CHEMICAL HERITAGE FOUNDATION

JAMES R. FAIR

Transcript of an Interview
Conducted by

James J. Bohning

at

University of Texas at Austin

on

19 February 1992
(With Subsequent Corrections and Additions)

James R. Fair

THE CHEMICAL HERITAGE FOUNDATION
Oral History Program

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Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
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JAMES R. FAIR

1920 Born in Charleston, Missouri, on 14 October

Education

1938-1940 The Citadel
1942 B.S., chemical engineering, Georgia Institute of Technology
1949 M.S.E., chemical engineering, The University of Michigan
1955 Ph.D., chemical engineering, The University of Texas at Austin

Professional Experience

Monsanto Chemical Company
1942-1943 Junior Engineer, St. Louis, MO, and Karnack, TX
1943-1945 Technical Service Engineer, Texas City, TX
1945-1947 Development Specialist, St. Louis, MO
1947-1950 Process Engineer, Texas City, TX
1950-1952 Project Manager, Boston, MA, and Texas City, TX

1954-1956 Process Engineer, Shell Development Company, Dayton, OH

Monsanto Chemical Company
1956-1961 Research Section Leader, Dayton, OH
1961-1963 Development Manager, St. Louis, MO
1963-1968 Engineering Manager, Corporate Engineering Dept., St. Louis, MO

1964-1979 Affiliate Professor of Chemical Engineering, Washington University, St. Louis, MO

Monsanto Chemical Company
1968-1969 Manager, Engineering Technology, St. Louis, MO
1969-1979 Director of Corporate Technology, St. Louis, MO

The University of Texas at Austin
1979-1985 The Ernest and Virginia Cockrell Chair in Engineering
1983-1996 Head, Separations Research Program
1985-1992 The John J. McKetta Centennial Energy Chair in Engineering

Honors

1965-1967	Elected Director and Member of Council, AIChE
1968	Personal Achievement Award, Chemical Engineering Magazine
1971	Elected to Fellow Grade of Membership, AIChE
1973	William H. Walker Award, AIChE
1974	Elected to National Academy of Engineering
1975	Chemical Engineering Practice Award, AIChE
1975	Andre Wilkins Award, Tulsa AIChE Section
1976	Founders Award, AIChE
1976	Distinguished Engineering Graduate, The University of Texas at Austin
1977	D.Sc., Washington University, St. Louis, MO
1979	Institute Lecture Award, AIChE
1979	CACHE Committee Educational Award
1981	Distinguished Advisor Award, The University of Texas at Austin
1983	Eminent Chemical Engineer Award, AIChE Diamond Jubilee
1984	Engineering Foundation Faculty Award, The University of Texas at Austin
1984	Best Applied Paper Award, South Texas AIChE Section
1984	Founders Award, Balcones Fault AIChE Section
1987	Joe J. King Professional Engineering Achievement Award, The University of Texas at Austin
1987	D.Hum., Clemson University
1991	Malcolm Pruitt Award, Council for Chemical Research

ABSTRACT

This interview with James R. Fair begins with a discussion of Fair's childhood in the Midwest, highlighting high-school experiences in Little Rock, Arkansas, and early interests in science. Fair attended The Citadel as a chemistry major for two years before transferring to Georgia Institute of Technology, where he studied chemical engineering. He discusses general and chemical engineering programs at Georgia Tech, early interest in unit operations, and effects of World War II on studies and career options. In 1942, he began work with Monsanto Chemical Company, where he advanced through several positions, focusing on work with TNT nitration process, ethylene and styrene, and set-up of a synthetic rubber plant. Fair discusses early involvement with the AIChE in South Texas, Monsanto's post-war entry into petrochemical production based on acetylene and ethylene, and work on an ethylene plant joint venture with Socony Vacuum Oil Company. In April 1947, Fair witnessed the explosion of the *Grandcamp* and Monsanto's Texas City polystyrene facility, which killed numerous employees and others and led Monsanto to rebuild and center its petrochemical ventures in Texas City. Fair contributed to redesigning and rebuilding the plant, heading process design of ethylene before taking academic leave to pursue coursework in reactions, separations, thermodynamics, and mathematics at the University of Michigan. He returned to Monsanto and was again involved in ethylene- and acetylene-based work. In 1952, he entered a Ph.D. program at The University of Texas, working with Howard Rase on catalysis and reaction engineering and upon completion accepting a basic research position at Shell Development in Southern California. In 1956, Fair returned to Monsanto to start an engineering research program, doing basic research in chemical engineering and serving as company consultant for ethylene and hydrocarbon pyrolysis. He traces Monsanto's ventures in petrochemicals through the fifties and early sixties to the formation of a corporate engineering department. From 1964 to 1979, Fair headed corporate Monsanto's technology function and increased involvement with academia, particularly Washington University. In 1979, he took early retirement and accepted an engineering chair at The University of Texas, where he was well received by faculty and students. Throughout the second half of the interview, Fair emphasizes changes in chemical engineering curricula and need for industry/academia collaborations in research and funding. He discusses research collaborations, publications, and efforts to develop and license computer programs for process simulation/computer-aided design. The interview closes with discussion of student research and careers, involvement in the AIChE, consulting activities, and family.

INTERVIEWER

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

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INTERVIEWER: James J. Bohning

LOCATION: The University of Texas at Austin

DATE: 19 February 1992

BOHNING: Dr. Fair, I know you were born in Charleston, Missouri, on October 14, 1920. Could you tell me something about your parents and family background?

FAIR: My father was a flour miller. He was running a mill in Charleston at the time of my birth. We didn't stay there very long. We moved to Kansas where he started the Fair Mill and Elevator Company in Tonganoxie which is about forty miles west of Kansas City near Lawrence. So, my early days were spent in this little town in Kansas.

From there it was to South Carolina and then to Little Rock, Arkansas. By this time my father was not milling flour. He was selling flour and feed, as we called it, a grain broker—in Little Rock. So, I went to junior high school and high school in Little Rock. In fact, I graduated from what later became the famous Central High School in Little Rock where there were some racial problems.

My mother was always a homemaker. My father did a lot of traveling.

BOHNING: Do you have any brothers or sisters?

FAIR: Yes, I have a sister who has been a homemaker. Her husband is retired from the military and they live in Fayetteville, North Carolina. I have a younger brother who for many years has been on the faculty of the University of Virginia. He is in the Darden Graduate School of Business. So, a brother in academia, and a sister, retired.

BOHNING: Before we get to Little Rock and your high school days, what was it like growing up in Kansas?

FAIR: Tonganoxie was a little town. It had a flour mill and that was about it for industry, but it is near Lawrence, and not too far from Kansas City. It's in a nice part of Kansas, too. It really

is. Eastern Kansas is very attractive. So, when I tell people at Kansas University where I lived, they all know about it. They're about the only ones. [laughter] It's about fifteen miles from Lawrence.

The thing I think of most about Tonganoxie, which then had a population of about one thousand, had to do with spending a lot of time at the railroad station. It was on a branch line, not too many trains. But then, interspersed were trips to Kansas City, which was a real railroad center. My fondest recollections are involved with my father—who was something of a railroad fan himself—taking me to various trackside locations to watch the trains go by. That interest has never left me.

BOHNING: They have a magnificent passenger station in Kansas City.

FAIR: Well, I'll have to tell you that for almost all of my life I have been a railroad buff.

BOHNING: I noticed that you published at least one or two books (1, 2). I wanted to talk about that, because I am a railroad buff too. [laughter] I thought I'd put that to the end of our talk.

FAIR: Yes, that may be doomed, but it was impressive, I'll tell you, to a boy eight or ten years of age. It was a tremendous place. We did a lot of train riding in the family because my father was from South Carolina, and we took trips back and forth from Kansas City to South Carolina. That involved a couple of nights on the sleeper.

That was a great experience, growing up in this little town in Kansas where you had the run of the town. At six or seven years of age I was quite free to go down to the station. I didn't have to have somebody with me. We had a mixed train each day, each way, and we had a McKeen motor car each day, each way between Lawrence and Levanworth—that was the extent of the branch. It's gone, but I still have a picture of the Kansas City Depot. That was a great experience there.

Then we moved to South Carolina to take care of an ailing grandfather. My dad stayed behind in Kansas. I spent a year there [in South Carolina] in the public schools before moving on to Little Rock, and later returned to South Carolina to go to college.

BOHNING: In Little Rock you were in high school. How did that compare with your experiences in Kansas?

FAIR: Little Rock was a city. Our home was right on a trolley line, double track, so I got very interested in streetcars. Little Rock was also an important division point on the Missouri-Pacific, and I also spent a lot of time at the Rock Island depot. I'm in the middle of a manuscript now on that part of the Rock Island system. But Little Rock was the big city. In high school we had a very large, attractive facility. We had about twenty-eight hundred students—which was big for me—but with a fine college preparatory program. It was by far the outstanding high school in the state. In the state of Arkansas at that time there were many poor high schools, poor in several ways, but I felt I got an excellent preparation for college.

BOHNING: When was the decision that you would be going to college made?

FAIR: It was always assumed that the children would go to college, and we all did.

BOHNING: Was your father a college graduate?

FAIR: No, he wasn't. Nor was my mother. My mother went to college for three years and then went into school teaching. My father just went for one year. He attended The Citadel in South Carolina and then went to work. But it was always a given that we would go to college, never any consideration otherwise.

BOHNING: You started high school during the Depression.

FAIR: Oh yes. Of course the Depression is still a vivid memory of mine, and I'm sure it has influenced me a lot. My spending habits, for example, through the years. Everyone knows that an ice cream cone should only cost five cents, and we even got to where you could get three dips in a cone for five cents at the Triple Deck Ice Cream Parlor. [laughter] Now you can pay a dollar for a single dip—it's probably better ice cream. [laughter]

BOHNING: When did your interest in science start developing? Was it when you were in high school?

FAIR: It was in high school, but I wasn't decided about just what major to undertake in college until a friend of mine whose name is Matt Jones got me interested in chemical engineering. I really have to give Matt Jones credit for this. At the time of my decision, I was bound and

determined to go to Georgia Tech. Tech required a year of physics in addition to the other entrance requirements, so I got a late start in taking physics. In fact, I stayed on a semester after February graduation to complete the physics course, which was a very good course with good laboratory equipment. That convinced me even further that my interest in math and chemistry and physics would be the right combination.

So, that was the idea, that I'd go to Georgia Tech; but my father changed my mind. He said I needed to go to a military school.

BOHNING: Did he give you a reason for that?

FAIR: His reasons were generally that he was paying for it and he had gone to a military school, and I needed to gain some weight and stand more erectly and learn military discipline. So, I went off to The Citadel and majored, if you will, in chemistry.

BOHNING: In Central High School in Little Rock, were there any teachers who had any influence on you?

FAIR: Yes, there were two very definite ones. I had a Miss Flora Armitage for solid geometry and trigonometry, and in part for what was then called advanced algebra. She was a great teacher. The other was a physics teacher. His name was E. Barnes. He really put the spark into the idea of chemical engineering—small class, excellent laboratory.

One of the features they had—and this was in the late 1930s—was to use a series of movies put out by the University of Chicago. I remember they were called Erpi Films. They were science films that showed a number of phenomena in motion—simulated, of course—that I thought gave great support to the textbook, the problems, and so forth. I don't know whether those films are still shown to physics classes, but they were shown one day a week, not always during the regular class period. If you wanted to see how molecules mixed, they had this representation. Those were good films. I don't know whether they've ever come up in your discussions (3).

BOHNING: No.

FAIR: They were University of Chicago films, and there were a lot of them. So we were using that type of audio-visual back in the 1930s.

BOHNING: Which is very unusual for that time, I would expect.

FAIR: Yes, and I think it was unusual for that region because the films were fairly expensive, and you had to have the projection equipment. They were sound movies. That was a great help, I think, to all the physics students.

BOHNING: Were there many other science-oriented students in your graduating class in high school?

FAIR: Only a few. We had a reunion not long ago, and at least of those attending, I'd say not more that three had had careers of science or engineering. This is out of a hundred or more people who came to that particular affair. So it was not a big thing at the time, although chemical engineering in the 1930s was thought to be a glamorous field with great opportunities. It was, particularly, as it turned out, with the war coming along so suddenly. Chemical engineering graduates in the late 1930s were certainly getting good jobs. Many of our very illustrious chemical engineers did their graduate work in the early 1930s, in part at least because there weren't jobs out there. They went to graduate school and then in many cases into teaching. I've talked with some of these people and that's what happened. There were opportunities, I gather, for financial support in the colleges at that time, I think mostly as teaching assistants.

BOHNING: How did you feel about going to The Citadel as opposed to your dream of Georgia Tech?

FAIR: Well, I certainly didn't like the idea, but after I got there I sort of liked the military discipline. It was certainly different from the usual college campus. The Citadel and VMI are the two military colleges other than the service academies that are hold-outs on the enrollment of women. At that time we had fifteen hundred cadets. In a way it was not good for all of us, because we didn't have to do a lot of thinking for ourselves.

For example, you were awakened by the reveille bugle in your barracks. You might see outside that it was overcast or perhaps even raining lightly. Your next move would be to go to the door and look at the flag. If the flag was green you wore a raincoat. If it was gray, you didn't. You didn't have to make that decision. [laughter] Of course, at breakfast you had formation, and then you marched to the mess hall.

But I got to like it. In fact, I wanted to go back the second year because I had been made a cadet officer. There was a lot of hazing at The Citadel at that time. There's been a book

written about The Citadel and the hazing, a popular novel (4). Freshman year was so full of freshman requirements and directives that I thought it would be nice to be on the other side of the fence, so I went back the second year. That made transferring to an engineering college more difficult.

BOHNING: What kind of curriculum did you have in those two years at The Citadel?

FAIR: Well, I had general chemistry, mechanical drawing, history, English and mathematics, which led in the first year to some calculus. In the second year there was one semester of qualitative analysis and one semester of quantitative analysis, a full year of physics, and a full year of calculus. Then you had English and military science and so forth.

Let me go back and correct this. That's not the way it was at The Citadel. There was a full sophomore year of qualitative analysis. You didn't get quantitative until the junior year, while at Georgia Tech you had quantitative analysis the second year. At Georgia Tech you went much farther into calculus, you got into differential equations. The net result of this was that I had to make up some time at Georgia Tech—the courses I hadn't had. In fact I picked up the quantitative analysis at the University of Arkansas in the summer between the sophomore and junior year. I got that one out of the way by going up to Fayetteville, Arkansas, but there were problems of getting transfer credits from a liberal arts school to an engineering school; that's really what it amounted to. I don't really recommend it to anyone. Now there are three-two programs—three years at a liberal arts college followed by two years at an engineering school—and two-three programs that would work, but not two-two.

BOHNING: The Citadel didn't have any chemical engineering at all?

FAIR: It had, at the time, an accredited course in civil engineering, accredited by the Engineers Council for Professional Development [ECPD]. It later added an accredited program in electrical engineering, but not in chemical engineering.

BOHNING: What were their laboratory facilities like?

FAIR: The facilities at The Citadel were fairly good. The chemistry laboratory was good with one exception. The qualitative analysis laboratory sophomore year heavily utilized hydrogen sulfide. We didn't regard hydrogen sulfide as much more than simply a gas that didn't smell very good. One whole end of the building smelled like hydrogen sulfide. Now, I think we would have to wear gas masks if we were in a place like that, so that was a pretty sloppy lab, as

a matter of fact; but the other labs were okay. They were standard. The physics lab was good, but not quite as good as the one we had in high school. I would say their facilities for science were mediocre at best.

BOHNING: How did you convince your father about transferring to Georgia Tech?

FAIR: I never really did. I found aid in two uncles and an aunt, his two brothers and a sister. They agreed to loan me the money to go to Georgia Tech for two years. So, each month I got a check from them. I later was able to pay that back pretty rapidly when I went to work. I give them the credit for making it possible.

Now, my father would have agreed to a transfer to the University of Arkansas, but at that time Georgia Tech was one of four schools in the South that were accredited by the ECPD. There was a fifth school that could be considered Southern: the University of Louisville.

BOHNING: What were the other three schools besides Georgia Tech?

FAIR: The University of Tennessee, LSU, and VPI. Not Texas. I had wanted to go to MIT and really couldn't afford that as a freshman. I had the grades to get in but I didn't have the money. The MIT people wrote and said, "If you can't really afford to come here, pick one of these schools in the South and they will be much less expensive." So, that was how Georgia Tech got in there. I'll have to admit that in my earlier travels to and from South Carolina I'd always been impressed by Atlanta. I'd thought going to school in Atlanta would be a pretty good thing, and it was. It was a great, great city.

I think location does have something to do with it. It was a much more lively place than Charleston, South Carolina, I'll tell you. [laughter] That's the way that went.

BOHNING: You spent 1940 to 1942 at Georgia Tech. What was their curriculum like?

FAIR: The curriculum of course had many more hours in it than we have today. A typical course load was in the range of eighteen to twenty-one credit hours, more often twenty-one. I had to go more than that simply because I had so much to make up, so I was up to twenty-four credit hours a semester. This was on the basis that three lab hours counted for one credit hour. We had more labs than we have now in our curriculum, so that it was not unusual to have four afternoon labs a week, and of course we had classes on Saturday. So, you had thirty clock hours

of time spent on campus either in laboratory or in class. In my case, I had to take some additional work.

For example, in the sophomore year at The Citadel, as was typical in those days, a year of German was required, and I took it. I made excellent grades, but for some reason Georgia Tech wouldn't accept that. Now, Georgia Tech also required six credit hours of Elementary German. The only way I could get credit for The Citadel course was to pass a reading examination in German, and that bothered me. I put it off until the senior year but reviewed and did fine. That's just kind of the way it went.

While at The Citadel in the sophomore year, you had six credit hours of math. At Georgia Tech they had eight credit hours of math, so I had to take three hours more of sophomore math while I was a junior. I had restrictions at this time. They said they would give me credit for the Qualitative Analysis course that I took at The Citadel if I made a good grade in Physical Chemistry. I plunged into Physical Chemistry at Georgia Tech.

I was also introduced to college fraternities at Georgia Tech, and a lot of freedom. I didn't have to wear a uniform. The big shift there was going to personal time management, the same problem industry has with salesmen who are located away from the main office. I've been told that this is one of the biggest problems with a person who is operating out of an office where the boss is, perhaps, miles away. There is nobody to tell this individual when he should go to work, or what he should do or whom he should see. That was one difference. At The Citadel your schedule was made out for you: between four to six you <u>will</u> get out on the parade ground and engage in some method of sports, or you <u>will</u> do extra drill during that period. Everything was mapped out. At Georgia Tech you were on your own. That made it an interesting endeavor.

[END OF TAPE, SIDE 1]

BOHNING: What was the chemical engineering department like?

FAIR: Well, the chemical engineering department, in order to get accredited, had to construct an addition to the chemistry building for its laboratories, classrooms, and offices. I think the accreditation came in 1938, so these were new facilities. They had a very good unit operations laboratory. The faculty was small. I believe there were only four faculty members, but the classes were small. Our senior class had about sixty people. We were always split into two sections. There were never any classes in chemical engineering with more than about thirty people, which is something we'd like to be able to do today, but really can't.

Also, the classes at The Citadel—such as freshman math, freshman chemistry, sophomore physics—were small classes. I had to take an extra semester of physics at Georgia Tech and that was my first time in one of the big, amphitheater-type classrooms. We may have had a hundred people. Now we can put three hundred to five hundred in some of our classrooms here, and that's a big problem for the student as well as the teacher.

The facilities at Tech were good. The relations with industry were good. The engineers, of course, seemed to be sought after. They had no Ph.D. program, but they did have a master's program, and there were M.S. candidates serving as teaching assistants. The quality of education at Georgia Tech, I found, was very good.

BOHNING: Was there any one aspect of chemical engineering that interested you during that time?

FAIR: I would say the unit operations courses and the unit operations lab. One thing about the Tech curriculum at that time was that between the junior and senior years you were required to come back during the summer for six weeks to design, build, and operate a pilot plant facility. We built an aniline plant where we nitrated benzene and from the nitrobenzene made aniline. We ran around the clock on shift, and that was fun, so I enjoyed the unit operations aspect.

Unfortunately, the curriculum at that time did not include an undergraduate thermo course. You obtained thermodynamics in physical chemistry and that was it. That was a shortcoming. Of course, at that time there were no courses, no textbooks for reaction kinetics. Again, it was something that you had to pick up on your own as the discipline developed.

The Tech experience was a good one. Of course, during my senior year there occurred the Pearl Harbor event, and that changed the complexion of a great deal of the academic program. In fact, it was speeded up. Graduation date was moved up about three weeks so that the students could get on with the war effort in some way. Ours would have been the last large class in chemical engineering until after the war.

That made a big difference, but the companies were flocking in to interview students about war-effort directed work. That was something that you needed to take time for, to interview with these companies. I ran into one of my first problems in professional ethics at that time. I had really wanted to get an offer from Monsanto, and I had talked to them and gone over to their plant at Anniston, Alabama.

BOHNING: Why Monsanto?

FAIR: I guess I got acquainted with Monsanto through the *Monsanto Magazine* in the library at Georgia Tech. Going through those issues, it just looked like a good chemical company with good policies. Then an article came out in *Fortune Magazine* at that time about Monsanto, and I read that. But there was also an article that came out on Koppers Company, and Koppers had made me an offer.

I didn't hear from Monsanto, so I accepted the Koppers offer to go to Pittsburgh as a cadet engineer. Then in came the offer from Monsanto, so I went to see the chairman of the department whose name was Jesse [W.] Mason. "What should I do?" He said, "Well, clearly you want to go to work for Monsanto. Why don't you write to Koppers and ask if you could be relieved of the commitment?" So I did that. They responded promptly that, while it was a very unusual request, they would grant it. Then I accepted the job with Monsanto. Of course, I never regretted accepting a job with Monsanto.

The word from Monsanto came that I would be assigned to an ordnance plant in Texas which was under construction. Until it was completed I would work at the general headquarters in St. Louis. I reported there on June 1, 1942, which means that in a few months I will celebrate fifty years of continuous professional effort in chemical engineering. So, that turned out to be a good thing—to get acquainted with a company at its headquarters location.

BOHNING: I don't mean to interrupt, but you had made that comment about your first exposure to professional ethics.

FAIR: Well, my concern was, should I just accept the Monsanto job and write to Koppers and say, "Sorry, I changed my mind." I didn't feel that that was quite the ethical thing to do. I preferred to do what the chairman had suggested. I don't know that you would really call it a matter of ethics, but we do run into that sometimes on interviews. Students—I don't think they do it these days, but they have in the past—might visit two companies in California and bill each company for the full round-trip cost, that sort of thing.

So, my decision put me in St. Louis, another nice, big city.

BOHNING: A good railroad town.

FAIR: A good railroad town. One of the best after Chicago, I suppose. Monsanto was even then gearing up, so they were on 5-1/2 day weeks. Then I was sent up to the Chicago area for training at Kankakee Ordnance Works. I'd like to mention here that this was a plant that was operated by the DuPont company, designed by DuPont for the government. I think that what DuPont did in 1941 and 1942 to allow us to move quickly into explosives is really something to

write about. Certainly their plants worked. The designs were set. They were freely given to the government, and tremendous amounts of lead azide, TNT, and other materials were produced quickly.

The plant that I went to in October of that year was known as a five-year plant. This was Longhorn Ordnance Works near Marshall, Texas. I'll talk about that in a minute. It was constructed quickly, in less than a year. Five-year plant means that the buildings were all frame, the pipe supports were wooden, but all of the processing equipment and the controls were first class.

The Kankakee Ordnance Works was a twenty-five-year plant, so it was brick and stone and steel. The training at Kankakee was interesting also because there were trainees there from a number of different ordnance plants. So, you could get the perspective that explosives were being produced in many locations in the country.

BOHNING: In what specific area were you being trained?

FAIR: Well, I was trained as a control chemist initially and was given the title of Shift Supervisor Chemist. I had only myself to supervise, and I was on shift. [laughter] It was a control laboratory. It was out in the TNT area, and I analyzed the samples of nitrated toluenes as well as the toluene.

Incidentally, it was at that time, in 1942, that we first started using toluene from petroleum. It was shipped out of Baytown, Texas. Before that it was all coke-oven toluene. We could see the difference in the analysis. The petroleum, toluene, which came from a reforming type of operation, was really a good quality, close boiling range material. But that did change the complexion. In fact, it was while I was in training at Kankakee that this news came out that the first tank car of petroleum toluene had come in. I later talked with some of the people at Humble Oil who were involved with producing that first tank car. At least, that was the first tank car for the Kankakee Ordnance Works. Then, of course, the shift was made mostly because, I think, there wasn't nearly enough coke oven toluene capacity to supply the war effort. Not only for toluene, but for benzene. So, that was an interesting shift that we noticed.

BOHNING: You went up to Kankakee in October of 1942. When did you get back to Texas?

FAIR: No, I went up to Kankakee in mid-July of 1942. I was really assigned to St. Louis, but temporarily. When I got back from there the 1st of September, I moved on down to Texas in the middle of October. In between I worked in the research lab for a chemist named Ferd [Ferdinand] Zienty, who was a good teacher. I learned a lot in the six weeks with him.

East Texas was quite different from St. Louis. The plant was out at the town of Karnack.

BOHNING: Still a very small dot on the map.

FAIR: Yes, but it has become famous because that's where Lady Bird Johnson was born and grew up. I remember that we went on the train to Marshall, and they put us in a truck to take us out to the plant. We drove by this large brick home of T. J. Taylor. He was the boss-man of Karnack and father of Lady Bird Taylor.

The reservation there was very large. It was on Caddo Lake. That was the source of water. It really was remote, which was needed for a TNT plant. I would say by today's standard that it was a good plant location, on a railroad. But I would guess, also, that Lyndon [B.] Johnson—by this time married to Lady Bird Johnson, a Taylor—had some influence over the location of that plant. In fact, T. J. Taylor sold a lot of that property to the government.

It was a good site and a good plant. We then went on a 51 hour-a-week work schedule. It used to be 48 hours, but they added travel time to and from the gate, because you had to use buses. The nitrating area was some distance from the front gate, just to be safe. With a 51 hour week, it was a busy time.

BOHNING: What were housing accommodations like?

FAIR: We moved as a group; there were about ten of us, all single. We just got rooms in homes around Marshall. There was housing for married workers. In fact, some relatively new housing was put up—some at the plant, right on the premises, and other housing in Marshall, which was where most people lived.

That was a good opportunity. We just went into start-up after I got there, so I was able to learn about the start-up of a plant of that type. The operating headquarters for the nitrating area was part of the building where we had the control lab. One night we had an explosion and fire at the trinitration plant.

I moved out of the lab in another three or four months and into the engineering department because we were beginning to design a so-called direct nitration process that had some of the elements of a continuous process. The TNT process was a three-step batch process. You made mononitrotoluene, and then you moved that to another building, separated for the dinitration step. Finally, for the trinitration step you used oleum along with concentrated nitric acid for the nitration step. So we had oleum to deal with, and analyses of that. We concentrated

sulfuric acid on site, but we shipped in the oleum which, as you may have experienced, is an interesting material to work with.

BOHNING: That was what I was going to ask. What quantities are we talking about in a given batch?

FAIR: These batch reactors were about six thousand to eight thousand gallons. They were big. You took the recipe, and you made a batch. Then you cooked it until the toluene was converted to nitrotoluene. They were agitated vessels. They were elevated in the structure.

They had safety chutes, so if any operator was up on the second deck—the problem was with runaway reaction—he would move to the side of the platform where there was a big chain pull. As he hit the chute he'd pull this chain, and that dumped the contents into a big water vessel to kill the reaction. That's the way they controlled it.

Ultimately, as one assignment I had there, I put together a number of flow diagrams of the nitration process as well as the ammonia oxidation for nitric acid, and we concentrated dilute sulfuric. We concentrated nitric too. But the net input of sulfuric was in the form of oleum, which came over from Shreveport, not too far away. We shipped in ammonia, which also emphasized the need for ammonia in the early days of the war effort. That was the source of the nitrogen for a lot of TNT.

The operation would be considered crude by today's standards. Of course, after the war the TNT process went to continuous operation. But there was a lot of chemical engineering to learn in the process. So, in my role in the engineering department, I was involved in piping changes and putting in new foundations, a lot of civil engineering work, such as surveying. It was a good experience.

BOHNING: Did you have that type of civil engineering background at Georgia Tech?

FAIR: I did not take a surveying course there. If I had started there as a freshman, I would have. At that time they introduced most engineers to surveying or to shop. But I worked with a more experienced engineer. He ran the transit most of the time, and I went out and made the measurements and held the pole. [laughter] I had gotten some surveying experience in high school, so I had some background in it to begin with.

I got into engineering, and then I was shifted down to the nitric acid concentration unit, where I had my first introduction to bubble-cap tray columns, because the nitric acid was concentrated using azeotropic distillation. Sulfuric acid was added as an azeotropic agent. You

could take dilute nitric—water and nitric acid—and add sulfuric to it to pull the water out of the nitric, and the nitric went overhead as a very concentrated material.

I guess I get kind of scared now about working in such close contact with these high-concentration acids. We didn't have to wear hard hats. The people in the acid area at Longhorn had to wear felt hats. They had to wear some kind of head covering, just for acid spills, but not for falling wrenches.

[END OF TAPE, SIDE 2]

FAIR: We also had change houses. We were required in the TNT area to change into laundered uniforms. We had to guard against striking sparks and that sort of thing, so the Longhorn experience was a good one.

BOHNING: What was the atmosphere in the plant like? You must have been able to smell things. What were you breathing?

FAIR: Well, it was fairly clean. Over in the acid area, of course, there were a lot of nitrogen oxide fumes. You certainly smelled that a lot over there. You also had sulfuric acid mist from the concentrators, but the plant was well designed. In particular, it took care of waste waters. In the nitration process you generate so-called red and yellow waters. These were piped to a very modern, efficient, multi-stage evaporation system, and the final concentrate was sent to a rotary incinerator, so just a modest amount of ash came out of the end of it.

I think that, so far as wastes were concerned, they were pretty well under control in those days. We didn't have ditches with acid flowing toward the lake. The plant was built quickly to DuPont's specifications. In fact, DuPont was involved in the construction of the plant. There were DuPont people there to supervise construction. I don't know how that company supplied as many engineers as it did for all of the things it was doing.

The plant by mid-1943 was operating pretty routinely. At that time the plant at Texas City was being completed and needed help on start-up. So, in August of that year several of the engineers and I were moved to Texas City—I might say summarily moved. We were called in on Friday afternoon by the general manager, who said, "You are reporting to Texas City Monday morning. A car will be here Sunday morning to pick you up and drive you down." We could move fast if we needed to.

BOHNING: Well, by that time, 1943, we were in the midst of the war.

FAIR: Yes, the synthetic rubber program was not moving nearly as rapidly as the explosive program. DuPont was not involved to move it. But, by that time, it certainly was agreed that the synthetic rubber to produce was a GRS—at least in large quantities. This required styrene and butadiene. Monsanto had contracted with the government to operate a plant to produce styrene monomer at Texas City.

The plant was built under the direction of what was then called the Defense Plant Corporation, DPC. DPC, for other than ordnance plants, was involved with the construction aspect of these plants. The operations were under the direction of the Rubber Reserve Company. So, we had DPC people on the premises during the construction of the plant, but we worked with Rubber Reserve officials on production matters.

BOHNING: Had Monsanto been a presence in Texas City before that?

FAIR: No. In Texas City an abandoned sugar refinery was purchased by the government, and we utilized some of the sugar refinery facility, primarily the old steam plant. The other equipment was removed. A building, about six stories, was there. It was kind of interesting occasionally to go over and tool around that building. That was a building that was right next to the dock at Texas City. It had its own platforms and locations which had been used for bringing in the sugar cane, and to some extent for shipping out the refined sugar. So, we built the plant right next to this old sugar refinery. In fact, the office building had been the office building of the sugar company.

While we did not have DuPont, we did have Dow Chemical involved in styrene. Dow had done a lot of the work on the styrene reaction itself—ethylbenzene dehydrogenation. We were able to use that. We had a thermal unit that was based on Monsanto experience, but when I got there we were also installing the Dow catalytic system. The thermal unit was very interesting. You took ethylbenzene, vaporized it, and bubbled it through molten lead. The dehydrogenation occurred there, but only to the extent of about forty percent conversion—far from equilibrium conversion, which then placed a burden on the separation system. The pilot plant work for the distillation had been done in part in Dayton, Ohio, by Monsanto, and the alkylation research had been done by Monsanto at Dayton, Ohio; this was a Friedel-Crafts type alkylation of benzene with ethylene.

Our problem at that plant was the ethylene unit. So, my first assignment was in ethylene, and I stayed in ethylene for quite a while. The problem was with the cracking furnaces. This was the first plant designed for ethylene by the Lummus Company, and it turned out that the cracking furnaces simply would not work. We could not get them to work and stay on stream very long before they coked up. Fortunately, in Texas City, there was a chemical

plant of Union Carbide, then called Carbide and Carbon Chemicals Company. They put in a pipeline between Carbide and Monsanto, and we started the plant up with Carbide ethylene. I don't know whether this deprived Carbide of ethylene that could have been used in other ways, but apparently we were able to get it when we wanted it.

We had completed the Dow dehydrogenation units and that freed up those lead pots. I was in charge of modifying one lead pot unit to serve as a pilot plant for cracking propane and propane/propylene mixtures to ethylene.

BOHNING: You had a paper on that.

FAIR: I sure did, later. I did that pilot plant work and later hauled it out for publication. At that time the pots were still being used, but I got permission from Monsanto to put that paper together. The fellow who had been my boss at the time was Jack [W.] Mayers, who was one of the co-authors of the paper and one of the greatest chemical engineers I've ever encountered. A Dr. Will [William H.] Lane and I published the paper (5).

Anyway, going back to the 1943-1944 era. We started running propane and propane propylene mixtures through those baths. I'll never forget when we first fired those pots up. We took the tops off. These were big pots, about four feet in diameter. We had them on; we had molten lead down in the pots. We would stand over it, and we would work around it—didn't seem to know that lead had any vapor pressure. [laughter] But we did that and we got some good results and we very quickly got approval from Rubber Reserve to work up a proposal for converting all 24 pots to ethylene.

In the meantime, I was sent to Boston for about three months to work with Stone and Webster on the design of a tubular-type cracking furnace so that we would have an alternate to the lead pots. I worked with them on the design of a cracking furnace complex to serve Texas City. The Lummus furnaces were out; we just couldn't use them.

We did that. Then I went to St. Louis for a while, worked up the estimates of lead pots versus tubular furnaces, and submitted our report to Rubber Reserve. We recommended the lead pots because they were there, and we could put them in operation very quickly. That's what we did: we converted the lead pots. We added some lead pots; but there were initially 24 sets, and each one had two pots. We expanded that to 36.

I was involved with the engineering work. We did that in-house. We put those things on stream and they worked. I'll tell you, you would stand around these monsters and the gas would sort of bloop through them and they would move around on their foundations a little bit and occasionally one would drop its lead out the bottom. But the housing was designed to contain the lead.

To add charge to the pilot plant pots, we would get the lead molten and then just drop lead pigs in there. It would splash, but we would simply stand back. [laughter] So, we had a good unit there. It was crude and inefficient—those furnaces ran about fifty percent thermally efficient, and a good cracking furnace would run around seventy to seventy-five percent—but it really served our needs at the time. We did expand the production of ethylene and styrene. We just made a lot of product with those famous lead pots.

BOHNING: That's a lot of lead you're talking about.

FAIR: Oh yes, and we had lead collectors. Of course, some would vaporize and flow over into the quench system. The hot gases from the pots went over to a water quench, so we collected lead at the bottom of the quench tower. We had a separate tower for each pot set, so we could take a pot off stream if it dropped its lead. Or if we had other problems we could just take off a unit, clean out the quencher, put in more lead, repair the pot, and put it back on stream. They were direct-fired. You had a combustion chamber surrounding the pots, gas fired.

BOHNING: What were the pots made of themselves?

FAIR: They were made, as I recall, of a 25-12 chromium nickel steel. They were high alloy, very thick walled, lots of heat transfer resistance in that wall. [laughter] I had a sketch of the arrangement in the paper that was published later, so that's on the record.

BOHNING: Well, Dow was building the Freeport, Texas styrene production facility at the same time, because they were on stream in 1943, I think.

FAIR: They were in the styrene business also. The Rubber Reserve published operating data production statistics and there was, you might say, some competition to see who could stay on stream and who could produce the maximum amount of material. The Dow unit was always a tough competitor because Dow really knew how to make styrene from the start. They had made it up in Midland, Michigan. They had a different kind of refining system from ours. Their alkylation system was different, but they had the same type of catalytic dehydrogenation reactors.

The Dow process for styrene was published in an issue of the *AIChE Transactions*, a very useful paper that came out, I think in either 1945 or 1946 (6). That paper pretty well told the story of what we were doing at the dehydrogenation step. Upstream you used a superheated

steam and mixed that with vaporized ethylbenzene and passed it over a catalyst. It was a much cleaner, higher conversion process than our old lead pots, by far. So Dow, I would say, should get a lot of credit for getting the country's styrene production going. Koppers was also making styrene.

BOHNING: Did you have any interaction with Dow or Koppers at the time?

FAIR: Not really. I did as part of the South Texas AIChE. We took a trip to the Dow unit. Now, Dow had more than one unit in the Freeport area, but we were able to visit their government unit. The same for their magnesium plant, the so-called Velasco versus Freeport. So, I will say at this point that in those days, the AIChE section was very active. Their meetings were in the Houston area mostly, but ranged down to Freeport and over to the Beaumont-Port Arthur area.

The difficulty, of course, was getting to these meetings with the limited gasoline and rubber tire problems. We were using a plant car to go to a meeting in Houston, and we heard this big explosion. We stopped the car to see what happened. The spare tire was in such bad shape that it had a blow out while it was just in the trunk. [laughter]

But that was on the way to a meeting. We could use a company car. We could get the gasoline for that. It was a very active AIChE section, and a good one. They had sort of a fixed procedure. You would visit a plant in the afternoon. Then you would have dinner nearby. Finally, you would have a talk about the plant or about the business in the evenings. It was all integrated that way.

We went down to Freeport Sulfur. We would tour the sulfur plant, and then we'd hear about sulfur that evening. It was very educational for young engineers. We went to the Baytown Refinery. We went to a sugar refinery. So, it was a good thing.

I got involved with the AIChE as soon as I got to Texas City. I guess I became a member officially in 1944. That was really what got me started on spending a lot of time and effort on AIChE affairs.

BOHNING: Yes, I have something I do want to cover with you toward the end of the talk about your AIChE involvement. Certainly at that time you had so many young chemical engineers arriving in that area.

FAIR: Our plant was staffed by young engineers; it really was. I suppose someone like Jack Mayers was about four years older than I was, and he was one of the more senior people. So it

was a young effort. Our plant manager had been around a while; his name was H. K. Eckert. He was, I remember now, about twenty years older than us, so he was one of the senior people. Our director of manufacturing was a Ph.D. chemical engineer from Yale, and that was impressive to me. In the 1930s there weren't many Ph.D. chemical engineers. Charlie [Charles S.] Comstock was his name. He had done his Ph.D. with Barney [Barnett F.] Dodge at Yale, and had in his office a shelf of the *AIChE Transactions*, which I now have on my shelf over here.

We didn't have a library, but one of the first things I did was to make an inventory of the personal books of the engineers there, a sort of serial list of who had what, because that's what we had to use. If you ever needed to look something up in a journal, you'd probably have to go to Houston to the Rice Institute library, in our field at least. So, books were important and people tended to buy personal books to have for reference purposes. There was a good bookstore in Houston for that. I started building a pretty good-sized personal library then in chemical engineering.

It was a young group, an enthusiastic group. Not much time for play because we worked full Saturdays. I started Texas City in shift work and discovered there were electric typewriters in the office that I could use when I was not down in the unit. Ultimately, when the plant was running well there was formed what was called the Owls Athletic Club, which meant that some of the supervisors would go into the office and pull tables together and play ping pong at night. [laughter]

But that plant did well. It had good production through the war. While I was there, as I said earlier, I spent a lot of time with ethylene and some on styrene, and I was in charge of a revamp of the ethylene plant to increase its capacity. My knowledge of that ethylene system helped me a great deal in 1947 when we had to rebuild it. I was in charge of redesign and rebuild, but that of course is ahead of the story.

The end of the war with Japan came finally in August of 1945. It didn't take Monsanto long to realize that if the company wanted to keep the Texas City plant it would have to do two things. It would have to need the production of styrene for its own commercial purposes, and it would have to buy the plant from the government. Well, the first one of these was pretty easy to take care of because by this time the company was very much into polystyrene—it had been before the war—plastics at Springfield, Massachusetts; so there was a need for styrene.

Late in 1945, I was transferred to St. Louis as part of a five person team. Our assignment was to come up with a dollar figure that Monsanto should offer for that plant. This was an interesting assignment. We had to take a very close look at what we had, what it would cost to replace, and how it could be used for the company's need for styrene. That got us involved with the economics of the monomer and the polymer—whether it would be more economical to put in a polystyrene plant at Texas City or to ship the product up to Massachusetts.

[END OF TAPE, SIDE 3]

FAIR: We were set up apart from the headquarters site of Monsanto in a downtown office building. For the next six months we toiled on styrene, five people. Dr. Comstock was in charge of the group. In about mid-1946, because of my ethylene background, I was moved into another group which had been formed for the purpose of charting Monsanto's entry into petrochemicals.

We were in another downtown building. We were apart from the plant and the general office there. What we did during the next eight months or so was to put together a master plan for petrochemicals for Monsanto. We went in two directions. One of these plans was based on acetylene. The other plan was based on ethylene. For the ethylene plan this would be a joint venture with Socony Vacuum Oil Company—now Mobil Oil Corporation. The acetylene we would do on our own. The acetylene complex would be centered in the hills of Kentucky where we had the coal and limestone that were needed. In 1973, I reminded Monsanto that there is a way to make many petrochemicals without using petroleum.

So, that was the acetylene complex. We would make vinyl chloride and vinyl acetate there. We would make ethanol by starting with acetylene. A great deal of work was done on that project. We took options on property. We laid out the plant at a place called Kutawa, Kentucky. That was the later site, near there, of the Calvert City complex based on acetylene that several companies were involved in. Air Products has a unit there now.

I worked on the venture with Socony Vacuum. We designed an interesting ethylene unit with a pyrolysis system based on Socony's petroleum cracking experience using a moving bed. They called this a Thermofor Pyrolytic Cracking System—a moving bed unit that Socony had developed to replace the fixed bed Houdry units for the catalytic cracking operations. We did a pilot plant study of that at Paulsboro, New Jersey. Our design then included the moving bed cracking plus a distillation recovery system. That complex also had ethanol and vinyl chloride. It did not have vinyl acetate, as I remember.

It would be located in Port Neches, Texas, near the Socony refinery at Beaumont. We would actually use refinery gases. We would extract the ethylene from the gases, and then we would crack the other gases. We had that option, and we were moving right down the line with this when it came time—well, let me get back in sequence. That was in 1946 and early 1947.

I also worked back and forth with the styrene plant analysis group. We put our bid package together for the government. We bid about ten million dollars for the Texas City plant. It had been built for seventeen million dollars. It turned out that our bid was slightly higher—not a lot higher—than the next bid, which was put in by a colorful personality in Texas named

Glen McCarthy. Glen was a wildcatter, but he had gotten into chemicals. He built the Shamrock Hotel in Houston. But our bid was higher and we got the plant. That was in early 1947.

We pretty well completed the ethylene venture; that was still under consideration, so they transferred me back to Texas City along with the four other members of this group. We had done our job. Now we were to go back and do certain modifications on the Texas City plant. One of these was a polystyrene plant which others had worked on, and this plant was beginning operations when I got there April 1. We were actually producing some polystyrene. The overall plant was now running as a Monsanto plant.

On April 16, we had the explosion of the *Grandcamp*, which destroyed the polystyrene facility. It was completely destroyed, and of course we had a heavily damaged remaining plant. Well, that did one thing. Monsanto decided to rebuild Texas City and to center its petrochemical venture at Texas City. I think the Socony deal did not come off because Socony didn't want to do it. For us, the economics favored ethylene over acetylene at that time, so the Kentucky project was abandoned. So the Port Neches project was abandoned and attention was given to rebuilding Texas City. These things were all happening at about the same time.

Charlie Comstock, our leader, was killed in the explosion, and another member of the group was killed.

BOHNING: Where were you at the time of the explosion?

FAIR: Well, on the morning of April 16, I was still living in a hotel in Galveston. I had just been transferred down there. My colleague, one of the group members, and I were driving over to the plant when we saw the orange smoke. We hustled on over there to see whether it was our plant on fire. Well, it wasn't. It was the ship, the *Grandcamp*, on fire right next to the plant, so with many others we went out to the site of the ship to see what was happening. We could see the people up there working on trying to put the fire out. Our plant volunteer fire group was hustling around hosing down the side of our plant.

We were there by eight o'clock. I would say that around nine o'clock, maybe a little bit later, I decided, "Well, I've got work to do and I don't know how long this will last." I remember one of our draftsman came out and said, "Say, do you know what's burning in there?" And I said, "Well, we're told it's ammonium nitrate." He said, "Boy, that stuff has its own oxygen. I'm getting out of here." He hustled back to his office and was killed sitting at his desk!

Anyway, I decided I had work to do, so I told my colleague—Charlie Krause was his name—"I'm going to get to work. I've just got too many things to do." He said, "Aw, come

on, this thing might blow up. We might get to see it." I went back, and I had gotten around the side of a building and really into the entranceway to the building when that ship let go. Of course, Charlie was killed.

My only problem was that I was buried under some scaffolding—they were adding a third story to this building—but I got out from under there and got out of the plant. It was really roaring then. Everything was on fire. Then I was picked up and taken to a hospital in Galveston. My head had been cut, but I was out of the hospital that evening and back over to the hotel. Several of us gathered at the hotel to try to get information on what had happened. Of course there were the newspapers and radio and all.

I hadn't been using my car. I had driven to the plant in Charlie's car, so I had my car. The next morning I took off for Texas City and managed to get past the patrolmen who had blocked it off. I got into the plant. I was after two paychecks. We'd had a locker room, and I had changed into plant clothes, so my clothes with the paychecks were still in there—not that I couldn't have gotten the checks replaced.

It was a couple of days before we got organized on what to do and who was missing. The first thing they got me to do was to work with the insurance companies who were there to look at the damage and to take a lot of pictures. I was the guide for some of these photographers and people who went through all parts of the plant to see what the damage had been. It was really something to see.

While this was going on some of our benzene storage tanks were still burning, and there were still other fires in the area. It was quite a thing. I figured it up one time that of the people in the plant at the time, over half of them were killed. A number of people were killed who were not Monsanto employees; that's the way it worked out.

BOHNING: There was the ship that exploded before the plant exploded. Would the casualties have been in the initial explosion or the subsequent ones?

FAIR: For us they were all in the initial explosion. The next one occurred about two o'clock in the morning of the next day. I heard it, went outside, and there was a reflection of fire over in Texas City. I was over in Galveston. That's when the *High Flyer* exploded as they were trying to pull it out away from the docks, because it also had ammonium nitrate in it and it was on fire. Our losses had been in the first explosion. All of our volunteer fire crew had been exposed; so many people had been out there by the ship for one reason or another, a lot of them just looking.

The loss was heaviest among our engineers who were assigned to the operating units, because they were there involved in protecting their operations. It turned out that my assignment, as I might have expected, was in ethylene. There was only one survivor of the

engineers associated with ethylene, and he was very badly injured. He never really recovered from his injury, but in time he would be available for interviews. Then there was another person in the hospital, a plant operator, who was very helpful. Those guys should never be underrated for what they know about their plant and its operation.

We put together the remnants of our five-person group; there were three of us left. That was the start of a temporary engineering group which we located in Galveston to get involved very quickly at reworking and rebuilding the plant. We were very busy; the decision had been made to rebuild.

We had the problem of getting working drawings of the plant. Some of them had burned. Some of the tracings we had were splattered with blood and had been cut with flying glass. I remember we had a lady there with an ironing board, trying to iron out these drawings; they were all crumpled up. She used a flat iron, and she got them to where we could get copies made in Galveston on a printing machine.

For the remainder of 1947 we redesigned and then rebuilt the plant. I was in charge of the process design of ethylene. Others had styrene and ethylbenzene, but we worked as a team. It was another great learning experience. By the summer of 1948 we had the ethylene plant back on stream.

I guess as sort of a reward for my efforts, I was awarded what was called a Monsanto academic leave. This program had been started the year before. Four, sometimes five people a year from Monsanto would be given a year to go back to college. They picked their own schools and were kept on full salary. Moving and registration expenses were paid, so this was a good thing. I chose to go up to the University of Michigan at Ann Arbor, where there were people like Don [Donald L.] Katz and Bob [Robert R.] White, very prominent and publishing in the area of petroleum and petrochemicals.

So, that's what I did in September of 1948; I went up to Michigan. I took courses of interest to me in reaction, separation, thermodynamics, mathematics: fairly concentrated things. I did the equivalent of a master's thesis under Professor [G.] Brymer Williams, who is retired but still at Ann Arbor. I completed the M.S. program and was back at Texas City by the summer of 1949.

The year at Michigan was interesting in that our students were primarily people on the GI Bill. They were good and industrious and hardworking and very competitive, and they had a certain maturity, too, that the regular graduate students wouldn't have had. That was the group at Ann Arbor—a very good department of chemical engineering. Stu [Stuart W.] Churchill was a Ph.D. candidate there at the time. In fact, he taught one of the classes I took. That's when I got to know him. He stayed on, of course, as a faculty member after he got his Ph.D.

That got me back to Texas City. Just to move things along, I immediately met my future bride, Merle Innis, who had gone to work at Monsanto while I was at Michigan. She had been with Braniff Airlines. They called them stewardesses then, not flight attendants. We were married in January of 1950.

By this time interest in the petrochemical venture had returned, and acetylene had made a comeback. Acetylene was sort of in the news as a glamorous building block for many chemicals of interest. We did expand ethylene at Texas City, and I was involved with it, but acetylene was the thing. In 1950, I got an assignment to work with BASF in Germany on an acetylene plant. We were looking at two different processes, one at Chemischa Werke Hüls and one at BASF. The BASF process was a partial oxidation process. The one at Hüls was an arc process; the BASF process was the one we eventually pursued.

My assignment was to collect pilot plant data for commercial design. I was over there for several months doing that, then returned to Texas City. Stone and Webster in Boston had gotten the job of final design and construction of the acetylene plant, as well as other parts of a new complex. The year 1950 was spent mostly in Boston working on acetylene, but we had other people there working on hydrogen cyanide, acrylonitrile, and vinyl chloride. So the new complex was acetylene-based; our acetylene would go to acrylonitrile, to vinyl chloride, and to vinyl acetate. Vinyl acetate came a little bit later.

For the acetylene plant we needed a tonnage oxygen plant, because of its being a partial oxidation process. We got involved in the problem of what to do with the waste products. Monsanto was interested in this problem. It was considered important even back in 1950! We installed an incinerator there built by Nichols Engineering Company to take some of the by-products—particularly the sooty products from the acetylene plant and some of the bottoms from the vinyl chloride plant—for incineration.

Work on this complex moved right along. We bought the HCN know-how from DuPont. We had our own pilot plant in Dayton for the acrylonitrile. The acrylonitrile was made from HCN plus acetylene, a liquid phase catalytic reaction. That complex was in the last phase of construction by fall 1952, and the acetylene plant was ready.

That's where things stood. I decided it was time to return to graduate school.

BOHNING: And you were thirty-two?

FAIR: Yes, I was approaching thirty-two, would be thirty-two that October. I had gotten to know the people at Texas through AIChE work. I also had an interest in going back to Michigan. While I was there they had urged me to stay and go on for the Ph.D., but I could not

do that ethically. I did not sign a paper with Monsanto saying I would come back, but I felt that I should.

Well, the economics worked out in favor of Texas. I called the graduate advisor there, a fellow named Kenneth [A.] Kobe, and he told me at that point, "You come here, you've got a fellowship." That was impressive to my wife—and child by that time—so we packed up and moved up here to Austin, and I spent the next twenty-five months working for the Ph.D.

[END OF TAPE, SIDE 4]

FAIR: That was a good twenty-five months, and I got a lot of things done. I had a leave from Monsanto, which was protection against some emergency. There was no money associated with it, but there was the agreement that if I wanted to come back I had a job—in case there was a serious illness or something like that. I had no insurance. That's something that is a big thing now with my graduate students: they must have insurance, and I as their professor must pay for it.

BOHNING: What was the University of Texas at Austin like in 1952?

FAIR: Well, first, it was smaller, but not really small; it had about fifteen thousand students. It was smaller than Ann Arbor, Michigan, which had about twenty-two thousand students at the time, or maybe it was a little higher than that. The chemical engineering department had its own building, which was a relatively new building at the time—it was about ten, eleven years old. The facilities were very good. The faculty was very good: [John J.] McKetta was here, [Kenneth] Kobe, and [Matthew] Van Winkle, who had become well known in the separations area. They had several good people. I chose to work with a brand new assistant professor named Howard [F.] Rase. I was his first graduate student. Howard is still here. We are about the same age.

BOHNING: Why did you select him?

FAIR: Well, at Michigan I took a course in reaction kinetics, a graduate course, the first time it was ever offered. That's how new that discipline was in chemical engineering. We used Volume 3 of Hougen and Watson (7), and I just got really interested in catalysis and reaction engineering. That was Rase's field, so that's what I did for the Ph.D. I did an experimental study of dehydrogenation over a nickel catalyst of several different feedstocks. So that's the

reason I worked for Rase. Otherwise I probably would have worked for Kobe or Van Winkle. Van Winkle was a separations guy. Things worked out well.

Howard Rase was a good supervisor, and the second year I began to think about employment and had always been intrigued with Shell Development Company and the San Francisco Bay area, that combination. There had just been many key articles in the literature by Shell people; it had an outstanding reputation. It still has one, but it does a different sort of thing now. Then it was very basic generic research out at Emeryville, California. I had an interview trip to Shell, and I got an offer from Shell. I certainly had an offer from Monsanto and some others, but I decided to leave Monsanto at that point and go with Shell. I moved to Berkeley, California. It was and still is a lovely area.

The work at Shell was great, and the people were outstanding—just really impressive people. By the second year I was there, through indirect means, I got overtures from Monsanto to return. They had just the right job for me, or so they thought. I could have my own engineering research group. It was a great temptation, so I finally decided to do it—return to Monsanto. I'd had ten years with Monsanto before that, but credits for those years had all been lost; I would start all over again on building service credits. It seemed like kind of a dumb thing to do, but their work was interesting, and I really had missed Monsanto. But Shell had been really good to me, and I really had regrets about pulling out.

One of the offers from Monsanto was to go to Dayton and work at the Central Research Laboratories with a fellow there who was very well known nationally—he had written a McGraw-Hill book on thermochemical calculations—named Ralph Wenner (8). Anyway, I took the Monsanto offer and moved back East to Dayton, Ohio. I moved from beautiful Berkeley, California, to Dayton, Ohio; a contrast, but Dayton's a nice city. I don't know if you've ever been there. That's where Monsanto had its Central Research Laboratories. They wanted to start up an engineering research program, so I picked that up. I was there for five years doing fairly basic research in chemical engineering and also serving as a company consultant, particularly in the areas of ethylene and hydrocarbon pyrolysis.

BOHNING: It was during that period that the paper that we had talked about earlier came out (13).

FAIR: Yes, that's right. I was at Dayton then.

So this group came to have some twenty-five people in it, divided into five sections. We got into such things as membrane separations then. We published a number of papers; we wrote a lot of reports. This was still during the time when there was a lot of let's call it "research push" on chemical products, acetylene products. The acetylene plant was running by this time.

We said, "What should we do? Shouldn't we develop derivatives of acetylene that would be saleable chemicals?" A lot of work was done at Dayton on that, but it never really panned out.

In 1959, a new study team was formed to develop an ethylene-based complex at Texas. So I became a member of that group, primarily as a consultant, but spending a fair amount of time with the group, which was headquartered in San Francisco in the offices of the Bechtel Company. Bechtel had the contract for the study—not necessarily for the plant itself. Bechtel provided a lot of the people needed to do this evaluation, called the Chocolate Bayou Group, because we had pinpointed the location at Chocolate Bayou, Texas. There we got back into ethylene. I still was in charge of the group at Dayton, but I was doing personal consulting with this other group, in pyrolysis and separations. Howard Rase and I had published some key articles on light hydrocarbon pyrolysis (9, 10).

BOHNING: What was the problem with acetylene that you said didn't work?

FAIR: In the first place, the acetylene process was very difficult and expensive to operate. It generated tremendous quantities of soot; it was messy. The separation system had difficulties. You started with an 8 percent acetylene and refined it to 99 percent plus. Incidentally, you stored it in a gas holder right in the middle of the plant. Acetylene is a very dangerous, sensitive, explosive material, but we never did have any serious problems. The acetylene derivatives market simply did not develop. Acetylene was expensive and when you ran a comparative analysis, it was cheaper to make these materials from ethylene. The decision came along in the sixties to shut the acetylene plant down. It ran long enough to serve the needs of acrylonitrile. By this time, we had ethylene-based vinyl chloride. We then got involved with the Sohio process for making acrylonitrile and early on, at Chocolate Bayou, put in this Sohio plant to serve our needs for acrylic fibers.

Acetylene was kind of a dog. There was a competitive process called the Wulff process that Union Carbide ran for a while. Carbide also, in Texas City, had a partial oxidation plant of its own design. So the acetylene derivatives became ethylene-based, and this was now the thrust of Monsanto's next venture into petrochemicals. That plant went on stream in 1961, and at about the same time, our Dayton lab was closed, and a group of us—probably fifty or sixty—were relocated to St. Louis to a new research center which was opened in 1961. Those left behind became Monsanto Research Corporation [MRC], which was a subsidiary set up to do government contract research. Folded into that subsidiary was the operation at Miamisburg, Ohio, which was funded by the Atomic Energy Commission. So the Miamisburg and Dayton labs were part of MRC.

Back in St. Louis, our engineering research program began to dwindle. I've since studied this period; this was a time when companies such as Shell, DuPont and American Oil decided that they really couldn't afford a lot of generic research in engineering. They began to

phase it out. But we didn't have a nice, neat death of our engineering research group. We branched out into some other things; we got into biotechnology at that time. I was transferred to a commercial development group where we were looking at new products for Monsanto. I had a very interesting experience there, getting into such things as a material for putting the stripe down the middle of highways and the idea of Monsanto getting into the business of selling a floor covering surface. We studied the large-scale culture of algae as a food source, and a number of other ventures.

By this time the company had decided to expand the complex at Chocolate Bayou, and I shifted back into engineering work to deal with that complex. Also, by the early sixties, the company decided to form a corporate engineering department—this happened formally in 1964. We would bring in the engineering groups from Texas City; from Springfield, Massachusetts; from Decatur, Alabama; put them all in one building in St. Louis, and do all of our engineering work out of one central location. That was a major move for the company, and a very expensive move. As part of it, we had project groups. We also had construction groups, and we had a core technology group. This group provided tools and internal consulting. Some remnants of our engineering research group were brought into this. There were several sections, and I had the chemical engineering section. I later took over the whole thing.

Just to move things along a little bit, that's the sort of thing I did from 1964, really, until I left in 1979: heading up a technology function that served corporate Monsanto. We operated as a cost center, not a profit center, but we operated each year with a grant from the company, you might say, to do basic work, develop new computer programs, and so forth. The rest of the time we had to sell, so we became the corporate consultants. These were very good people, and they were very much in demand. We had problems spending the corporate grant because project work or plant work would take priority. That led to a problem, so what I did about that was that every summer I would bring in college professors to help us catch up on our research projects. They were very good. We paid them well and they seemed to enjoy getting the experience. Of course, I got more and more involved with academia this way. Also, in 1965, I started teaching a class at Washington University every year.

BOHNING: I wanted to ask how you made that affiliation with Washington U.

FAIR: Well, I had at one time a corporate responsibility for continuing education. You have to remember that back then, at one point, this began to be the big thing in the sixties. "You had better go back to the books; the half-life for your knowledge is only ten years!" Monsanto decided to do something about it, so I got the assignment of putting together an in-house program, and in doing this I utilized a number of the universities, including Washington University, University of Houston, University of Massachusetts, and Georgia Tech. They provided faculty for our courses. So I got to know the people at Washington U., and I started teaching a course in process design.

The first year, I taught it as a two-semester course, and I was given some leave time from Monsanto. In the spring this was a design course. We entered in the AIChE student contest competition, a national competition. The students had to do the contest problem individually. It's still going today. They had a limited time to do it, six weeks. Each turned in a report, and we could select the two best reports for the national competition. There were at that time, I believe, six prizes. First, second, third and fourth, and two honorable mentions. And, by golly, we got the first prize winner and an honorable mention in our two submissions.

This was something great, and I would say that one of the greatest moments of my career, really, was when, at the annual meeting of the AIChE, my first contest prize winner went up to accept the prize and to make a few comments, and he thanked me for making it possible. Now that was really something; a big room of people at the awards banquet, and I could just sit back there and glow that this had happened. I mention that this was Mel Bagley who made these comments, and then my honorable mention was Ron Cordes, who later worked for Exxon; I don't know what's happened to him. Mel is still with Monsanto.

Anyway, I had the design class. I taught it on Saturday mornings in the spring semester after that first year. We could still have Saturday classes. We also met on Wednesday afternoons, late in the day. I didn't always make the Wednesday class because of travel, but I could always make the Saturday class, and I always had a regular faculty member who could substitute. So that got me interested in teaching, and I also very much enjoyed the association with Washington University. I was an engineering commencement speaker, and I received an honorary degree from Washington University.

I was on search committees and did all these things; in fact I was on the search committee to find a new chairman of the department to replace the chairman who had stepped down, Eric Weger. I have to say here that Eric was asked to step down by the faculty. I didn't agree with the faculty at all; I voted against this. But Eric stepped down and helped us on our search. Within about two months, one evening he told his wife and daughter he had a meeting in Chicago the next day, and he drove out to Lambert Field, put his car in the garage, called one of the nearby motels to pick him up, he wanted to stay the night. Then he took his life. That was quite a blow. We had a memorial service for him on campus. He just couldn't take this lack of confidence by the faculty. I thought he'd done a tremendous job; I just never could understand this. We found a replacement who is there today but now planning to step down, named Rudy Motard. The department's done well; it's a good, small department. It's never going to be in the top ten, or probably the top twenty, but it's a good department. It could be in the top twenty.

I had always liked college campuses. I had been very active on the accreditation committee of AIChE. I did a lot of accreditation visits for ECPD, later ABET [Accreditation Board for Engineering and Technology]. I served on the accreditation commission of ABET, where I would lead teams, and so I got very much involved in university things. I served on the

visiting committee here at the university, as a Monsanto representative. It was during my last year on this committee that the dean took me aside and said, "What would it take you to get you to join our faculty?"

Before we take a break here, I will say that in 1979, early in 1979, I got an offer for a chaired position here, which would be the first chair in engineering at the university. I also was eligible for early retirement at Monsanto. We had the rule of eighty-five—age and years of service. I had had my early ten years of service restored—that happened not long after I rejoined Monsanto—so that I suddenly had more vacation and more service credits and so forth. I had thirty-four years of service with Monsanto at that time, so I could make the move. I gave Monsanto plenty of advance notice. In fact, I was able to pick my successor, who is an outstanding person named Stan [Stanley I.] Proctor.

BOHNING: I know Stan.

FAIR: Stan was later president of AIChE—a tremendous guy. So I was happy. In fact, he had been in my group and I had told him at one point, "Stan, you've always been in St. Louis, either at one of the plants or one of the headquarters, and I've got this opportunity for you at Chocolate Bayou to get some real plant experience." He wanted to do it, so Stan and his family moved to Alvin, Texas. He later got experience in Texas City, so he had a couple of years of this, maybe a little bit more. He was ready for the next thing; it just worked out beautifully. I could come here, and Stan moved up from Texas City to St. Louis at the same time, and it all worked out.

[END OF TAPE, SIDE 5]

BOHNING: We were about to talk about Texas, but before we do that I want to ask you a few questions. During that period that you were at Monsanto, according to your bibliography, I counted maybe thirty to forty papers that you had written while you were at Monsanto. I wanted to ask you about a couple of them specifically, but first I wanted to ask about the attitude of Monsanto towards your publishing this kind of information.

FAIR: Well, this group that I'd had, which was sort of an engineering research activity both at Dayton and then later at St. Louis, did enough generic-type work that was not process or product sensitive, so we could usually get publications approved. It might take a while; we might have to sit on it for a while, and I encouraged that in the group. In other words, I had really top-flight people, mostly Ph.D. Chem E's, and I said, "I think you ought to always worry about your visibility, your employability on the open market. We'll try to hang on to you, but

think about your résumé. There ought to be some publications on it, particularly if you want to get into teaching."

So our group did a fair amount of publishing—the only difficulty being that some people were not all that inclined to write, even if they could get clearance. I think Monsanto had a pretty good attitude. I know there was one paper that was cleared for a person in my group—it was a solo paper—which had to do with some real mistakes that Monsanto had made involving a start up. My feeling was well, this would be a service to industry, so others wouldn't make these same mistakes. It did make us look pretty dumb. We had to argue over that one a little bit, but they finally agreed to do it; it was published in one of the AIChE journals (11).

Our work was enough generic that we could do it. I also encouraged these people to teach short courses; that was permitted in our department contract. Since I was chairman of the AIChE Continuing Education Committee at one time and a charter member, and we had an in-house program—it was a very good one—it was pretty easy for us to provide a course that was wanted by the AIChE. In my group I got six or eight of the people into teaching AIChE courses, and some are still teaching. I think Monsanto has a very generous, maybe liberal, policy on sharing its technical developments with others—as Shell Development had back in the forties and fifties, and DuPont also, through the years, including Perry's Handbook (12).

BOHNING: Yes, I wanted to talk to you about some of these handbooks.

FAIR: Oh, Perry's Handbook was a tremendous contribution to the profession by DuPont.

BOHNING: I think that came later.

FAIR: Well, I was at Monsanto when I began my contribution to the handbook.

BOHNING: Yes. That's right. I looked at that paper that you had on the history of the handbook (13). Of course your interest in history shows up in a number of other papers as well. How did you get involved in doing that?

FAIR: I worked side by side at Shell with Bob [Robert H.] Perry, the only child of John [H.] Perry, and I was always fascinated by stories of his father and the handbook, and the money that it brought into the family. Well, when Bob took over the handbook, he recruited me to do a section. I talked with him about the story, saying, "We really ought to tell it," and so we wound up by splitting it into a two-part paper—one part by him, one part by me, with my part being

approved by him. I didn't want to put anything in there about his father that he wouldn't want to see, but he was very happy with it the way it was. I took John Perry up from his early days and how he'd put this thing together, and it was a very successful enterprise.

Now, as you may know, Bob Perry was tragically killed in an automobile accident in England. McGraw-Hill and his widow had lots of negotiations about the handbook, but there will be a seventh edition (14); they're beginning to talk about it. The editor-in-chief now is one of Bob's old associates at the University of Oklahoma, Don [W.] Green, who's now at Kansas University on the faculty. There is the criticism of the handbook these days that it is no longer really a handbook—that it's too involved in theory and fundamentals and that what the profession really might want is just a handbook. Give references to how you got these pieces of information, but make it simple and useful. I don't know. That's a comment that's come in many times. It may be ripe for competition on that basis.

BOHNING: John Perry was originally in Cleveland.

FAIR: Yes.

BOHNING: Was he associated with Case at all?

FAIR: No, he was with a DuPont subsidiary company.

BOHNING: Graselli?

FAIR: Graselli. He got started on the handbook while he was in Cleveland.

BOHNING: Because Hodgeman, who edited the first chemistry handbook, was at Case at the same time.

FAIR: I see. There could have been a lot of influence there.

BOHNING: I just wondered whether there was any connection there at all.

FAIR: I bet there was. Now, right behind you—see, there's a green book to the left of that—is an earlier handbook of chemical engineering, a McGraw-Hill book (15). It's a two volume set, right there.

BOHNING: Oh yes. Okay.

FAIR: So, there had been something of a handbook before Perry's. The red book over to the right is a first edition, then the dark green one is a second edition.

I met John Perry at an AIChE meeting, but I knew him mostly through his son. He was chain smoker and died of throat cancer, at a relatively young age.

BOHNING: Many of your publications while you were at Monsanto dealt with distillation technology in one form or another, and you had a number of annual reviews, current techniques kinds of things. I'm just looking at a title here that I pulled aside, "Conversion of the Transport Phenomena Purist" (16). [laughter]

FAIR: I gave that at an AIChE meeting as part of a session at a time when some chemical engineering departments, including Washington University, had dismissed traditional chemical engineering and were going into more of an engineering science program. This was happening back in the sixties, and the idea was to emphasize a fairly theoretical and pure type of training for the engineer, and let industry convert this person into a useful, productive engineer. [laughter] That had to do with that question: "Here's what he knows when he comes in; now, what do we have to do with him in industry to make him useful?" It got a lot of interesting comments.

BOHNING: I can imagine.

FAIR: It grew out of an invited paper at an AIChE meeting.

BOHNING: Another one is "Educating Tomorrow's Process Designers Realistically" (17).

FAIR: That was a collaboration with Buford [D.] Smith and a fairly comprehensive article on how industry and the university can work together to teach undergraduates the elements of design. Now, in our curricula in chemical engineering, we have a very firm requirement that all

engineers take a capstone course in design—one that will bring together their thermo, their kinetics, their separations, their transport operations, and, so that they can all be used, blended with economics. That's a requirement. It worries many faculty people because they don't feel qualified to teach it.

What we initiated, Buford and I, was a program to get industry people into the classroom to do this course. We had four special meetings, at a rural location in the Missouri hills, sponsored by the National Science Foundation and The Ford Foundation. We brought in at one time maybe fifty educators and, in some cases, an equal number of industry people whom we could identify as being potential collaborators. The concept worked out pretty well, and some of those collaborations are still intact. We had Standard of Indiana working with Notre Dame; Standard of California working with Berkeley; UOP working with Northwestern; Phillips with Arkansas; and so on. At the sessions we would go through the methodology of teaching a design course collaboratively. The meetings were very popular. We had a fixed number we could accommodate with our money and space, and we did that four times. These were so-called Brownwoods Conferences, and at those conferences, Buford and I and usually a couple of others from Monsanto would take turns dealing with various topics.

That article summarizes some of the things that we accomplished, and I think it would be a good article for some people to review again, because we're getting away from the collaborative concept. Most of our faculty these days have not had practical design experience, so they really aren't qualified and really don't want to teach the course. At this very time, our department is searching for my replacement—someone with a separations background who's willing to administer the separations program and teach the design course. [laughter] The last one is the tough one.

BOHNING: It's interesting what I've heard and I've noticed about chemical engineers today. Someone said to me, "Nobody's building a plant anymore, and the concerns are the environment." Is that partially responsible for this?

FAIR: Well, I think it's the economy, but there is a lot of capital spending on waste handling treatment facilities. That's still going strong, and may involve the design of a large and expensive plant. Also, there are some plants that result from environmental causes, such as plants to produce additives for gasoline. I don't know whether you've come across the MTBE— that's methyl tertiary butyl ether—which is sort of a tetraethyl lead replacement for gasoline, to improve the knock rating. There is no lead to get into the environment or foul the catalytic converters. MTBE construction is going pretty strong; there are some ethylene plants, but by and large, there is a lot less classical plant design and construction. More chemical engineers are being involved in microelectronics projects, not so much biotechnology projects. We just have to see what the future brings.

I remember working on ethylene plants, thinking that now that we're up to x billions of pounds per year, we surely don't need any more ethylene capacity; but it keeps going up. I don't really fear for the industry. Although right now the so-called commodity chemicals are not setting things on fire, they are stable. Monsanto ten years ago decided it was going to go very heavily into biotech. They've spent a lot of money on biotech. They do not yet have their first real product from their research. It hasn't been cleared yet. The industrial chemicals keep bringing in the cash: fibers and fiber intermediates, organic and heavy inorganic chemicals, and certainly petrochemicals. So we're still there. [laughter]

BOHNING: What about, "Dictation and the Engineer" (18).

FAIR: [laughs] Now, there's one. Well, that one grew out of a discussion I had with one of the editors of *Chemical Engineering* magazine. It really had to do with a little crusade that I had in my department. This was before word processors. Our engineers were taking an outlandish amount of time to prepare simple reports and letters. The usual practice was to write something out in hand, ask the secretary to do it in draft, correct it, and then the secretary would type it in final. My theory was, why don't you dictate it? You can still get a draft if you want it, and you correct that. Better still, dictate it in final.

That's what this paper had to do with—techniques for leapfrogging to the final copy, the discipline involved. It's been very helpful to me; I dictate in final. It has changed a little bit now, because I can dictate something in final, and I will get the beautifully typed copy, but it's been done on a word processor. I know that if I want to change a "was" to a "were," there's no sweat, so that makes you a little bit more relaxed on dictation. Also at that time, there were a lot of people around—at least at Monsanto—who could take dictation directly. I like that. That's the way I prefer to do it, but the people have to know shorthand. You don't find very many people who know shorthand anymore.

BOHNING: That's true.

FAIR: So that article talked about pocket dictating machines, how you might dictate a technical report, how you deal with equations.

BOHNING: That is an obvious question.

FAIR: The answer was that you need your equations prepared in advance pretty well. Then you have the secretary leave space for those equations and then put them in after the second pass. You just can't dictate equations.

That was kind of a fun thing. It was part of a special section on technical writing.

BOHNING: Did you get much response to it?

FAIR: Oh, yes. Yes. I've still used copies of that old article for some of the women around the building, even here. I've always gotten good marks for my dictation, and I think it's because I can, better than some, visualize what it's going to look like on the paper as I talk about it.

BOHNING: You did an international textbook on distillation (19).

FAIR: Yes. That was a little book to teach non-chemical engineers how to handle some of the elementary calculations of distillation. It was used quite a lot in Monsanto by the plants and their operator training programs. It's not a very impressive book, but it's got the principles of distillation and graphical materials in it.

BOHNING: So the impetus came from Monsanto for this.

FAIR: Yes, for training.

BOHNING: By 1972 the fifth edition of Perry was coming out (20), but I was intrigued by the fact that you were emphasizing environmental concerns. You had a paper called "Trace Quantity Engineering" (21). In there you talked about the already growing environmental concerns.

FAIR: Yes. That was, I guess you might say, sort of an attempt to develop a new discipline called trace quantity engineering. It was all built around the need for control and analysis of small amounts of materials that could be harmful to the environment. That was the thrust. Sampling and analysis were very much involved, a choice between alternates: for example, adsorption versus scrubbing versus incineration. That's really what it covered: cleaning up of liquid and gaseous discharge streams.

BOHNING: What kind of response was there to something like that? To me, it seems like you were probably on the forefront of calling attention to those concerns.

FAIR: I would say that the response was not all that great if you measure it by requests for reprints, which is hard to do these days because of copy machines. I've encountered a lot of people who recognized the article or recognized the author of the article, but in general it did not touch off any usage of the term TQE. We used it around Monsanto, because of that article. At the time there was a commercial that said, "Did you DQ today?" That means, did you go to Dairy Queen? "Did you TQE today?" [laughter] It was an attempt to get people interested in not just cleaning up but finding out by analysis and evaluation whether clean up was even needed, based on regulations.

BOHNING: Well, in your short bio at the end of that paper, it indicates not only your Monsanto connection, but also that you were a member of the executive committee of Fractionation Research Incorporated. That was the first time that I had seen that term.

FAIR: Yes. Well, we call it FRI; it's still in existence. It was formed in the mid-1950s as a stock company, a closed research company. The shareowners would be companies interested in the results of distillation research. FRI had its officers and staff and research facilities in California; it has since relocated to Oklahoma. It has perhaps seventy or eighty member companies right now, and a fairly big budget. Its technical program is steered by a technical committee, and my first assignment was on that committee. They had, at the time, six members of the committee from our various companies. It's grown to nine now, actually nine plus a chairman. When we formed this corporate engineering department in St. Louis and I brought in Bill Bolles from Texas City to be my distillation specialist, I got him on the technical committee. They asked me to serve on the executive committee. In that capacity, I got involved in negotiations to change FRI from a stock company to a member company and to negotiate with the IRS for it to be considered a non-profit company and not taxable. I served then on the executive committee and was vice president until I left Monsanto. I now attend the meetings as a consultant. I keep up with what FRI is doing because it could result in duplication of the work that I'm doing here at the university.

BOHNING: Another paper during that Monsanto period was something called "Process Design: Past, Present and Future" (22). That was twenty years ago. One of the things that intrigued me, and you touched on it earlier, was how you noted the decline in the coursework required for a degree in chemical engineering.

FAIR: Yes, it's really unfortunate. We're right now faced with another problem here at the university. At that time we were realizing that you really needed five years, and the most appropriate thing would be a five-year professional degree. That would get in those hours that you really needed. That has not sold; people don't want to spend more than four years worth of tuition. We have simply had to do away with a lot of very useful courses. The thing that was troubling me at the time was that I felt academia was saying, "Well, that's okay. Let industry do it; let industry do the training. We don't need a design course. They can go to industry and they can take a good in-house design course." There's a lot of controversy about this, and, at the moment at the university, we're faced with having to free up six hours to take courses of a multi-cultural type. There has been that kind of pressure put on us to do that. Engineering's opposed to it. We have a problem in engineering of attracting students simply because our workload is heavier than the other colleges; it has always been that way.

[END OF TAPE, SIDE 6]

FAIR: So we are now edicted by the state of Texas to have three hours of government and three hours of history. The government is U.S. and Texas, the history can be any of the several alternate subjects, elected-type subjects. So we've got six hours that we are required to have, and here will be another six. There will be twelve hours, and the situation begins to be difficult. In that paper, we were talking mostly about the educational aspects and whether we ought to have a five-year program to get everything in.

BOHNING: Well, it was twenty years ago, virtually, when you wrote that, and twenty years later, what do you see?

FAIR: Well, I'd have to go back and take a look at the article, but we certainly have not gone to five years and probably won't. We've had to cut things out of the curriculum—sometimes things of a more applied type—on the basis that the student will pick those up somewhere else. I'd have to go back and see what was predicted at the time. There was a group of people who were involved in that session. The five-year need is the one that stands out most distinctly in my mind.

BOHNING: Well, I know in my own experience that the number of credit hours required for a chemistry major has dropped from one hundred forty to one hundred twenty-one, one hundred twenty-two, something like that. That's a lot of work to cut out.

FAIR: It is. Right now, we need an additional required course in chemistry, but have compromised on the basis that it can substitute for the second semester of physical chemistry, which is heavily oriented toward quantum chemistry, at least at this school. The needed course is in modern analytical methods, very important to the chemical engineer, with stream analyzers and needs for trace quantity analysis. I recommend it to all the students.

I think they'd probably be better off with that one than with the other physical chemistry, but I'd hate to see them lose that physical chemistry. How do you put in another course? Now, of course, the pressure is also to get in some life sciences courses, that every Chem E needs to have at least a course in biology, general biology, perhaps another one in microbiology, and so on.

BOHNING: The rise of biotechnology.

FAIR: Yes, but we need it. We need some sort of an acquaintance with the topic, anyway, by the undergraduates, and the same in the microelectronics area. We have an elective for that now, in chemical engineering. It's fairly popular.

BOHNING: Do you think—I'm going a little afield here—but do you think that this need to have all of this background at the undergraduate level—and that's only expanding as things change—may require even more specialization at the undergraduate level?

FAIR: Well, I think it will. In fact, it already has, here. We have blocks of electives. It's required that you take three courses out of one of those blocks of courses, one of them being biotechnology. You will see listed maybe six courses, and the student has to take at least three of those. One or two of the courses will have an asterisk meaning, "this is required." You can choose biomedical as another one, process control as another one, product development as another one, and so forth. We require that little bit of specialization within the curriculum. Environmental engineering is a specialty, and it's a very popular one.

BOHNING: Oh, yes. Yes.

FAIR: For this there will be a course listed over in civil engineering that they take; there may be a course in chemistry. It's a good thing, and it would be even better if we had more hours available. We've already started that kind of a specialization. You just have to do it.

BOHNING: Maybe this is a good lead-in to this next paper I have noted, which is "Competence of Engineers: Who Should Judge?" (23).

FAIR: I'd almost forgotten that one. [laughter]

BOHNING: Well, I was intrigued. I just pulled one sentence out from there, which I'm quoting now. You said, "Industry has its special methods for assuring competence in its engineers."

FAIR: Yes, those of course deal with performance reviews and compensation. I believe that paper—I haven't looked at in years—would deal with registration. I remember now—there was the question of whether there should be re-licensing. It's fine if you get a license, but does that tell the world that ten years later you're still competent? Should you have to be examined periodically? The question was, should that be thrust on engineers by professional societies and state licensing boards and so forth, or should industry take care of its own? If I'm employing this engineer, is it up to me to be sure that he's competent and keeps up to date?

I don't know that there was anything profound here. There was a question of what should the societies do about this, and I think our conclusion was that we should provide, as professional technical societies, continuing education programs, but that we should not be in the business of examining and re-validating competence. It was an interesting part of a panel, a discussion that we had.

BOHNING: Well, I guess that brings us to 1979 and your arrival in Austin.

FAIR: There's one thing that I wanted to mention that happened during this time. In our department at Monsanto, we did a lot of pioneering work in what is called process simulation, computer-aided design. We built a program there called FLOWTRAN. It quickly became recognized as one of the best in the business. Early on, we made it available to the universities, but they could only access it on a service bureau basis. We also established our own service bureau—a hotline service for customers. We had a lot of companies hooked into our FLOWTRAN system.

The company at one point then came to me and said, "We've got to get out of this business. It's peanuts; it's not really Monsanto's charter. You have a year; phase it out." So, we got busy and decided we would certainly phase it out, but we would see if we couldn't license the package. We were very successful at this, and so we had eleven or twelve companies that bought licenses, first at one hundred thousand dollars and then at one hundred twenty-five thousand dollars, and then they paid us a support charge. A number of companies

then had FLOWTRAN available to them. We put out a book on the use of FLOWTRAN; we made the system available to the universities (24).

Then in the mid-seventies, the Department of Energy [DOE], not too long after the Arab oil crisis, funded a program at MIT to develop what was known as the Advanced System for Process Engineering, ASPEN, headed up by professor Larry [Lawrence B.] Evans at MIT. They had a lot of money. They got started, and finally they came to us and said, "We just need a base system. We've got to build our system on a good local system, and yours is it. Will you license it to us?" Well, if we're licensing to a government project, there's a question of security. They finally got around the security issue, so we licensed FLOWTRAN to the ASPEN project for five hundred thousand dollars. The ASPEN project then took this over, modified it, extended it. They had some very good people working on it. They completed the project for the Department of Energy. They now had a system which was in the public domain, but it turned out to be very difficult to use. This group was in place at the end of the project, so they decided to form a company to help people use this program, called Aspen Technology. The group then took the public domain program and started improving it and making it available separately to clients, and it has been hugely successful.

I don't know whether you've come across Aspen Tech. Headquartered in Cambridge, they've got their computer-aided methods in use all over the world, including Mainland China. They have an excellent program. They've got a large database; they've added a lot of functions. It's basically FLOWTRAN, but it really is, some would say, the best in the business. I go back for their special meetings; they had one last fall, called Aspenworld. I went to the one before, which was in Amsterdam. It is just an enormously successful enterprise by chemical engineers. One of the principals of the company I had loaned to the DOE project from my group stayed on; he's the vice president for marketing, and he's doing well. The president is one of our ex-Monsanto people.

We pioneered that at Monsanto. We spent a lot of money, and the company was very patient. We made it available to the universities, and there is a group of chemical engineering academicians called the CACHE Committee—Stu Churchill would know all about this—Computer Aids in Chemical Engineering. I got a very nice award, plaque and all, for that back in the late seventies because I had gone to my management at Monsanto and said, "We think these students ought to be exposed to a Monsanto process simulator. They don't have to have the code; they don't have to know how it works, but they should get some experience with it." The company went along with it, not only let students use FLOWTRAN, but made a series of grants to the CACHE program. That was a successful undertaking.

FLOWTRAN is still being used in the schools, although it's now not difficult to access. We have ASPEN at the university on an educational basis. That was a big accomplishment at Monsanto, and it really gave Monsanto, I thought, excellent publicity as being a first-rate technical organization. Now, things may have changed since I left—they don't maintain FLOWTRAN as they used to—but it was surely going well at the time. We demonstrated it on

some plant start-ups, how effective it could be to ensure a smooth start-up. We had classes in-house; all of our process engineers used FLOWTRAN.

BOHNING: How soon did computers enter into your work? You were using computers well before this.

FAIR: Oh, yes. Computers were just getting into the act during my latter days at Shell, in 1956. That same year, Monsanto got its first computer that would be useful for technical calculations. Before that, they were all accounting-type computers. Our first computer programs out of my group in Dayton came out in late 1956 or early 1957. We were pretty well up to date in this area. Our mainframe was in St. Louis, so we had to go to St. Louis to run it. We didn't have telephone access. It was pretty primitive by today's standards, but I had a couple of people who spent a third of their time in St. Louis, going to the computer center, in the fifties. I think you'd find, if you looked into the history of computers and chemical engineering, that the mid-fifties was the turning point. Since that time, anything of a large, particularly repetitive-type calculation is done on the computer.

BOHNING: I would think that in design work that would be crucial.

FAIR: Oh, yes. Yes. You just don't do that by hand anymore.

BOHNING: All right. In 1979 you moved to the University of Texas at Austin. In some respects, as I look at those latter years at Monsanto, you almost had an academic job before you came here. You were publishing papers.

FAIR: Yes, I had a lot of freedom. Many, many people agreed with me that I had the best chemical engineering job at Monsanto because I had a good budget; I had excellent people with high performance. We got the reputation of being able to solve the tough problems. I was on review panels for our technical ladder program so that I would review distinguished fellows' work every eighteen months or so. I think I had nine or ten of these technical ladder people in my department. I had the ear of senior management. We had a vice president for technology named Monte Throdahl. I was on his advisory board and we met at least once a month, over in the Board of Directors room. I had a good job. I could travel as much as I needed to. It was a good job, but when I left I had done it for ten years, and I was ready to do something else.

BOHNING: What was it like when you first arrived here?

FAIR: The thing that worried me most about coming here was that, as an outsider from industry with the first chair in engineering, there would be a lot of resentment, which I could understand. I never detected any of this. I think it helped that I was a member of the National Academy of Engineering at the time. I had received a number of senior level awards, but one cannot be certain of how well he really is received. I found the people to be very helpful, and I seemed to be accepted pretty well by the group. I knew that if I was going to start a second career of any substance, I would have to get busy and get some research going. I think there were a number of people—and this still prevails in other instances—who felt "Here's a guy who's taking early retirement from Monsanto; he's going to come down here and take it easy." I'm reminded of a friend of mine up at Air Products who decided to take early retirement, a fellow by the name of Jack [Jacob M.] Geist, a member of the Academy of Engineering. He thought he would just go get himself a university job, and he went down to North Carolina State and interviewed. He came back and said, "Do you know what those people want me to do? They want me to teach at least one course a semester. They want me to write proposals. They want me to have graduate students. All I want to do is go down there and maybe once a week have a little seminar where I would tell them about my experiences." [laughter] He tried it on Lehigh, too, which was closer to home; it just didn't work. So, I knew that if I was going to make anything of this, I had to get busy and get some things going.

That led to this Separations Research Program [SRP], which I think is considered to be very successful here. We have a lot of industrial support. Industrial support for research had not been too common in this department. The money would come from government agencies, primarily—some state agencies—for research. Now, industry was putting in some money in the form of unrestricted grants. Those are relatively low dollar figures, but if you have fifty companies putting in five thousand dollars each, that's a lot of money to play with by a chairman and the faculty. You can have dinner parties for staff, or you can support research assistants or teaching assistants with that money. Well, that support still comes in, and we can support sixty graduate students or more with money that largely comes from industry.

We have thirty-five companies putting money into this program, from all over the world. We have reviews here in Austin twice a year; every eighteen months or so we have a review in Europe. We just had one in January at British Petroleum in London, and before that at Terneuzen, and Dow was the host of that one. The next one will probably be in Finland. It will be in the summer of next year. We have an international group; we have some good students. We do things and get results that are, in part, fairly readily useful to the companies. That's been important. They say that I'm the cause of that because of my industrial background, but I don't think we'd have these companies with us if we'd done only pure research. We do a combination of fundamental and applied, and then add to that some "goodies" for industry, like computer programs, design manuals, special state-of-the-art reports. So we've got a good thing.

BOHNING: Did you have this in mind from the time you started here, or did it take you a while to develop it?

FAIR: I didn't have this sort of thing in mind. I certainly had in mind some sort of a program where I would get several companies to put some money into it. I was talked into this by the person who's now our dean of engineering, Herb [Herbert H.] Woodson, and it coincided with plans for a new building at our Balcones Research Center. He was then director of the Center for Energy Studies and chairman of electrical engineering. We got space allocated in the new building out there, and the Board of Regents gave me $3.6 million for equipment for that building. I had fun with that. [laughter] I will give you a brochure. We have a first-class separations research facility that extends into a fairly large scale of operation. We have seven faculty; we have postdocs. We have visiting scholars, visiting faculty people of various types. This sort of thing now has really caught on. Almost every university has some kind of a collaborative project.

BOHNING: This was a pioneer effort, would you say?

FAIR: Yes. It was certainly the first in its area and in its way of doing business. We've had some very loyal support companies. They're the ones you might expect. You've got DuPont, and Dow, and Carbide, and Monsanto; you've got Exxon, and Shell, and Amoco, and what used to be Sohio, now British Petroleum. You've got Lummus and Kellogg, companies of that type; Ethyl Corporation. Just a whole host of top-flight companies who value good technical work.

[END OF TAPE, SIDE 7]

FAIR: We have had some companies come and go that I guess I would say I was a little surprised they came in at all, like CPC International, or Pennzoil. A. E. Staley came and went.

The SRP is very good for the students. A Ph.D. candidate will probably present his work two or three times before this industrial group in the course of his studies. He or she will go to dinner with the group or to lunch with the group. This student will get a chance to get to know people who are there, who are interested in his work, and who might be interested in offering him a job. It happens. Now we see when we have a meeting of our SRP sponsors, maybe eight or ten of these people coming in, out of maybe fifty or sixty, will be our own graduates. They're now at the companies, and they're coming back to hear what we're doing and write a report. I'll show you one of our newsletters where we had a picture of this.

It's been just a lot of fun. It does take a lot of time, because we have thirty-five companies and you're their point of contact. You get a lot of phone calls; you get a lot of visits. Next week we're having 3M in to spend a day with us. They say they're very seriously interested in joining, and I think they are. It takes time. We're looking for another person—I mentioned this earlier—who could handle some of the administrative work, and teach the design course, and just let me do some of my own research in separations, and writing. It may happen by next September. Meanwhile, I'm a full-time faculty member, but it's been a lot of fun.

BOHNING: What kind of expectations did they have for you? You had in your own mind what you wanted to do. Were you given any expectations from the university?

FAIR: No, I really wasn't. I was expected to teach a class every semester. That was it. They had to sort of bet that I wouldn't come down from St. Louis and just sit around. See, I came with a tenured appointment and a chair. As a matter of fact, I switched chairs a few years ago.

BOHNING: Yes.

FAIR: The reason was that, for the first chair, the university was willing to accept a five hundred thousand dollar endowment. Well, that after a while began to get a little crowded, so they found another chair, the McKetta chair, with over twice that endowment. There's another story to that. The John McKetta chair was funded by McKetta's business and professional associates. He is an emeritus professor of our department and a long-time friend of mine. He wanted me to have that chair. [laughter] So John McKetta helped.

But the university really didn't place demands on me; there wasn't anything that they told me that I was expected to do. They might have been saying, "Well, he's done pretty well so far; we'd expect him to continue to do so." The university was generous in giving me some start-up money for research, and has been generous since. It has been a great experience. I think that now I'm coming up on thirteen years. That's not as long a second career as I would like, but it's not too bad, and it won't end next July, I hope.

BOHNING: How was it attracting graduate students when you first came?

FAIR: It was not difficult at all because these students had heard of me and had seen some of my publications. I immediately loaded up with as many students as my research funding would support. You do have to recognize that in chemical engineering here, as in most institutions, our students have financial support—if not with money from the faculty or the university, then from

their own countries or their companies, or in very few cases from their families. That's part of the game—if you take on a student, you have to have the money to support the student. Right now, I happen to have three students who are here on Mexican scholarships. They pay their tuition and fees, medical insurance, books, so that's kind of nice. In some cases I will augment that money a little bit, so the students can afford to buy a computer, that sort of thing.

I didn't have any trouble in attracting the students. It was more a problem initially just getting the funding for them. I had one student come in, right after I got here. He wanted to work with me. He was from Taiwan and turned out to be one of the best communicators and one of the smartest students I have ever known. He was willing to be a grader for other people, initially, until I could find the money. It worked out pretty well.

BOHNING: You had a number of people who started producing master's theses quite early. Your first Ph.D. thesis student was in 1984, I think.

FAIR: Well, the first two students I had for a Ph.D. were [Hong] Chan and [Frank J.] Matthews, and then [Joan M.] Schork. The first three students I got were master's people. Perhaps the fourth, but I've forgotten right now the sequence. That was good too. Before I had developed some direction to the program, I could work with shorter projects. They were all experimental research projects. Also, the commitment for the M.S. student was for less time. So that was good for me because that gave me enough time to formulate what more expanded programs I might want to undertake.

BOHNING: The direction of the research that you did here still seems to follow pretty much, well, distillation in a broad sense, I suppose. Although I guess if one were to pinpoint your areas of specialization, I've seen heat transfer being a prime area.

FAIR: Yes, I was interested in separations and heat transfer, and I've had some projects on heat transfer, some on combined heat and mass transfer; but the areas have been more distillation, extraction, and, in more recent times, adsorption. Right now, I have more students in adsorption than I have in the other fields. Within extraction we got involved early in the game, in supercritical fluid extraction. We have a nice facility for that too.

BOHNING: One of the things I also noticed is that you got into, very early it seems, structured column packings versus random packings.

FAIR: Yes. I think that we have really the premier know-how on that here, and we've certainly made presentations on that all over the world and will do so again next September. That was a case where a device that was developed in large part empirically was found to work well for the cases that were applied, and it intrigued us as to why it worked as it did and what the contacting mechanisms were for developing a thin-film liquid surface—a thin film of liquid on a textured, specially treated surface. The extent of the film formation depends on this surface as well as the wetability of the surface material.

We've done a lot of work on structured packings, and I think it's been very good work. The packing type, of course, is not proprietary with us at all, but it's been big business for the packing vendors and also for users. For example, down at the Texas City plant, one distillation column was retrofitted with structured packing at a cost of over eight million dollars ten years ago—just one distillation column. But it was justified; it was a vacuum column for styrene. I would say that our know-how on structured column packings may be better than that of the vendors, with the possible exception of Sulzer Brothers in Switzerland. We can't tell; they don't disclose all of their knowledge.

BOHNING: Three years ago, you gave a lecture for the Joe J. King Award (25). Unfortunately, since it was local, I wasn't able to get a copy of it.

FAIR: I have many copies for you.

BOHNING: Okay. But the title, of course, is obviously one that catches the eye, "Separations: Essential to Life and Well-Being."

FAIR: Well, that was for a special lecture—and incidentally, the King Award lecture for this year is being given this afternoon—for a very mixed audience. I had really treated it as a lay audience. My wife was there, her brother was there. I really dealt with the separations that would be familiar to these people. I have to do that fairly often. I know I gave a talk on the SRP to a group of purchasing agents from various companies around Austin, including state purchasing people.

I got up to the stand, and I had with me a tumbler half filled with water. Then I got out a jigger glass that was actually filled with tea. I said, "Now, I'm going to undertake a very simple operation. I'll pour the contents of this jigger glass into the water, stir it with my finger, and I've got a mixture. Real easy. Suppose I wanted to take that brown liquid back out of that mixture? Extremely difficult, because alcohol and water form an azeotrope, as you know." The idea is that it's easy to mix, but it's hard to unmix, to separate. That's kind of the approach that I used in that lecture. If you have any extra space, I'll get you a copy.

BOHNING: Sure.

FAIR: If nothing else, it will show you what the university does for this undertaking.

BOHNING: Thank you.

FAIR: It mentioned, for example, the separations in the body. One of our faculty members is very much involved in the development of artificial kidneys. That was just a summary article, but you'll find in there certain material on membrane separations and so on.

BOHNING: In the last few years you've had several papers which I assume are coming out of the separations group. For example, there was one in 1987 with three authors, and you were the only one from the University of Texas (26). There was somebody from Rohm and Haas and somebody from Garrett. Then a few years later you had another one with somebody from Germany (27). I'm intrigued at that variety of authors.

FAIR: Yes. We get into this with the separations program. In the case of the one from Germany with John Stichlmair, I got him to come over and spend a semester with us. He's at the University of Essen. We started some collaborative work that we continued after he went back. Then I got him a second grant to come back, and that time we plunged into a book, a comprehensive distillation book, which we're working on right now. He has a Macintosh in his office, and I have a Macintosh in my office. They're compatible, and we send diskettes back and forth. That's what happens in these things.

They're likely to be SRP involvements, but in some cases they're simply collaborations. Last summer, Mike [Michael W.] Biddulph of the University of Nottingham and I had a paper (28). But again, I got Mike to come over here and spend several months with us, and we worked on a project together.

BOHNING: What about, "Distillation: Whither, not Whether" (29).

FAIR: [laughter] Well, I don't know about that title. They didn't change the title that I had given it. That was the keynote opening address of the last International Symposium on Distillation, which was held at Brighton, England. It was published as part of the proceedings

of the conference, and they later contacted me and said, "We think that it was kind of buried in these proceedings, would you like to put it in our journal?" Which I did. It's a pretty prestigious publication of the Institution of Chemical Engineers. That's how that one happened. It was a look down the road, if you will, and whether there's any life in distillation; and, if so, what directions it should go.

BOHNING: Just a year or so ago, if one were to look at this list and the titles and nothing else, you would seem to be a real disciple for distillation. I have this one from 1990, "Distillation: King in Separations" (30).

FAIR: [laughs] Well, I'll tell you why this is. If one has a liquid mixture to be separated, if it is amenable to distillation separation, then that will be the cheapest way to do it. There's one particular advantage that distillation has over the other larger scale separation methods: it does not involve what we call a mass separating agent. If I want to separate a mixture using adsorption, I have to find the right adsorbent; by extraction, I have to find the right solvent. Distillation uses energy. It can be brought in, in various forms. Energy is energy, you might say. It simplifies the research and development costs, if you can do it.

One of the things that we're very much into now, and there's at least one paper on it (31), is the use of artificial intelligence approaches to develop expert systems. I have two people now working with me on what ostensibly will emulate a person, the expert, in directing the development of a design: what's the best way to separate this mixture?

Invariably—you've probably had this experience—the junior engineer goes into the office of the gray-haired expert. He's made an appointment, and he has this mixture. The question is, "How should I go about separating this mixture?" What are the first questions of the expert? "What are the boiling points? Do the boiling points look spaced out to the right extent?" The suggestion is, "Let's try distillation first." The boiling points might not be spaced out enough. There might be azeotropes; there might be heat sensitivity. But my point is that the first consideration is, see if distillation will work. Because if it will work, that's going to be the way to do it. The problem's done.

What you get into are strange mixtures—perhaps lopsided mixtures, perhaps very dilute mixtures—where distillation really is not appropriate. Then there are gas mixture problems. What's the best way to separate air? I'll tell you, fifteen or twenty years ago, the answer would be easy. It might even be, what's the <u>only</u> way to separate air? It's by cryogenic distillation. All oxygen plants were distillation plants. That's certainly not true today. You have to look at pressure swing adsorption, you have to look at membrane separation, as two ways to do it. And there are some chemical methods that are being developed too. For gas mixtures such as air—even if the boiling points of nitrogen, oxygen, and argon are spaced enough—distillation may not be the way to do it. So the expert's system has to have qualifications built into it. It has to

differentiate between a lean mixture and a concentrated mixture. But distillation is still around, and it's so heavily used in the industry, you can't walk away from it as long as there is anything left to research, and there is. As long as we've been using it—and that's a long, long time—we still don't understand a lot about it.

We talked earlier about Fractionation Research, FRI. FRI does equipment testing. If you start looking around the United States at any university where there is distillation research, you won't find much. You will find some theoretical work at Massachusetts and at Clarkson; that's about it.

So it's still with us. It's a large energy consumer, too, so there's been a lot of interest from a conservation point of view, to reduce the energy consumption in distillation.

[END OF TAPE, SIDE 8]

FAIR: So, since there is not activity elsewhere in the U.S., we're doing some work here, and there are some who say if we were to cut out our distillation program, we'd lose quite a few of our sponsors of the SRP. We're continuing to do it even though it's not, shall we say, the "in-thing" or the popular thing to do.

BOHNING: I have some other brief notes. We've talked a little bit about your students. Are there other comments you'd like to make about your graduate students and some of the things that they've done since they've left here?

FAIR: Well, my graduate students have, for the most part, gone into industry, and I'm not all that proud of it. I would like to have more of my graduates in academia. It's probably me and my philosophy, but those going into industry have gone into some very good positions, and they're doing very well. So, I'm very proud of these graduates. I would like for more of them to be in teaching; I have a couple of them in industry I think eventually will go into teaching.

This is not just me: something is traditional with this department that represents, I think, a problem. If one would pick Berkeley, Wisconsin, MIT, Minnesota, Michigan to some extent, many of our top chemical engineering educators took their Ph.D.s at these schools. I think we could do better if we worked at it and got our students more involved in teaching, and also took their credentials to our friends in academia.

I think it's a problem we need to work on here, because it's not only that some people ought to be in teaching, but it also has a lot to do with ratings and rankings of institutions. I don't know whether you remember, not long ago there was a *U.S. News and World Report* on

programs, including engineering programs. They had two rankings: one by academia—deans mostly—and the other by industry. We ranked number two in the country by industry. They like our graduates; they do well. They like the kind of work we do here. We were tenth by the academicians, which is not all that bad out of a hundred and fifty or more schools. But if we had more people in teaching, I think that ranking would be higher.

I suppose one could say that if these students really liked what they saw around the building—what the faculty do, how they live, how well they like their work—more of them might want to do what we do. It doesn't seem to be working that way. That's something I knew when I came here and something I've been working on to some extent, but we haven't made a lot of progress. Oh, every year we'll send a couple people into academia, somewhere in the world. Our last one, one of our biotech people, went out to the University of Washington in Seattle. But I wish I had more of my students in teaching.

BOHNING: You have been very active, and we mentioned this earlier, in the AIChE, going way back to your early Texas days.

FAIR: Yes.

BOHNING: You've been on many committees and quite active in a number of areas. Are there any particular AIChE areas that have more meaning to you, or are more important to you?

FAIR: There's one in particular. When I was in Dayton, I was asked to serve on a blue ribbon committee of the AIChE called the Dynamic Objectives Committee. Why I was on it, I don't know, but the eminent chemical engineering people were on this committee. That includes [Edwin] Gilliland from MIT, [Thomas H.] Chilton from DuPont, [Donald L.] Katz from Michigan, [Olaf] Hougen from Wisconsin, [Joseph C.] Elgin from Princeton. That, of course, was highly inspirational, just to be sitting around the table with these people and to get to know them. I found myself to be on a three-person committee to write the report. It was a very extensive report published in *CEP* (32). This committee was composed of Katz, Charles [R.] Wilke from Berkeley, and myself. We had a number of meetings to put this report together. It was a good report; it was a report that was read and used, and the experience of meeting with these people was highly motivating. In recent years there was a second Dynamic Objectives Committee, and I did not participate on that one; but that first one was really something. It was a great experience.

Other than that, I would say heading up the Publications Committee, and at another time the Continuing Education Committee, were of great interest. I would say the Continuing Education program has been a big thing with me, and it's been a good program for the AIChE;

we've thought, through the years, a much better program than any of the other founder societies have. Stu Churchill teaches in that program, for example, so we've got some good people in it.

BOHNING: You've given a rather large number, I believe.

FAIR: The course that I teach has been given—it's getting close to a hundred times now—I would guess to around three thousand people. Still going strong. In fact, we've just expanded it from a two-day course to a three-day course. It has been a popular course, but we've had a lot of other good courses, and we've had good management at AIChE of this program. There's a person there you may have run across named Hal [Harold I.] Abramson, and he's good.

We will have three to four thousand people a year taking these AIChE courses. Things are off a little bit this year because of the recession. We've held these courses at meetings and also on an in-house basis for companies. We have the local sections. AIChE has been very aggressive about this; it has been a good program. I would say the Continuing Education activity has been a close second to the Dynamic Objectives Committee activity.

BOHNING: You have off and on mentioned certain consulting activities, and I just want to treat that as a separate area, because you do list a large number of companies that you have consulted with over the years. I was wondering whether you'd like to make some comments about that.

FAIR: Since coming here, of course, I've been permitted to consult. I couldn't do that at Monsanto. I would say that the best of these relationships has been with Air Products. It's a great company; it's a company that values good technical effort and invests in good technical effort. I think that while this was an Air Products tradition, there is an individual there who has had a lot to do with it. His name is [P. L.] Thibaut Brian. He was on the MIT faculty, was recruited by Air Products, is on the board and a senior vice president, but he still likes to involve himself at a technical level. So Thibaut has been able to convince management that an investment, let's say <u>a priori</u>, in checking out the technical detail is a good investment: don't just plunge in.

So the group there is a high-powered group, a very stimulating group. I have not been permitted to consult at Monsanto, so I don't consult with Monsanto. You wouldn't see that on the list, I don't think, because I am a retiree of Monsanto. I've been on retainer at Air Products for a number of years, and I've made many, many visits to Allentown. I've also visited overseas for Air Products; it's a great company. They have a history out that I'm sure is in your collection: *Out of Thin Air* (33).

BOHNING: Right.

FAIR: I also get involved with smaller organizations in actual design. I've designed a number of facilities that have been built and run, and there have been some cases where my design didn't work out quite like it was supposed to. We'll take a trip next month out West on one of these to see what we can do to correct the operation. I still enjoy sitting around a table doing this sort of work.

The week after next I'm going to spend four days in the East, retained to review a process design from start to finish—looking for any possible pitfalls, sort of troubleshooting the design. The company that has this assignment really does not have a lot of strength in chemical engineering and got involved more from a process safety point of view. Now the plant wants a full review by me of what they've done. I don't have any conflicts of interest on it; I don't have any agreements with other firms who deal with this particular chemistry and engineering.

That's still a lot of fun. I have in storage a lot of drawings that I work with on projects of this type, so I like to try to keep my hand in.

BOHNING: You do computer design now?

FAIR: Well, yes and no. I'll tell you why that's a little hard to answer. If I have developed a program more or less on my own time, I can use it. If I have developed it for the University of Texas in some way, I can only use it by some arrangement with the university. That gets to be difficult. So I have a lot of my own programs. Let's take the case of using the ASPEN simulator, or we also have the SimSci simulator or the ChemShare Simulator. Our agreement with these companies is that their software will not be used for consulting, which is quite understandable.

In many cases, I find myself working; say I'm out at some plant and we need to have a heat-exchanger analyzed. Really, the only way is to use a computer program. I will phone up a friend and say, "Here, these people want to use your program and we'll pay for it." So they work it that way. That's an interesting question. I would certainly not do by hand a lot of these things, but, for example, I've bought software to do distillation systems that are my property, adsorption systems, extraction systems, so I could do that. If it was developed by university funds, then there are restrictions.

We're right into that now in our expert systems work. They're the property of the university because they were developed here with university facilities and people. It's kind of an interesting area, but if I'm going to design a distillation column for some client, I'll use one

of my own programs. It's remarkable what you can get for something like a PSII or 386 or 486, that used to require a large mainframe system.

BOHNING: I'm pretty much at the end of my questions, but I did want to ask you just a little bit about your family. I know you have three children, and you've already told me when you were married—in 1950, I believe.

FAIR: Yes. We've celebrated our forty-second anniversary. My wife's maiden name was Merle Innis, and our first child came in 1951.

BOHNING: That's James Rutherford III.

FAIR: Yes. He was quite a tremendous student, and he went to Rice University to study architecture. As part of his program there he spent a year in The Netherlands studying under an architect. He spent seven years; he got the three degrees: the Bachelor of Science, the Bachelor of Architecture, and the Master of Architecture. He's with a company that now, he says, is the largest architectural firm in the country, and he's a senior vice president. He's quite prominent in his firm, so he's done well. They have two children.

Number two child is a daughter, Elizabeth, who graduated from the University of Indiana as a journalist, worked for a couple of newspapers for several years as a reporter, went back for an MBA degree and went into marketing work, first for Coca-Cola and then with Quaker Oats. She is now a homemaker with a child. They lost their home in the Oakland fire, but they were in the process of relocating to the Chicago area. They're there temporarily now; they've gotten their insurance settlement and are starting to look for a house.

The third child, Richard, came eight years after the second child. He got a bachelor's degree from Trinity University in San Antonio and tried the banking business, but at a bad time. He went back for an MBA and got interested in health care. He is now with a company named Manor Care—stock is on the big board. They have retirement homes all over the country; they also have hotels all over the county. They have Quality Inns, several different chains of motels and hotels, so they're in that kind of business. But this young man seems to be dedicated to working with elderly people, and if you have had experience with nursing homes or rest homes, you know that it takes a certain type of individual to put up with that. I would never do it. He's very good at it; they all love him.

He is a management trainee and will be assigned to run a home within the next few months. His company is headquartered in the Washington, D.C. area, in Silver Springs, Maryland. It's pretty big. You might say that's a growing field. It's bound to expand. They

have the problem of working with Medicare, which does not provide all of the money needed for the prestigious homes, but in many cases there is a supplement from savings in a family. That's the interesting one.

We have three children with master's degrees, and we have an architect, a marketing person—and she'll go back to work—and a health care person. It's kind of interesting.

BOHNING: No engineers, no scientists. That's not unusual.

FAIR: I guess it really isn't.

BOHNING: I've noticed this with a number of people I've talked to, and in my own family, the same thing.

FAIR: Well, you know, they won't come out and admit it, but I'm not sure that they liked what they saw. [laughter] Their father was away a good bit, came in late many evenings.

BOHNING: As you said way back at the beginning, when you were selecting where your life was going to go, chemical engineering was the glamorous area and it was a place to get a job.

FAIR: Yes.

BOHNING: Perhaps more important than anything else.

FAIR: I think that influenced many people; I'm sure it did me. I'd have wanted to be a railroad person. [laughter] Chemical engineering was a good field; it was an interesting field, but as you think back—I expect this happened to you—I could probably have been happy in law or in other fields. Teaching history. [laughter] But it's been a good one.

I would say, Jim, that in my view, I've been involved in chemical engineering at a very good time. We had the surge at the end of the thirties and then at the end of the war. We had that exciting time after the war when the petrochemical industry was really taking off, and that was an exciting time. It has simply been a good time to be involved. You know, I talk about railroads; I've said, "Well, I'm sure I was born ten years too late, because I could have had ten more years of riding behind steam locomotives." [laughter] But I still got to see a lot of steam

locomotives and still practice chemical engineering; and if I may say so, I think I've demonstrated, at least to myself, that you can work in engineering, you can work on something that's fun to work on, and you can still make some money.

BOHNING: I think on that note, we'll stop. That's a good place to stop. I want to thank you very, very much for spending the day with me.

FAIR: Oh, this has been quite an experience for me. I hope that you can make some sense out of the recording.

BOHNING: Oh, I don't think that will be any problem.

[END OF TAPE, SIDE 9]

[END OF INTERVIEW]

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