing war material, a small suburban mill town nearly had to be completely evacuated to prevent mass deaths from steel mill pollution. So it is. The triumphs of one period are the menaces of the next.

These are the problems that you, as budding chemical engineers, must face at the outset of your life work. I say outset because of another teaching of history. While current problems are being solved, new and more difficult ones are generated, and often become acute before people have completely met the old challenges. There is no evidence that this effect won't appear, perhaps more than one, during the half-century of your active lives. Then, maybe you will be in my position today, pontificating to your successors on just what they should do with the messes that you probably will be leaving them to clean up.

## APPENDIX H

### A CONTRIBUTION TO THE DEPARTMENTAL SELF-STUDY REPORT

Dr. Edward M. Schoenborn  
1971

To obtain a proper perspective of the growth and development of Chemical Engineering at North Carolina State it is appropriate to look back briefly to the climate surrounding higher education in North Carolina in the year 1935. The "Great Depression" had wrought havoc with the economy of a predominately agrarian population and, although there is no record of any sizeable default on the bonded indebtedness of the State or of its municipalities, the level of financial resources required to maintain even a modest measure of adequacy for its system of higher education left much to be desired.

Some say that it was by a stroke of genius that the then Governor of the State, O. Max Gardner, conceived of the consolidation of the three major colleges (universities?) into a single consolidated university system, eliminating overlapping of functions yet dividing them among three separate campuses. Despite the trauma that inevitably occurred, it appears that the scheme worked – slowly but surely.

To make a long story short, it must be remembered that, prior to 1935, Schools of Engineering were well entrenched at the University of North Carolina at Chapel Hill and at the North Carolina State College of Agriculture and Engineering at Raleigh. There was no Engineering at the Women's College at Greensboro, a predominately female institution. State College was the Land Grant component of the trio. As might be expected, departments of chemical engineering competed with each other at Chapel Hill and Raleigh. Chapel Hill had some distinguished leadership in persons like McLarin White and Frank Vilbrandt. Raleigh was headed up by E. E. Randolph.

As a result of the Consolidation, all degree-granting programs in engineering were transferred to Raleigh – chemical engineering among them. Chapel Hill retained the programs in Arts and Science, Business Administration, Education, Law, Pharmacy, Medicine (Pre-Med. at that time), the Graduate School, and others. North Carolina State was permitted to offer degrees in Agriculture, Vocational Education, Textiles, Forestry, Engineering, and the like. What seemed clear after the sounds of battle abated and the debris was picked up was that UNC was to be the center of the elite in higher education and in the social progress of the State. It was left to Greensboro to see that its "Southern Belles" were trained in the arts of music, the dance, and home economics. It was left to Raleigh to see that the State was provided with its full complement of tobacco and cotton farmers, textile mill weavers and dyers, surveyors, road builders, brick manufacturers, water works and coal gas operators.

A lot transpired between 1935 and the end of 1945, when Dr. E. M. Schoenborn came to Raleigh as Head of the Chemical Engineering Department. The "Great Depression" began to be

forgotten in favor of the "Great World War II". There is every evidence that North Carolina State did its part in training all sorts of people to do all sorts of jobs during this agonizing period. There were various training programs associated with the war effort such as the diesel program for Naval Officers and special programs for returning veterans. Beginning in 1946, large numbers of GI's and their families returned to civilian life and campus enrollments began to swell again.

Under the able, benevolent leadership of Frank P. Graham, the first president of the Consolidated University, State College began to grow and prosper. Chancellor John W. Harrelson had the good fortune to have at his side three strong deans, all dedicated to the improvement of higher education in their respective fields and to the welfare of State College and the constituency it served. Leonard D. Baver was Dean of Agriculture, Malcolm E. Campbell of Textiles, and J. Harold Lampe of Engineering. There were other members of the Administration, of course, but these three provided the vision and the vigor so greatly needed at the time.

When Lampe became Dean of Engineering in April 1945, he found himself in the rather unique position of having to replace practically all of his department heads since several had already reached the age of retirement while others would do so within a few years. It was clear that he could establish a new regime unhindered and unprejudiced by the past.

The School of Engineering at that time comprised nine line departments: Aeronautical, Architectural, Ceramic, Chemical, Civil, Electrical, General, Geological, Industrial, Mechanical; and four service departments: Engineering Experiment Station, Engineering Mechanics, Mathematics, and Physics. (Chemistry was administered as a service department within the School of Agriculture and Forestry.) To trace the changes that have occurred within the School since that time is interesting and instructive but with several minor exceptions hardly relevant to this report.

The chemical engineering faculty consisted of E. E. Randolph (already beyond retirement), B. E. Lauer (on leave to the Army), T. C. Doody, R. Bright, J. F. Seely, R. L. Overcash (also on leave), and E. M. Schoenborn. Within a few months Lauer and Overcash resigned and it was necessary to secure two young Ph.D.'s who could qualify as Associate or Assistant Professors. K. O. Beatty and F. P. Pike accepted appointments effective September 1946, students began returning to the campus, and at this point the Department started rolling.

The next job was to take a good, hard look at our courses and curriculum with a view toward seeking E.C.P.D. accreditation. It was not difficult to decide that courses of a purely qualitative nature such as Chemical Nature of Engineering Materials, Treatment of Water and Sewage, Gas Engineering, Industrial Oils Fats and Waxes, and the like should be replaced with problem courses emphasizing chemical process principles, the unit operations, thermodynamics, and kinetics. A sequence in the humanities and social science was subsequently introduced. Following an inspection by B. F. Dodge in 1948 our program was fully accredited.

Physical facilities in those days were horrible. The Department was housed on the first and ground floors of Winston Hall and while space was ample there was little usable equipment



to work with. The unit operations laboratory had inadequate services, lighting, drainage, power (440 volts, what there was of it) and headroom. The size or nature of the budget before 1946 was unknown. For the academic year 1946-47, however, the operating budget included \$800 for supplies, \$200 for repairs, and \$2500 for equipment. With this a small shop was set up, some new equipment purchased, and modest renovations made to the laboratories.

Funding of the Department was, and still remains, the major problem of the Dean and his department heads. The line-item nature of the State budget precluded the transfer of funds from one category to another or of retaining unexpended funds from once fiscal year to another. Despite this handicap, budgets did increase from one biennium to another. The rate of increase was far larger for salaries over the years and we finally became competitive with other colleges and universities, particularly in the southeast, in this regard. The maintenance budget increased much more slowly yet there has never been provision for obsolescence of apparatus, equipment, furnishings, and buildings. With the exception of stipends for a few teaching assistants, no funds have been appropriated for graduate work or research per se. This was and still is a major deficiency of the system,

An important consequence of the war years was that the State found itself, during the late forties, in possession of rather sizable surpluses. Wisely, the legislature anticipated the growth in size and stature of its Consolidated University and made generous appropriations for new construction on all three campuses. Thus began the new era of building and expansion that still continues, albeit at a currently slower pace.

For the School of Engineering, the first major acquisition in many decades was Riddick Engineering Laboratories Building, completed in the fall of 1950. The general layout and allocation of space was largely the result of the informal, collaborative effort of Dean Lampe, W. B. Van Note, and E. M. Schoenborn. It was agreed that chemical engineering would occupy the entire east wing, the Department of Engineering Research, the west wing (with some space reserved to the Dean for his administrative offices) and the remaining front portion to be devoted to general classroom use. Of the total appropriation of something over \$1,000,000, chemical engineering was allotted \$100,000 for laboratory furniture and equipment. It is safe to say that at that time, Chemical Engineering at North Carolina State had facilities far superior to those of our sister departments throughout the South.

Having become accredited and having acquired new space and facilities, the next major problem facing the department was clearly that of establishing a reputable graduate program. Few schools in the South granted more than the Master's degree, so it was obvious that to attract high quality faculty and superior students, and to maintain a superior undergraduate program, it was necessary to seek to offer the Ph.D. To fund such a program out of existing State appropriations appeared impossible; to seek legislative support seemed fruitless. To obtain outside support from industry and government agencies was the only way. But the success of such a "bootstrap" operation depended wholly upon the willingness of the faculty to engage in research and personally to secure the grants and contracts to finance it. This the faculty was willing and able to do. Although I like to think that chemical engineering took the leadership in this undertaking, it was aided in the overall effort by a similar appreciation of the problem by several other departments, notably electrical and ceramic engineering, who also felt as we did.

Chapel Hill jealously guarded its control over graduate work on all three campuses and although six department in the School of Agriculture were authorized to grant the Ph.D. it was clearly stated in the State College Record that "The Degree of Doctor of Philosophy is offered in cooperation with the University at Chapel Hill under supervision of the Graduate School of the Consolidated University of North Carolina." Thus, to obtain approval for new engineering programs meant fighting uphill battles with our own Administrative Board, strongly influenced by Agriculture, and then with the Executive Council at Chapel Hill, highly reluctant to approve any proposals from either Raleigh or Greensboro.

At no time during the deliberations that took place was the problem of funding such a new program considered or even discussed. It was simply assumed that each department with the help of the Dean could secure from outside sources the necessary grants and contracts to support it. The need for such support and the means to secure it were, however, early recognized by Dean Lampe and his new department heads. In 1946 the Engineering Research Department, with W. G. Van Note as Director, was established to supersede the Engineering Experiment Station. Although the function of the Station, namely, to aid small industry in the State was retained, a broader and yet more important responsibility was to assist faculty in securing research contracts - primarily from government agencies, to administer the contracts, and to provide those services the departments were not equipped to handle. ERD had some funds of its own which were used to help defray the costs of travel, research assistants, shop work, report preparation, and the like. Van Note, and even Dean Lampe, made frequent trips to Washington and elsewhere with faculty members to help sell proposals and to make new contacts.

Chemical Engineering, mainly through the effort of Beatty, Pike, and Schoenborn, early acquired sufficient support to initiate and maintain an active graduate program. As our sister institutions later developed graduate programs of their own, competition for good students far exceeded the number of assistantships and fellowships needed to support them. The graduate effort resulted in numerous benefits, notably an ability to recruit and maintain an excellent faculty, to upgrade continuously the instructional effort, and to enhance the prestige of the department and its graduates.

The growth and development of the department since 1945 was not without the trials and tribulations that any successful enterprise must endure. Problems always existed but many were capable of being solved by means that are no longer available today. As the state, its system of higher education, and this campus has evolved so have perplexities and vexations increased in number and magnitude. Many of the problems discussed in this brief history are still with us today. It is now our task to deal with these problems in an environment vastly different from that of the past and under a university system that has increased enormously in both size and complexity.

The more recent development of the department, 1965 to 1971, is discussed in detail in a later section of this report.

**APPENDIX I**  
**MEMORIAL ADDRESS**  
**AT**  
**DEDICATION OF THE E. E. RANDOLPH READING ROOM**

Presented by  
Professor J. Frank Seely  
May 6, 1961

Dr. Randolph was born in Charlotte, North Carolina, July 22, 1878. He was educated at Sardis Academy and the University of North Carolina where he won his A. B., A. M. and Ph. D. degrees. He later did graduate work in Chemical Engineering at the Massachusetts Institute of Technology and for one year was a Carnegie Research Assistant, working at the College of the City of New York.

Dr. Randolph held professorships at Lenoir-Rhyne College, Elon College, A. and M. College of Texas, and North Carolina State College, to which he came in 1920. He taught several summers at Appalachian State Normal College and Tulane University. In 1924 the Board of Trustees authorized a Department of Chemical Engineering at North Carolina State College. Dr. Randolph was appointed its head and it was from this position that he retired in 1945. Following his retirement he remained active as a consulting chemical engineer.

During his active career he had industrial and research experience with the Revere Sugar Refinery and with the Bethlehem Steel Company. He served as a consulting chemical engineer at various times for the Carolina Power and Light Company and the Southern Public Utility Company on gas plant problems; for the State Budget Bureau on the quality and specifications of fuels for State Institutions; for the State Department of Conservation and Development on the quality of surface waters for industrial uses, and for the Utilities Commission on gas plant inspections. He was a ranger during one summer in Yellowstone National Park.

He was a member of the American Chemical Society, the American Society for Engineering Education, American Society for Testing Materials, and the American Institute of Chemical Engineers. In 1936 he organized the North Carolina Water Works Association and in 1938 the North Carolina School for Gas Workers which subsequently became the Southeastern Gas Association. He was also a member of the North Carolina Society of Engineers, Raleigh Engineers Club and of the honor societies Tau Beta Pi and Phi Kappa Phi.

Dr. Randolph published two books and a number of articles in technical and scientific journals. He had a keen interest in the utilization and development of North Carolina resources. His fields of research included such problems as water quality and water conditioning, industrial wasters, wood products, fish oil industries, farm product utilization and industrial corrosion.

In addition to being a recognized leader in the field of education and research, Dr. Randolph maintained a keen interest in the welfare of his students. He knew every student in his department and something of his background. He was above all interested in human values and

was never too busy to discuss a student's problem with him. During depression years, he worked many long hours toward securing employment for his and other State College graduates.

Dr. Randolph had little with which to build his department. Financial aid for equipment and supplies was meager. However, his many contacts plus a determination to succeed enabled him to get together the equipment necessary for his laboratories. Despite these handicaps his approximately 600 graduates have been able to compete on equal terms with chemical engineers from other schools throughout the country.

He married Ora M. Huffman on December 30, 1909. Mrs. Randolph shared with her husband a continuing interest in both the affairs of the department and of its students and her assistance and understanding became a significant factor in his success.

Edgar Eugene Randolph, Professor Emeritus of Chemical Engineering at North Carolina State College, died at his home in Raleigh on November 10, 1954.

In his death the Greater University of North Carolina and the State of North Carolina have lost a most distinguished and loyal member; however, through his former students and many associates his personality, his teachings and his interest in human values are being propagated throughout the world.

## APPENDIX J

### Chemical Engineering Practice

Dr. E. E. Randolph  
1934

#### INTRODUCTION CHEMICAL ENGINEERING

Engineering is the practical application of the laws of Physics to machines to produce useful results on a large scale for human comfort and progress. It deals with the transportation of natural materials and forces, the control and conversion of these forces to as to bring about a change in natural conditions that will be of benefit to a large number of people.

Chemical Engineering is that branch of engineering which selects proper raw material, chooses, modifies, or devises processes, designs, and constructs buildings, machinery, and control equipment, supervises operations, provides proper water supply, arranges for most economic use of energy and controls conditions for the transformation of the raw material on a large scale into products necessary for human comfort and progress.

As long as we study the forces of nature, such as gravitation, heat, light, magnetism, and electricity, as natural forces and phenomena, – it is Physics. As soon as we apply these laws to make a machine and use the machine to do useful work, – it is Engineering. To illustrate:

All the principles involved in a classical study of lever, screw, inclined plane, wheel, pulley, as such, is Physics. The employment of these principles in making a machine for the production of power for any purpose or for doing work, – is Mechanical Engineering. The nature and laws of these principles in making a generator, dynamo, motor, the great power houses, electrification of railways, electric communication, and lighting is Electrical Engineering.

The basic principles involved in the constitution of matter and its changes is the subject matter of Chemistry. The energy manifestations accompanying these changes is Physics. The application of the principles involved in Chemistry, Physics, and Mathematics to the designing, constructing, and operating machines and factories to produce chemical products or to carry out chemical processes on a large scale is Chemical Engineering.

The engineering idea suggest production on a comparatively large scale in which considerable money is involved, the interests of many people are concerned, the safety of the operators must be guarded, the community is served, more or less hazards are always potential and must be prevented, leadership in managing men is essential, and technical knowledge of a highly specialized and comprehensive nature is of prime importance. The idea of designing, of construction, of employing the inflexible laws of nature for power, of changing huge quantities of materials into a different form to satisfy the needs of hundreds of people is the essence of the engineering idea. Such a conception makes it apparent that special and rigid training to equip a man for such work is absolutely required.

The leading engineering authorities, national engineering societies, National Bureau of Education, Society for the Promotion of Engineering Education, and leading educational institutions, unanimously divide engineering into five major divisions: Viz., Civil, Chemical, Electrical, Mechanical, and Mining.

Chemical Engineering is not only a training in chemistry and in certain groups of engineering, although it includes these subjects as a foundation along with physics and mathematics, it is itself a definite branch of engineering with its own special field and kind of necessary training.

Of all engineering training Chemical Engineering requires especial training because of the large number of people working in the typical chemical industries, the imminent dangers at all times, the exact control necessary, the nice adjustments of equipment, processes, and conditions, the large amounts of money involved in plants, machinery, salaries, and cost of operation.

All engineering is based primarily on mathematics. Stoichiometric calculations of mass and energy are basic in Chemical Engineering. A knowledge of the methods of industrial calculations is essential, because we must rely implicitly on the integrity of the calculations. We must be able to estimate definitely, for instance, the amount of pressure which will be developed in normal operation in order to know how to design a container, autoclave, or digester to take care of the pressure and to insure a proper margin of safety.

The chemical engineer must be able, not simply to analyze his product to see if he has what he desires, but rather he must control each step of the process so as to make his product come out exactly what he had planned for it to be in quality, quantity, and cost.

Chemical Engineering also comprises the matter of the proper location of the factory with reference to such considerations as suitability of the water, availability of raw materials, transportation facilities, markets, climatic conditions for all-year-round normal production, the type of building needed for the purpose in hand, the proper materials for the building, the materials best suited to the purposes of the machines, containers, conveyors, and other equipment, the lay-out of the plant for the most efficient utilization, the proper routing of materials during the manufacturing processes, the design, and the capacity of the various units of the system, and the accurate control of each unit of the system. The chemical engineer balances one process against another, advantage against disadvantages, input against output, cost against receipts, and one type of operation against another type of operation. He of necessity must find and face sources of loss, waste, by-products, improvement of methods, and safety devices. The chemical engineer constantly strives for the production of a large yield of purer product at minimum cost.

It is evident that special training is necessary for men to meet the requirements of the various chemical industries. From a monetary consideration alone these industries cannot afford to entrust even a single unit of such costly equipment and its operation into the hands of untrained men. Of greater consideration even is the dangers to life of having untrained men in such responsible places.

Report of the Committee on Chemical Engineering Education of the American  
Institute of Chemical Engineers

“Chemical Engineering as a science is not a composite of chemistry and mechanical and civil engineering, but is itself a branch of engineering, the basis of which is those unit operation which in their proper sequence and co-ordination constitute a chemical process as conducted on a commercial scale. These operations, as grinding, extracting, roasting, crystallizing, distilling, drying, separating, and so on, are not the subject matter of chemistry as such or of mechanical engineering. Their treatment in the quantitative way with proper exposition of the laws controlling them and of the materials and equipment concerned in them is the province of Chemical Engineering. Chemical Engineering courses should be based upon an analysis of Chemical Engineering practice into its three essential factors: Namely, knowledge of engineering science, skill of technic of application, and judgment in the appraisal of values and costs, and that the balancing of value and cost is the controlling factor in all intelligent production.”