THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

MALCOLM M. RENFREW

Transcript of an Interview
Conducted by

James J. Bohning

at

New Orleans

on

31 August 1987

(With Subsequent Corrections and Additions)

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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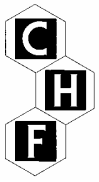

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

MALCOLM M. RENFREW

1910 Born in Spokane, Washington on 12 October

Education

1932 B.S., chemistry, University of Idaho
1934 M.S., chemistry, University of Idaho
1938 Ph.D., chemistry, University of Minnesota

Professional Experience

University of Idaho, Moscow, Idaho
1932-1933 Fellow in Physics
1933-1935 Teaching assistant in Chemistry

University of Minnesota, Minneapolis, Minnesota
1935-1937 Teaching Assistant in Chemistry
1935-1938 du Pont Fellow

E. I. du Pont de Nemours & Co., Inc., Plastics Department, Arlington, N. J.
1938-1944 Research Chemist
1946-1946 Supervisor of Process Development
1946-1948 Supervisor of Product Development

General Mills, Inc., Minneapolis, Minnesota
1949-1952 Head of Chemical Research Department
1952-1953 Director of Chemical Research
1953-1954 Director of Chemical Research and Development

Spencer Kellogg and Sons, Inc., Buffalo, New York
1954-1958 Director of Research and Development

University of Idaho, Moscow, Idaho
1959-1967 Head of Physical Science Department
1968-1973 Head of Chemistry Department
1959-1976 Professor of Chemistry
1976- Professor Emeritus
1977-1978 Executive Vice President, Idaho Research Foundation, Inc
1978-1986 Patent Director, Idaho Research Foundation, Inc.

Honors

1976 Honorary D.Sc. University of Idaho
1976 Norris Award, Northeastern Section, American Chemical Society
1977 University of Idaho Alumni Hall of Fame
1977 College Chemistry Teacher Award, Manufacturing Chemists Association

1977 University of Minnesota Alumni Achievement Award
1985 Division of Chemical Health and Safety Award, American Chemical Society
1986 Mosher Award, Santa Clara Valley Section, American Chemical Society

ABSTRACT

Malcolm Renfrew grew up in the northwest. Despite an early interest in music, drama, and the arts, Renfrew studied chemistry at the University of Idaho, in part influenced by a chemist uncle. After serving as a teaching assistant in both physics and chemistry and completing a Masters thesis, he joined George Glockler at Minnesota for research on Raman spectroscopy. He recalls contemporaries at both Moscow and Minneapolis as well as a summer spent on the road with a tent show. When Renfrew joined the Arlington laboratories of Du Pont he was much involved with plastics development, especially of Teflon and he recalls the enthusiastic interest aroused by the disclosure of its properties at an ACS meeting in 1946. Malcolm Renfrew has long had a special interest in health and safety in the chemical environment and he recounts laboratory accidents during the development of PTFE. After moving to General Mills and then to Spencer Kellogg, ascending the research management ladder, Renfrew went back to his alma mater in 1959 as head of physical science. He completes the interview with an account of his return to teaching.

INTERVIEWER

James J. Bohning holds the B.S., M.S., and Ph.D. degrees in chemistry, and has been a member of the chemistry faculty at Wilkes College since 1959. He was a chair of the Chemistry Department for sixteen years, and was appointed chair of the Department of Earth and Environmental Sciences since 1985, and was elected Chair of the Division of the History of Chemistry of the American Chemical Society for 1987.

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INTERVIEWEE: Malcolm M. Renfrew

INTERVIEWER: James J. Bohning

LOCATION: New Orleans, Louisiana

DATE: 31 August 1987

BOHNING: Dr. Renfrew, you were born on the twelfth of October 1910 in Spokane, Washington. Can you tell me something about your family background, your parents?

RENFREW: My parents were young parents. My father was a bookkeeper, who at that time was working in a bank. My mother was a very delightful woman. At ninety-eight, she has decided she's lived long enough, that she can't go on indefinitely and she'd like to get it over with. When I left her to come to this meeting she was still alert but not doing well. In any case; my parents were young and made some of the mistakes young parents make on me, and didn't correct all of them when my brother came along four and one half years later. I have a chemist brother, Dr. Edgar E. Renfrew, who also received a Ph.D. in chemistry from the University of Minnesota, and the two of us have had close relationships. He will be coming out from Lock Haven, Pennsylvania, where he is now a retired research director, and will be seeing our mother next week. [Mrs. Renfrew died while she was with the family in Moscow, Idaho.]
BOHNING: I see. What did your father do?

RENFREW: Well, he worked in banks as a teller and bookkeeper. He became an accountant later on, mostly in the Inland Empire area around Spokane; he was an accountant with the Potlatch Lumber Co. in my high school years. Later on, he became ill, which was at just about the time I started college, and, during Herbert Hoover's presidency, my mother became a postmaster in Potlatch, Idaho, where we were living. Then during the war years, my father was a traveling accountant for the Weyerhaeuser people, who had originally operated the Potlatch Lumber Company. My father ended his career working in the Tax Department for the State of Washington.

BOHNING: Where did you grow up, in Washington or in Idaho?

RENFREW: Well, both. We moved around in the small towns near

Spokane; I graduated from high school in Potlatch, Idaho.

BOHNING: Can you recall anything about your early schooling, grade school and leading into high school. Any teachers or anyone who had any particular influence on you?

RENFREW: Yes. Most of my teachers influenced me. In Potlatch, we had extraordinarily good teachers. Potlatch was a company town that was controlled by a man named A. W. Laird, General Manager for the Potlatch Lumber Company, who was a laird in practice as well as in name. He ran a very proper small town in which the school teachers were better paid than elsewhere in Idaho. They had to toe the morally straight line (especially women teachers), but were hired because of high competence. And I liked essentially all of my teachers. One later became the author of the Lippincott series of science texts. A math teacher later gained a Ph.D and became an astronomer.

BOHNING: What were your early interests, did you have anything specifically that stood out among the other things that interested you?

RENFREW: My natural interests were in the arts and in music. I became a trombone player at an early age and have continued this. I'm now a member of the Hog Heaven Seven, which tries to play Dixieland, of the Vandal Boosters Nonmarching Pep Band that fills in at University games and rallies, and also of the Pullman City Band. Also, one of my water colors was used in promoting our University Centennial celebration. If your tape will stand it, I can tell you how I became converted to an interest in chemistry.

I became a chemist primarily because I had an uncle who was a chemist. His name was F. Bradley MacKenzie. He at one time was the research director and later the quality control head for the Carnation Company. He spent his whole professional life at Carnation. I became a chemist because Bradley was a chemist. The story that I'd like to tell you is why he became a chemist. According to Bradley he had been working on a threshing crew in the wheat fields around Pine City, Washington, just before going to college. By the time the harvest was over, he decided that he didn't want anything to do with farming in his future life. When he arrived at Washington State Agricultural College, it's now Washington State University, the students were lined up for registration in a large gymnasium area with signs on tables where students could go to select a major. The first table was marked 'agriculture', and he knew he didn't want that. The next one was 'botany', and he was afraid that was some kind of agriculture. The next table was 'chemistry'. He didn't know what chemistry was, but he felt he couldn't go on shilly-shallying around. He ought to make up his mind. So he went over and stood in that line! His success prompted me to become a chemist. And that's how my brother

became a chemist, too, although his B.S. degree from the University of Idaho was in physics.

BOHNING: Did he talk to you about chemistry? When did you have your first course in chemistry, in high school?

RENFREW: No, I had my first course in college. I was better prepared than many students. We had an excellent physics teacher in high school, a good biology teacher and we also had a truly superior math teacher; I took all the math that was offered through solid geometry. I arrived at college with biological science and physics in my background but no chemistry.

BOHNING: But did Bradley talk to you about chemistry, had you seen any of his work?

RENFREW: I suppose somewhat, although I wasn't... He never particularly tried to influence me, but I was a practical sort. Although my interest really would have been in becoming a newspaper man, say, an editor or a writer, I knew that small town newspaper people didn't do very well financially. Bradley was considered to be quite prosperous by young people at the time. So, with a practical bent, I thought I'll become a chemist. I never really regretted the choice, although I didn't start working hard at chemistry until I was approaching the end of my college undergraduate years. I would probably have been better prepared all through my later professional life, if I'd taken fewer English courses and more math courses in college. If your tape permits these kinds of recollections I will continue my college commentary.

BOHNING: Sure. Go ahead.

RENFREW: At the time I entered the University of Idaho we had a college president who held that the chief emphasis in education should not be on teaching but on learning. He was determined to give a real education to young people. He was convinced that students who came to school and had teachers who thought of grades as very important would suffer as far as getting a [genuine] education was concerned. He also recognized that as students coming from small towns in the far west we had enjoyed few cultural advantages. He wanted to remedy these weaknesses by bringing in outside artists and musicians and outstanding thinkers of the period. He wanted us to read good books that had nothing to do with our classes. I tried to read one good book each week outside of courses. One student who was trying to read one book a day was in my freshman chemistry lab. I recall that he was having a bad time completing War and Peace in an evening laboratory session while conducting experiments. His grades weren't outstanding,

but he later became a foreign language professor at a major university.

This president had come to Potlatch, while I was a high school senior, and had talked about his plans; how he wanted us to feel that, when we came to the university, if there was something important that really interested us, we should cultivate that interest, instead of preparing for the next day's examination. And, as a college undergraduate, I took him quite seriously! But the faculty at the university was less enthusiastic. He had early on said that he wanted our young people to have contacts only with the best teachers in the university. He wasn't going to fire any of the others, but he just wanted the best teachers to be the ones students were exposed to. Naturally, not all members of the faculty were sure they were going to be in the best-teacher category. So he encountered resistance almost from the first day he arrived on campus with the consequence that he lasted only two years. He went on to become the head of the Office of Higher Education in Washington, D.C. His name was Frederick J. Kelly. He had a large influence on my life.

But in any case, I started trying to become more serious about my math and science courses later on. I wound up with a B average instead of a higher average. But I hadn't really felt grades were too important. (For example, I accepted without protest my C grades in scientific German: the rather eccentric instructor said that I did B work but should have done A work so I was given the C grade.) Actually the extra creative writing courses I took in early college years turned out to be professional assets. Thanks to them I had an easier time writing technical reports than did many of my associates. My reports required minimum rewriting by my supervisors, who were properly grateful.

BOHNING: Your selection of the University of Idaho; was that a foregone conclusion? Or were there any other possibilities?

RENFREW: It was essentially a foregone conclusion. I lived twenty miles from the University of Idaho. Also, I lived twenty miles from Washington State University where my favorite uncle had gone, but there were no tuition problems if I went to Idaho. And I had respect for the institution.

BOHNING: Can you tell me something about your first chemistry course at Idaho? That was the first formal time that you'd been exposed to chemistry.

RENFREW: We had as the freshman chemistry professor the head of chemistry. His name was Dr. Carl Leopold von Ende; he was a native of Iowa, but he had received his Ph.D. in Germany. He was a man of

high moral standards, rather rigid standards. He was a colorful lecturer who really made us do things strictly the way he felt they should be done. Basically I think he always had a weakness in math and he recognized his own weakness, and so he was determined that all of us chemistry majors at the University of Idaho would remedy what he found deficient in his own approach to science. All of our problems in freshman chemistry had to be worked with logarithms. Because of this some of the students, particularly home economics majors and nurses, could never pass the course because they couldn't handle logarithms. We had very rigid rules in the laboratory. Every entry had to be recorded in ink in the bound notebook. Anybody caught writing on a scrap of paper suffered the loss of the data. Our bound notebooks in which we took the notes on lectures were collected at the end of every week, and the instructor in our quiz sections had to review these to make sure that we were getting the material recorded properly. Then we were given weekly quizzes on the lecture material. Our quantitative experiments in the laboratory had to be done very precisely with analytical balances. Now and then students who weren't doing well would copy data from a fraternity brother's notebook of several years past. But the instructors had kept detailed records of all the vital elements of the quantitative experiments: the weights of precipitates and the dishes and so on. Every now and then they would find some poor fellow that they were convinced had not really done the experiment. They would then go back and dig out the records for past years and find the identical data in the big book! Anyway, in my freshman year we learned that chemistry was a precise science.

BOHNING: What kind of laboratory experiments did you do?

RENFREW: Oh, we prepared oxygen and determined the weight of a liter of oxygen at STP, and we did the combining weight of lead, etc. They were good, honest experiments for the time, and the quantitative experiments had to be done properly.

BOHNING: What kind of facilities did Idaho have for chemistry?

RENFREW: We had good facilities. Nowadays when I'm conscious of defective fume hoods, I wonder how good ours really were. I know that we never had instructions on how to use a hood properly. No one knew how in those times, of course. But we had fume hoods in the laboratory, and we did our weighing on good analytical balances. They were German analytical balances. The laboratories were well-kept and, at the time I entered them, were quite new. Again, since I have become conscious of safety problems with hoods, I remember how we had the hydrogen sulfide supply lines almost out the front of the hoods and how students used to stand there bubbling hydrogen sulfide into the qualitative analysis test tubes. I have wondered since why we didn't kill some

students. But nobody ever complained to my knowledge; we thought that was the way you did chemistry. (Nowadays, Jay Young says we're going back to H2S because we will be sued if we use thioacetamide, which is an anticipated carcinogen.)

BOHNING: I haven't heard Jay say that. But I...

RENFREW: That's what he said. We had a row about it in a meeting today concerned with the revision of safety in academic chemistry laboratories.

BOHNING: How many faculty were there in the chemistry department?

RENFREW: I suppose we had seven or eight. I'd have to name them to be sure, but I think we had eight members.

BOHNING: Were there a large number of chemistry majors?

RENFREW: Perhaps ten or twelve each year. Actually, chemistry and chemical engineering at that time were combined. I'm a little fuzzy in my memory as to who were chemical engineers and who were chemists. We shared many courses. At one time, for the period in which I was enrolled as a graduate student, ACS ran analyses of the institutions of the country that had turned out the most Ph.D.s per capita based on the undergraduate program, and Idaho ranked second. As I recall Oberlin was first.

BOHNING: Who was chairman of the department?

RENFREW: By this time Dr. von Ende had died, and I think one of the reasons our average went up was that Dr. Louis C. Cady took over as chairman. He had been an Idaho student who had stayed on for a Master's degree, then joined the faculty, and later went to Wisconsin for a Ph.D. While he was at Wisconsin he found out what went on in the world, and when he came back he did a wonderful job in organizing our students to get them into graduate school. There were no jobs available in those Depression years. He had each one of us select four or five schools, eliminating overlap, that we would be interested in attending on a stipend. Then he would write letters promoting our candidacy at the selected schools. We had quite a high percentage of our chemistry majors going on to graduate work.

BOHNING: I see. You mentioned an interesting point. The

Depression occurred, I guess, in your freshman year. Is that right?

RENFREW: Well, yes. I entered in the fall of 1928. The Depression became a real problem to many people over the next decade.

BOHNING: Did that affect you in terms of staying in school?

RENFREW: No. My parents were determined that my brother and I would get college educations so there had been savings accounts set up for us at an early age. When I graduated with my first college degree, I succeeded in getting assistantships, first in physics and then in chemistry. They paid around $400; I was supposed to get $500 a year on the first one, but it was reduced to $425 because of the Depression. But I could live quite well on $425 for ten months in Moscow [Idaho]. The way I got the physics assistantship was unusual. There were no assistantships in chemistry at the time, but the physics department had one assistantship. It had been offered to a student from the University of Kansas. But the Idaho legislature passed a law that no state jobs could be given to anyone who was not already a resident of the state of Idaho, and the appointment was withdrawn. Well, there was no physics major who was qualified to be a teaching assistant, and so the head of physics gave me the appointment. I learned my physics while trying to teach it. I'd had trouble in engineering physics myself, and suddenly I found myself leading problem solving sections in engineering physics! Then at the end of the year, Dr. von Ende felt that this teaching assistant business was the way to lower the costs and get more help, and so he started hiring teaching assistants. I spent two years as a teaching assistant in chemistry.

BOHNING: What did you take next after your freshman year?

RENFREW: We had a sophomore inorganic course at the time that went deeper into inorganic chemistry.

BOHNING: Do you remember any texts you may have used?

RENFREW: The sophomore text was by a man named [William H.] Chapin (1). (There's an NSF man of that name who comes to meetings. I've asked him if he's related to the author of that text and he says there's a remote connection.) The text that we had bought for our freshman course was by Harry Holmes, who had been a young Oberlin instructor (2), but we didn't use it very much. Dr. von Ende's lectures were the things that really counted.

BOHNING: As you proceeded through those last years, was there any area of chemistry that drew your interest more than the others?

RENFREW: No. Our courses were not the equivalent of modern chemistry courses at a good university. Dr. von Ende taught the physical chemistry course. He again did it by lecturing from detailed notes. Often this involved his copying down on the black board the analytical data from the original literature in which, for example, people had found that atomic lead from different sources had different atomic weights. We would record the data. When we got through with that set of lectures, my conclusion was that, based on my experience in analytical chemistry, I wasn't sure they really had found a difference. But Dr. von Ende was greatly impressed by the precise atomic weight methods of Theodore William Richards. We had bought the Getman and Daniels text in physical chemistry (3). It was the best seller of the day. But we had no reading assignments and we were not asked to work the problems. The organic course was taught by Dr. J. A. Kostalek, who had had industrial experience, was a very able lecturer, and had all sorts of interesting anecdotes. But his version of organic chemistry involved taking a particular organic group and lecturing a couple of weeks on that; we then had a "final" exam. We would then put that group out of the way, and take up the next one, forgetting about those past. We did have as a reference the Norris text (4), that did add interest and authority. When, later on in life, I won the American Chemical Society Northeastern Section's Norris Award for Teaching I felt there was poetic justice in my selection (as an early student of the founder).

BOHNING: Did you do any research as an undergraduate?

RENFREW: As an undergraduate senior, we had to do a research project for two credits over the year. It wouldn't qualify as a research project by present standards. Dr. von Ende was my advisor. He suggested several problems at the beginning of the year, and at the end I brought my thesis in. It had to meet the literary standards of a thesis, i.e., properly typed and bound. I had worked on "stick antimony electrodes" without any sensational results. It is unlikely that anyone ever read the thesis, but I benefited from writing it.

BOHNING: Now, did he assign you as an advisee or did you select him.

RENFREW: Oh, we had some element of selection from among the faculty, but no one could be overloaded. Rarely did anything

publishable result. We also had to do a long term paper in which we went to the literature and really read it. In many ways it was a fine education, headed by a man who recognized deficiencies in his own chemical training and who wanted us to be better prepared than he was. For example, chemistry majors were required to take engineering physics followed by a year of analytical mechanics. And so I've always been sympathetic to the people who aren't Nobel Prize winners but who, in teaching chemistry, are really doing the right thing by students.

BOHNING: As you were approaching the end of those four years as an undergraduate, and the times certainly hadn't improved much since 1929, had you given any thought as to what you were going to do when you finished your undergraduate degree?

RENFREW: Well, as I say, there was essentially no place to go for a job. I fell into the physics assistantship and then had two years in chemistry on teaching assistantships but then I really had to do something. It was Cady who pushed us into graduate schools, and I received the appointment at Minnesota. I also received an offer from Stanford, but the one from Minnesota came in first, and I had accepted. Actually, I had an alternative consideration. I'd taken a few education courses along with my Master's program, and I had toyed with the thought of teaching in high school. But the only two jobs that I really could have had (they were sort of thrust upon me) was one in Fairbanks, Alaska, where I would have to be the coach for the tumbling team, and I didn't think I could do that; the other was in Kimberly, Idaho, but I would have to get married before I went there. So I went to Minnesota. The teaching assistantship at Minnesota paid three hundred dollars a year. It was called half-time, but was really a quarter-time, teaching assistantship. Minnesota had split their assistantships to spread the opportunities for students. So I went to Minnesota with that appointment, although Stanford's offer was much better financially. Stanford was not then the prestige institution it later became.

BOHNING: That's less than what you were getting in Idaho.

RENFREW: I was supposed to do only half as much work as I did at Idaho. But when I got there, it turned out that Minnesota had an unusually large enrollment of undergraduates that Fall, and they had to have extra teaching assistants. So for those of us who had these three hundred dollar appointments, our working time was doubled and our pay was increased to $450. So some teaching assistants got $600 and some of us got $450 for the same loads.

BOHNING: I just want to come back for a moment to your Master's degree. You had a paper on nickel sulfide(5).

RENFREW: Yes, yes.

BOHNING: Is that part of your Master's thesis?

RENFREW: Yes. That was my Master's thesis. I've been trying to get Jean'ne Shreeve to look at the current status of nickel sulfide. I feel that we could expand that paper. Another fellow who was a Master's candidate and I participated in the original paper with Dr. W. H. Cone. Cone had a theory about "the nickel sulfide anomaly." You know, you can't precipitate nickel sulfide in acidic solution, but once you get it precipitated, it won't redissolve in the acid. At least it dissolves very slowly. Cone was convinced that adsorbed sulfide ions on the precipitate were there in sufficient quantity to maintain concentrations exceeding the solubility product constant. We did quite a few ingenious experiments to prove his theory, but the referees would not tolerate this explanation when the paper was submitted. They felt we hadn't measured the hydrosulfide ion content, there was no mention of activity coefficients, and so on. They wouldn't accept it as written. To my knowledge the historic explanation for the nickel sulfide insolubility still "holds", that is, the formation of isomeric forms of differing solubility. I haven't really gone back to it in recent years. Since I returned to Idaho, I have thought that I could do a little more work, take some other data that Cone had collected, and pull all this together as another paper. You've reminded me. I'm going to go back and ask Jean'ne if she's done anything with this. I told her that I didn't feel that I was up to modern inorganic chemistry, but if I had an associate we might make a publishable paper.

BOHNING: Now, why did you stay on for the Master's degree at Idaho?

RENFREW: There really wasn't anything else to do.

BOHNING: And Cady didn't work on moving students into other graduate schools until later, or was he already doing that when you went into the Master's program?

RENFREW: No. He did that after I'd started in the Master's program.

BOHNING: Okay.

RENFREW: Dr. von Ende died early in my last year at Idaho, and

Cady then became department head.

BOHNING: How many other students that graduated with you from Idaho went on to other graduate schools? Do you remember some of the other graduate schools they went to?

RENFREW: One of the fellows that I had roomed with went to Northwestern. One went to the University of Illinois, and one had gone to Columbia. There were four or five of us who went on to graduate school at that time; about half of us eligibles went on.

[END OF TAPE, SIDE 1]

One of them who left to go to the University of Cincinnati stayed there just a year, then went to the National Bureau of Standards (or the Patent Office) in Washington, and later became a patent attorney. When I returned to the University of Idaho I was looking up our alumni, trying to get in touch with them. I couldn't find this man. They'd lost all record of him, and didn't know what had happened. But just this last year, his widow gave $50,000 to chemistry and chemical engineering for a scholarship fund in his honor. It turned out he'd become quite a successful patent attorney, had spent a lot of time in government service, maintained his sense of identification with our university even though we had lost contact with him.

BOHNING: Those who didn't go on to graduate school, did they hope to find jobs?

RENFREW: Jobs of sorts. I'm not conscious of any of them who did a lot with chemistry. One of them became, I recall, a sort of a salesman with General Mills, primarily a flour salesman. One of them worked in the General Mills control laboratories. I can't remember. Jobs in chemistry, like any kind of job, were extraordinarily hard to find in those years.

BOHNING: Well, how did you feel? Had you done any traveling before you went to Minnesota or was that your first time leaving Idaho ?

RENFREW: That was my first real departure from Idaho although I'd had an experience in traveling during that summer which will clutter up your tape.

BOHNING: Oh, no.

RENFREW: I'd spent the summer with a tent show of traveling players. I'd had early ambitions as an actor and I'd appeared in several college plays. One of the dying repertory groups came through Moscow that spring. I talked to them just before deciding to go to Minnesota. It was agreed that I would join the show after Commencement. I doubled in brass, playing the trombone in the orchestra, and did various parts in the plays. I played juveniles, heavies, and "G-strings" (i.e., squeaky-voiced oldsters), and I sold prize-containing candy boxes between acts. The boxes were marked so we could sell those with flashy prizes first in order to encourage continuing sales. (Sometimes I gave out a good prize "late" to a little old lady or her pretty daughter. The concession owner would grumble about my poor salesmanship.) We traveled around the Northwest, spending a week in each small town with a different play each night.

The R. Ferris Taylor players were among the last of the "Toby shows." That is, a couple of our plays featured "Toby", a country boy who outwitted city slickers. But we also played Broadway shows -- with a change in name to avoid royalty payments. Ferris Taylor was a brother of Glen Taylor who became a United States Senator from Idaho and later on ran for Vice President with Henry Wallace. They were the children of an itinerant evangelist, I believe, and they'd all grown up playing musical instruments, singing, and so on. One of their sisters was Lee Morse, who made her name on Broadway appearing in musical comedies and had the Blue Grass Serenaders as a band that accompanied her in tours and in recordings. The group that I was with were professional actors. Ferris later had minor success in Hollywood and in television. He was convinced when I joined them that, having had a touch of show business, even though there was no business, I'd never get out of it. He was holding until the time I left that I'd never get away. They were planning to wind up the summer and fall in Hollywood with their tent. However, we were burned down in Yakima, Washington, at a time of union trouble, which helped my decision to leave! We were non-union. We kept going in Yakima for a couple of weeks in an idle movie house while they located another tent. But I gave up when they left on a dark night heading toward California.

BOHNING: Were you torn at all between going on in chemistry and pursuing your interest in the arts?

RENFREW: I knew the arts weren't practical. When I was with Ferris Taylor's show, I was supposed to get fifty dollars a week, an excellent wage for that period, but I collected only seven dollars a week, on a good week, eight dollars. As a bachelor I lived in the tent, of course, and we could get seven-course dinners for thirty-five cents in many towns in those days. I also picked up some extra money going house to house to make appointments for family photographs. One of our actors also was a professional photographer. He gave me 50 cents for each appointment that I scheduled. I could live all right, but I

wasn't making my fortune.

BOHNING: What did you find when you got to Minnesota? What was it like?

RENFREW: Well, it was a heady experience. I still have great respect for the University of Minnesota, and had great admiration for my major professor there, George Glockler. He later became head of chemistry at the University of Iowa. He was another German, but he was a more flexible German than Dr. von Ende had been. And I had really a good experience there. I was a little older, and I'd had some advanced training. Of course, I already had been a teaching assistant for three years, including that unusual year in physics. Although I never claimed credit in my record for taking advanced physics courses, it really gave me an edge as a physical chemistry major at Minnesota. But then, I took organic chemistry as a minor, which was uncommon for physical chemistry majors. Most chose physics as a minor, but I felt organic chemistry would be more practical. So I chose the combination of physical and organic, which turned out to be a very happy choice. I had good relationships in the organic department and good relationships in physical chemistry. Although I didn't do everything right, I succeeded in winning the Du Pont Fellowship. There were only two fellowships at Minnesota at the time that would provide income for study without having a teaching obligation. One of them paid $500 a year; Fred [Frederick T.] Wall, another Glockler student, had that. I got the Du Pont Fellowship which was $750. You could live like a king on $750 at that time. I didn't have to pay fees or tuition. Again, well I don't want to get you too far off the track.

BOHNING: Oh, no, fine. Go ahead.

RENFREW: Well, one of the things that showed how casually teaching was taken in those times is this. In the second year at Minnesota, when I was a teaching assistant in the freshman program, the professor who taught my assigned course called me in and said that I was to be in charge of the laboratories. He said, "Here's the lecture textbook," and he also gave me a laboratory textbook: I think that it was the Demming lab book (6). For lectures he was using a Minnesota textbook of which he was the co-author. I tried to follow him by assigning experiments that had something to do with the lectures, in so far as I could keep track of them. But I never pre-tested the experiments. I'd never seen them before. I would choose experiments, making sure that the stockroom had the materials that were required. We had eight sections in this course.

Everything went along really quite well until it came to the preparation and properties of chlorine. I'd noticed that in our laboratories, there were little canopy hoods around on the bench

tops, and I thought, "Well, isn't that nice, there will be no problem with ventilation". Unfortunately those hoods had never been hooked up to an exhaust system, as I heard later on. On the Monday that we started with the preparation and properties of chlorine, midway through the morning, one of the teaching assistants appeared, wild-eyed, down at my door in the basement of the chemistry building. He said, "We've got to do something. The kids are hanging out the windows coughing. And there's chlorine all over!" Well, before I could get upstairs, Dr. Lillian Cohen, who was a professor of inorganic chemistry and a dominant female type, took command. She had an anathema for chlorine; reportedly a relative had been killed with chlorine gas in World War I. She had always made sure that Minnesota didn't do anything with chlorine in the freshman courses. When she came into the building that morning and smelled chlorine, she came roaring upstairs and cleared the lab, got the students out of there. By the time I arrived, she was all ready for me. She gave me my first real safety lecture. It didn't really make as much of an impression on me as a Du Pont explosion did later on, but it was a genuine safety lecture, and I'd never had one before.

BOHNING: Here is a question because of your present interest in chemical safety. As you went through Idaho and Minnesota, and you just said you'd never had a safety lecture before, were there any comments or any indications on the part of the faculty about safety? Safety glasses or use of the hood?

RENFREW: Oh, no. We had no safety glasses. Actually, I suppose we might have been warned at times about such things, but I was never really conscious of a safety lecture. Cady, the man that I spoke of who had helped us into graduate school, had only one eye because of an explosion in a lecture demonstration he'd been doing with von Ende in the freshman course. I think they were demonstrating the interface disappearing at the critical temperature in a sealed tube. As I recall the story, the sealed tube had blown up during the lecture demonstration and put his eye out. Nonetheless, there wasn't much done about academic laboratory safety in those years.

BOHNING: You said these Minnesota hoods hadn't been hooked up. Did they have other hoods in the labs that were?

RENFREW: I don't think they were using hoods in the freshman courses. I don't recall that any of the Minnesota freshman laboratories had really functioning hoods in them. There were hoods in the organic research laboratories, of course, but we didn't have eye protection there either. There was no real safety program at the University of Minnesota at that time to my best knowledge - or at the University of Idaho.

BOHNING: How did you select your major problem for your Ph.D. thesis; there were a number of papers on Raman spectra(7).

RENFREW: The summer before I went to Minnesota, a brilliant chap named H. P. Robertson, who was married to the sister of Otto Turinsky, a chemistry instructor at Idaho, was there translating Weyl's book on group theory (8). Robertson was a mathematician of distinction who later was at the Princeton Institute with Einstein and then went on to Caltech. I think that he was head of mathematics there when he was killed in an automobile accident. Robertson had been a student at the University of Washington when George Glockler was there as a student, and he talked to me about Glockler. By that time I'd decided that I wanted to be a physical chemist, and so I went to Minnesota prepared to like George Glockler, which I did when I met him. We had to go around and talk to all the faculty members in our chosen discipline, and we also had to visit the heads of each department; Lee I. Smith in organic, [Izaak M.] Kolthoff in analytical, and so on. I'd expected to find Kolthoff already a hundred years old way back then, based on his accomplishments, but he really was quite young; he is still active. We also had to visit the head of the chemical engineering department, Charlie Mann, when we went around on our opening tour. Chemical engineering and chemistry at Minnesota were combined at that time. When I met the head of chemical engineering I said, "Well, you won't want to be spending time with me. I won't be doing anything in engineering." He said, "Oh, sit down. I just want to find out what they're teaching chemists these days." So, he began asking me embarrassing questions, questions which in later life I felt I should ask my students. He asked me if I knew the price of sulfuric acid. (I didn't). Then, "Here's a spill where we have to do a neutralization reaction, what's the cheapest way to do it," and so on. Questions on chemical economics had never occurred to me before. This made an impression then, and I hope that such questions in later years at Idaho made our students conscious of costs as an important part of professional chemistry.

This reminds me that at Idaho we ran a "professional seminar" for senior chemistry majors, a one-semester, one-credit course that discussed how to obtain a job and how to hold one plus more. As texts at various times we used such books as Chemistry in the Economy (9); Davies and McCarthy: Introduction to Technological Economies (10); Bradbury and Dutton, Chemical Industry: Social and Economic Aspects (11); and Billmeyer and Kelley, Entering Industry: A Guide for Young Professionals (12). Our students also were encouraged to read Chemical and Engineering News regularly. They were expected to know what chemicals were produced in the largest quantities, their price levels, and the major chemical companies. We also gave some instruction on the patent system.

I made good use of a paper by W. J. King of General Electric

(13), that had been distributed at Du Pont's Arlington Laboratory. Billmeyer, who also worked at Arlington in my time, evidently made use of this in the book cited above. This emphasized that most failures of technical personnel resulted not from inadequate technical proficiencies but from failures in personal relationships with bosses and fellow workers. King's paper had the title "What Every Beginner Needs to Know at Once." I urged our students to save the copies given to them and reread it when they started their first job.

A key part of the course required students to prepare a 20- to 30-minute talk on a technical subject with anonymous critical comments from class members. After television cameras became available we put the talks on tape, and I went over the criticisms with the speaker as we reviewed the tape. Harold Wittcoff of General Mills and I later outlined a book to combine this material with an economic discussion of the chemical industry. Publishers received favorable comments from reviewers, but they held that there weren't enough courses of this type offered by universities to insure adopting at the level required for profitability. Wiley finally began negotiations with us but then discovered that they had a contract with Fred Billmeyer to do such a book. Our interest goaded Fred into action. We were told by other publishers that if the Billmeyer book sold well, they then would be interested in ours. But Wiley never promoted the book. Our university was among the few users of it.

BOHNING: In your Ph.D. research; was it a new apparatus that you constructed to make the measurements?

RENFREW: No. It essentially had been built. Actually, Fred Wall had done his thesis on it. Fred did a much more high powered problem than I did. Mine involved some improvements in the apparatus. Raman spectroscopy was new in this country, and it took us a long time to get the spectra. Neon/mercury lamps were used for illumination. One of my studies involved liquid acetylene; I've shuddered a little bit in later life about what I did with that. I wanted to make two runs with pure liquid acetylene that I had manufactured, and I had to keep it liquefied by pouring liquid air into a reservoir which chilled the air drawn into the apparatus. I was thirty-six hours in the laboratory making those runs with liquid acetylene. Friends brought me hamburgers from the White Castle. Whilst feeling groggy I could easily have stuck my finger in the wrong place and been electrocuted while I was doing that run. Also, the shock sensitivity of liquid acetylene was ignored.

BOHNING: Anyone there at Minnesota that had a specific influence on you, outside of your, let's say your research advisor?

RENFREW: Well, George Glockler had the greatest influence. And

Lee Irvin Smith, who was head of organic chemistry was an important counselor. I had a good, friendly relationship with Lee Irvin. Later on, he was responsible for my returning to Minneapolis to join General Mills. They both had a strong influence. My fellow graduate students also were important, especially Theodore A. Geissman, who successfully combined culture and chemistry. He later became a professor of organic chemistry at UCLA. I shared quarters for three years with Norman H. Cromwell who became head of chemistry at Nebraska. And Stanley Wawzonek, who later was head of chemistry at Iowa, was a close friend. There were many talented students, including Stuart Harrison, who worked with me in General Mills, J. J. Lingane (Harvard), Henry Richter (Du Pont and Colorado), John Bachmann (Akron), Simon Wender (Arkansas), Cy Guss (Nevada-Reno), etc.; a fine group, and I have kept in touch with many of them. Glockler's Nobel Prize winner (Melvin Calvin) just preceded me, but I only barely met him.

BOHNING: Now, as you are completing your work at Minnesota what were you thinking about? It was 1938 when you finished?

RENFREW: Jobs weren't plentiful then. At one point, I had interviewed with General Chemical (a division of Allied Chemical). The General Chemical man had come around and was inviting students to come in and talk with him. I thought, "Well, I'll just have an interview. Maybe I won't pass this Ph.D., I should find out what the job opportunities are". So I signed up to talk to the General Chemical visitor. I remember that faculty members told me that if I truly was desperate and thought I just had to have a job and General Chemical was my only possibility, why they wouldn't complain. But if I had any choice, I shouldn't take a job with General Chemical. They had developed a very bad name for hiring people and then, if there was a slight depression in the Depression, they would fire them. This was considered very bad manners in academic circles. But anyway, since I held a Du Pont Fellowship, it was a foregone conclusion that if Du Pont offered me a job I would take it. And they did offer me a job.

BOHNING: Did anybody come to the campus and interview you before that?

RENFREW: Yes. I was interviewed by the traveling Du Pont personnel man, and also by Harold Paine, who was the laboratory director at the Arlington research laboratory of the Plastics Department. He came through and interviewed me. I was invited then to visit Du Pont for in-depth interviewing. While in Wilmington I made a call on Dr. J. Arthur Almquist, who was an executive in a Du Pont division that wasn't interviewing me. He was an Idaho alum to whom Dr. von Ende had pointed with pride. He gave me a friendly reception; later he was transferred to the

Plastics Department as Chemical Director, and in a real sense he then was my boss. He was a leading prospect to become general manager of the Polychemicals Department that was formed from the Plastics and Ammonia Department soon after I left the company, but he developed a terminal illness and returned to Arizona. When he died, he left a bequest for our use at Idaho in encouraging undergraduate chemists and chemical engineers to plan for advanced study. (He regarded his Ph.D from Berkeley as a major force in his own professional success.) We set up the annual J. Arthur Almquist lecture that has brought many distinguished leaders in chemistry to our campus: Clayton Callis, Mary Good, Ellis Fields, Butch Hanford, Julie Prager, and Otto Glemser, for example. Our students in the student affiliate chapters of ACS and AIChE are the hosts.

BOHNING: Well, I guess that leaves us with moving to Du Pont. What were the conditions like in 1938? It was prior to the second World War, but things were not looking that optimistic within Europe. But the Depression was winding down.

RENFREW: The Depression was still with us! But by the standards of the time, I was quite prosperous. I was hired at $225 a month and that was a really good salary in those days. Du Pont had been a good employer. There were many chemists working in the research laboratory who earlier had been hired as sweepers or dishwashers for a while. They were professional people who hadn't found anything gainful to do. Du Pont would bring them in and let them do manual labor; then later on, as business began to pick up a little, Du Pont would start giving them technical jobs. We were not at the height of prosperity, but we found satisfaction in being there, and it was a wonderful experience for my wife and for me. Arlington was just ten miles from Manhattan. On Saturday afternoons until the war came on, we would go in and see a matinee on Broadway for $1.10; my hearing then was such that, although we were at the back of the theater, I could hear things all right. We really had a wonderful time in the eleven years we were there.

I had married Carol Campbell just after the Minnesota commencement. My bride had graduated from the University of Idaho in economics during my last year there. She was a campus leader and a superior student, and she went to Brown for graduate work in economics the fall that I went to Minnesota. (She stayed only a year but completed her thesis for the Masters degree during our early years in Arlington.) She had returned to her home town (Rosalia, Washington) and worked in the bank until our marriage.

We were engaged for three years, carrying on our romance by mail and in short vacation-time visits. At Minnesota the marriage of chemistry graduate students was not encouraged. In fact, Lee I. Smith strongly held that a serious student would not get mixed up in such a diversion. And I really felt that the strain of a

graduate program was not compatible with the readjustments required by marriage. Nowadays students have proved me wrong in some ways but not others -- there are a good many early divorces!

Carol worked for Du Pont during the war years, and when we came back to Idaho she was a productive part-time worker in the University's developing computer center. But she never aimed for a professional career. Rather she has been a remarkably effective partner in our married life, devoting her talents to worthy causes, including the social aspect of my professional career. She was especially adept in campus activities, including work with students.

BOHNING: What was your first assignment at Du Pont?

RENFREW: My first assignment in the Arlington laboratory involved, as I recall, preparing a low-humidity box for the testing of plastics. So I had to do a little glass-blowing, get some drying agents and a big box, and set up a humidity cabinet.

BOHNING: And that was at Arlington?

RENFREW: That was at Arlington. We were in the period when most of the leading thermoplastics were coming to market. Du Pont was still, at that plant, manufacturing cellulose nitrate plastics. Cellulose acetate was a new polymer made by another division which we were compounding with plasticizers and were selling as a molding powder. Polymethyl methacrylate was just coming along; nylon was being studied as a potential paint brush monofilament and as a molding material. We were setting up an operation for the continuous extrusion of polyvinyl butyral for use as a safety glass interlayer. ICI had discovered polyethylene; we had an exchange agreement with ICI and we began receiving samples of their polyethylene. I was involved with most of these polymers. We also considered taking on a polystyrene research project. At the end of the war, my boss at the time went over to Germany to get the German process on making polystyrene, and we fiddled around with that for a while, but it was discarded in favor of products with a better patent position.

BOHNING: Who was your supervisor?

RENFREW: My boss then was Dr. John Haught, but for most of my first years I reported to Maurice Macht and later to Dr. Chester K. Rosenbaum - others too. Du Pont didn't have a fixed structure. There always were changes in progress.

BOHNING: Can you tell me something about the Arlington works; Oh, I know. There was something else I wanted to ask you first. What did you know about polymers before you started working there?

RENFREW: I didn't know much, in fact, not much was known. I knew that there were plastics, of course. When one of the students at Minnesota was taking his finals in chemical engineering Du Pont had sent around samples of polymethyl methacrylate to faculty to generate interest. One member of the examining committee had held up his sample of the plastic and asked this student, "Do you know what this is?" The boy said, "Celluloid"; and the professor said, "That's right." I knew more about polymers than that professor did.

BOHNING: When you started in the plastics department, did you take any instruction anywhere? Did you ever go over to Brooklyn Polytechnic?

RENFREW: Oh, yes. During the war years. That was a really wonderful experience. On Saturdays we could get out of work (we were on a six-day week because of the war), and go to Brooklyn Polytechnic where they had all-day seminars with such people as Walter H. Stockmayer and Charles C. Price as the instructors. It was a fine thing. And via night school, I took several short courses in chemical engineering in Newark as part of the war Manpower Training Program conducted by Princeton University. I also had a course there from Hugh S. Taylor in physical chemistry, a couple of courses in chemical engineering, and a course from [Eugene] Pacsu in monofilament chemistry, and so on. John Turkevich offered one course. It was wonderful that we could then get this extra formal training.

BOHNING: Can you tell me something about what you remember of Brooklyn? What were those Saturday seminars like? Were they well attended? A lot of people?

RENFREW: Oh, yes. Lots of people came. And we really were trying to learn something. It wasn't just to get out of regular work you know. Du Pont paid transportation and bought lunches for their employees, which was quite a thing for the time. And we'd stay there all day. Later there were Thursday night sessions. The most interesting thing; this jumps the gun a couple of years here. Chester Rosenbaum, who I then reported to, his family, and my wife and I were up at Silver Bay, a YMCA resort on Lake George, on vacation when the first atom bomb was dropped. We had known that this was in the offing, but hadn't known anything about the timing. We returned to Arlington the next week and learned about the chaos at the Thursday night session at Brooklyn Poly when the bomb dropped. Charlie Price was

the instructor. I'm a little fuzzy on this, but the atom bomb was dropped one Thursday when Charlie was trying to lecture. The next week when I attended his lecture, this was VJ day; the big event, and chaos reigned. Brooklyn was an extremely noisy place, so Charlie had give up on that Thursday night lecture, too. Those of us from Arlington took the subway through Times Square en route to the Hudson tube train to New Jersey. We stopped and went upstairs for a look at the square. What a mob scene! That's one of the polymer lectures I remember even if there wasn't much chemistry in it!

BOHNING: What were the facilities like at Arlington?

RENFREW: At Brooklyn Poly?

BOHNING: No. At Arlington.

RENFREW: Oh, Arlington. We had quite good laboratories; they were relatively new. The research was less sophisticated than it is now. We did a lot of taking things off the shelf and pouring them into a pot to see what happened. There had been a laboratory director just before I arrived who'd had the view that chemists shouldn't have desks. If you gave them a desk, he held, they'd be sitting down all the time, and he wanted people up working. He was gone when I got there, but it still was a little hard to find a desk where one could work. You would go into the library if you had to write up something. That gradually changed. But we had good equipment; not up to modern times, but pretty good facilities.

BOHNING: Who were some of your coworkers?

RENFREW: Oh, well, among the chemists... We kept adding them. One was Ernie [Ernest E.] Lewis, who was on the Teflon paper with me. Carleton Sperati, who I think won the history of chemistry award last year, was one of our young people. And Jack Lontz who is identified as an inventor of distinction, worked in my group. Bill [Wilbert L.] Gore, who was responsible later on for Gore-Tex, also worked in my group. (His son was the inventor but the family company, headed by Bill already was an extruder of Teflon and developed the commercial uses.)

BOHNING: Gore-Tex, is that it?

RENFREW: Yes. And goodness, I recall many able Du Ponters. I'm not sure which ones specially will be of interest to you. We may soon go back to the Arlington reunion. The people who were

transferred from Arlington to Wilmington have an annual affair at the Du Pont Country Club. The Arlington operations all are closed now; Du Pont doesn't have anything there. We all were about to be transferred to Wilmington about the time I left to join General Mills. In fact, that contributed to my decision to go back to Minneapolis. I'd known Minneapolis and liked it, and I was never so keen on Wilmington. But the folk who originally were in Arlington now get together every year. If we don't go this time, we will try the next.

BOHNING: Does this group print any programs or have any historical information about the Arlington group?

RENFREW: I don't know, we haven't yet been there. Ernie Lewis is going to be chairman this next time. Again he's the fellow who was on this Teflon paper. He hasn't sent us any formal program for the reunion.

BOHNING: Well, the reason I ask is that if something does come up, maybe you would send me a copy.

[END OF TAPE, SIDE 2]

When did you start to work on Teflon?

RENFREW: Well, most of the projects that we were working on during that period were connected with the war effort. In fact almost all in some degree were committed to victory -- but not with government funding.

BOHNING: Was that true even back in 1938 and 1939? Or did that develop only after 1941?

RENFREW: We began to become defense oriented before 1941. I'd been working on tooth reconstruction materials, early dental filling materials which grew out of my acrylic denture project. That had potential peacetime applications, but we began to get more and more defense oriented. At the time we began working on Teflon, of course there had been the public announcement of what [Roy J.] Plunkett had found. Plunkett, when he opened the cylinder containing tetrafluoroethylene and found the valve plugged, at that moment invented Teflon. He did a few scouting experiments. I think the original patent suggested silver nitrate as a catalyst (14). Some examples were needed for the patent; it was recognized that this would be an important case. Plunkett was in Orchem, and development work was transferred to the Experimental Station. There Butch [William E.] Hanford and his group, Bob [Robert M.] Joyce, John Sauer, and Ken [Kenneth L.] Berry, and a number of others in Central Research worked on

the polymerization process and some applications. It was recognized that Orchem's Jackson Laboratory, where Teflon had been discovered would do the monomer synthesis, and the Plastics Department would do the scale-up of polymerization and development. And so Teflon came to us at Arlington and we began working along on the same lines that they were doing at the Experimental Station. This leads into the story about my then boss, P. Willard Crane.

He came to me one day and said, "We've been invited to go over and talk to Professor [John R.] Dunning at Columbia." Willard was always one to play his cards close to his chest so I don't know how much he knew in advance of what we were going to hear. But I was quite popeyed by the conversation we had with Dunning. J. R. Dunning was an enthusiastic and able physicist who got right to the point when we went in to see him: he told us that there was a development now coming on in this country and in Germany which would determine who would win the war, that it was going to be extraordinarily important for us to be participating at our maximum strength. He knew that we were making polytetra- fluoroethylene (PTFE) at Arlington, and it was believed that this product was going to be vital to this war project. There was soon going to be urgent demands for what we then called poly-F1114. He had a schedule for us to meet: we were to have one week to complete our development work on the polymerization process; we would have two weeks for the design of the plant; we would have one week for plant construction, and then we were supposed to be producing polytetrafluoroethylene at the rate (I think) of a million pounds a month! Now, it may have been a million pounds a year, but I think it was a million pounds a month! We never made that schedule. But we did move ahead on process development into the pilot plant before the safety practices were in place.

BOHNING: What was Crane's response to Dunning's proposal?

RENFREW: Well, we both felt that this was a <u>little</u> unrealistic. I never really knew whether Du Pont management knew about this in advance or whether this was Dunning's first contact. Willard, of course, went up the line to tell management what our conversation had been. We both speculated on what could require so much polymer. We really didn't think then of nuclear energy as being involved, but ... Several weeks later, I took some of our samples of polytetrafluoroethylene over to Columbia. By that time Havermeyer Hall was known as the SAM Laboratories. They had a sign-in procedure when we went in, and there were security guards around. The fellow who had signed in ahead of me was A. O. Nier, a physics professor from Minnesota. There had been a piece in the New York Times some weeks earlier about Nier's finding that you could split the atom with the release of energy. He was almost up with what the Germans had done. I had read the news story, and I now deduced what we were involved in, but we didn't know quite how we fitted into the picture.

It turned out, of course, that they were counting on PTFE to be the gasket material for the gaseous diffusion process, that was to be used at Oak Ridge in the gaseous separation of uranium isotopes. But it didn't turn out to be a really satisfactory gasket material; it would flow under pressure. Also, there were enough impurities in the polymer the way we were making it then that there were reactions with uranium hexafluoride. There were a number of things that mitigated against Dunning's proposed use, but a lot of PTFE went into the war effort. A lot of it was used later at Hanford in the plutonium process and that's where Willard Crane soon went. He was transferred there by Du Pont and was there during the rest of the war years. I then moved into his position. I was head of an engineering group that was doing the scale-up of polymerization. I had taken a night-school course in Badger and McCabe (15), and thus became an engineer! It may have been a source of embarrassment to my chemical engineering friends that I got into the American Institute of Chemical Engineers as a "senior member". They had such a classification then and I made that on the strength of my short course and my associates.

BOHNING: You said that Dunning wanted to be making a million pounds a month. You then were making the polymer in hundred gram lots?

RENFREW: Fifty or a hundred grams. We had a small rocker tube and we did early manufacture in that equipment.

BOHNING: Were you principally looking at catalysts, trying to find better catalysts?

RENFREW: Well, this was one of the needs. Actually, I can't remember the exact schedule. I told you that one of my individual contributions, in a sense, was the development of an initiator that was used later on for the manufacture of most of Du Pont's polytetrafluoroethylene. We originally were using ammonium persulfate, and were getting a granular polymer. It was later, I don't know how many months later, that we got into this work on alternative catalysts to improve the properties of the product.

BOHNING: How many people were there working with you on this?

RENFREW: Oh, we had a group of, I suppose, ten or twelve young engineers on various aspects of the process, plus some ancillary help. Most of our group was involved in Teflon. Earlier, I spoke to you about the accidents that we had: the tragedies.

At that time we were running a pilot plant. We'd scaled the process up to a two-hundred gallon reactor and were making batch polymerizations. The monomer would come from the Jackson Laboratory with an inhibitor, and we would have to distill it to remove the inhibitor and then carry out the polymerization. We had a three-shift operation. Also, at Arlington we were molding the polymer by a heat-sintering process that made blocks which then could be shaved to produce tapes or produce gasket materials. As I probably often have said, and which most people know, the pilot plant stage is where processes are apt to run into accidents. Even at best, the hazards are there because you're doing new things with scaled-up quantities. And so we had our troubles. The first tragedy involved the Organic Chemicals department where workers who had been purifying monomer by distillation took still residues and tried to vent them up a hood in the laboratory. They were poisoned by material that leaked out of the hood. Three of them died.

BOHNING: What material would that have been?

RENFREW: Oh, they were fluorine-containing chemicals not then identified. I remember George Holbrook from the Jackson Laboratory came up to our place. We had a Podbielniak still at the time that we used in purifying monomers. He brought up some of this material, and we were trying to identify what it was. George had been the supervisor of the people who were killed. Later on he was a pillar in the American Institute of Chemical Engineers. I remember his anguish. Later on I could share it.

BOHNING: When you say that they died from inhalation, was it the fact that the hoods were incapable of venting it properly, or was there too much at one time?

RENFREW: I don't know. We didn't know as much about hoods then as we do now. They apparently had run a tube from a cylinder of still residues over into the hood and as I recall they had actually been venting for some hours. I don't whether they had the sash down. But anyway, they were venting the still residues from cylinders, and this evidently was not all going up the stack. Some hours later the lab workers developed the severe respiratory problems that killed them. (Venting toxic gases in a hood is now widely recognized as a bad practice.)

BOHNING: Was there anyone else in the room with them at the time?

RENFREW: I don't know. I wasn't down there. That was down at the Jackson Laboratory, across the bay from Wilmington.

BOHNING: What was the response of Du Pont after that happened?

RENFREW: I wasn't as deeply involved in that accident. When we had ours, I became impressed with the fact that the company regarded safety as an economic necessity. They weren't just paternalistic, there were economic factors involved. They were not going to make the widows who were left wealthy. Of course there were state laws as to what the compensation requirements were in industrial accidents. I think the implication was that Du Pont would not really exceed what they had to do, but in cases of hardship, they would then look after people. This was the impression that I had.

Our accident was in the pilot plant stage. We were short-handed and were trying to do too much. We had just two men on the graveyard shift. We had around-the-clock operations. There were bigger shifts daytime, but from twelve to eight, that particular shift involved only two men who had presumably less responsibility and could do a lot of things. I had been in Wilmington all day, had gotten home late. I lived near enough to the plant that I could walk to work. At one-thirty in the morning I heard an explosion. A little while later the phone rang, and I was told that I should now call the wife of one of the boys that was injured. She lived upstairs in the apartment where we lived. She was pregnant. They had been teachers in a Sunday school class where I was the superintendent. And I had to sit with her and the wife of the other young man at the hospital the night these kids died. It was just before Thanksgiving, and the parents of one boy that was killed, the one that I knew best, came from Kansas and my wife and I looked after them. But we immediately had to start rebuilding the plant. Also there was an immediate investigation to see whether sabotage was involved; an important question. I don't know how we carried the load during the redesign and immediate reconstruction of the plant -- with greater safeguards.

BOHNING: What was the time frame of these accidents? When did the first one occur down at Jackson Labs and then when was yours?

RENFREW: I don't know if I could reconstruct that. The one at Jackson Lab happened first, but I can't recall the exact time.

BOHNING: Okay.

RENFREW: For our plant we developed a remote control system where everything was done behind barricades. Our safety precautions earlier depended upon keeping monomer at low temperature. And the thing that had happened was that, every now and then, one of the cylinders containing refined monomer, which

was being kept at low temperature prior to polymerization, would start plugging the valve with polymer. Whenever this began to happen we would take the cylinder, chill it in dry ice and solvent, take the valve out, replacing it with a clear valve. Ultimately it turned out that the two fellows that were killed on the night shift had decided that they would change the valve on one of the cylinders that they had decided was empty. But they didn't get it done, so they left it for the next shift, but the next shift was too busy, so they didn't get it done. The next shift didn't get to it either, too busy, and so when these people came back on the twelve to eight, here was this cylinder sitting out that still had never had anything done to it. They were in the course of taking the valve out when the cylinder blew up. What had happened evidently was they had misread the weight of the cylinder. According to the log book, they missed it by ten pounds. They thought it was empty and they'd thrown it on the scale but then misread the scale. It contained 10 pounds of refined monomer. On a twelve to eight shift, I suppose human errors occur more frequently. It wasn't the kind of an event anyone could defend very well as a safety expert. It was a dreadful business.

BOHNING: How old were these people?

RENFREW: They were under thirty, both of them. One of them was a college-trained engineer and the other one was a superior technician.

BOHNING: What was the frequency of incidents like this happening at Du Pont during that...

RENFREW: Oh, they were rare. Du Pont was really a pioneer in laboratory and plant safety. This is interesting. [Edward G.] Jefferson, the retired president of Du Pont, gave a talk not long ago in which he attributed much of Du Pont's economic success to the fact that they developed a safety program which was a good program. Du Pont employees had all kinds of reasons to be safety conscious. There would be contests in which plants would compete for the maximum number of man hours without an accident. This was posted outside the plants every day. And boy, if a fellow had an accident, he was an unpopular character. If he'd smashed his thumb and had to go to the hospital, he was going to be in disgrace, since then we would not win a safety prize. The first nylon stockings my wife ever had came as a safety prize. During this period the company offered such prizes for all people working in a plant or a laboratory. We were a laboratory in a plant so we had this factory atmosphere in addition to having our research program there.

BOHNING: In view of the fact that your accident at Arlington was essentially human error, did the company take care of the families in any respect?

RENFREW: Well, to the best of my knowledge they met the minimum requirements. They had a life insurance policy, in addition to the minimum workman's compensation requirement. We were told, sort of unofficially, that if the wives and children became really troubled financially the Du Pont company would look after them. I don't know whether that ever happened or not. It certainly was not a case in which the families were compensated for the value of the lives of the young people who were sacrificed.

BOHNING: Were you involved at all in any of the UF6 testing or other property testing of the polymer? Or were you involved primarily in just the manufacturing?

RENFREW: We had to do some testing. At first, we would take most of our samples over to the SAM laboratories and deal with the people there. But we soon did the tests ourselves. We received some of the first fluorine gas that was being distributed in cylinders. One of the safety people in the Arlington plant nearly had apoplexy one day because he was walking by one of our laboratory facilities. Originally they had been nitration cells and had been rebuilt as small laboratories. This fellow came walking by one of them where there was a copper tube sticking out the window, and he happened to look just as a three-foot area of grass went up in smoke. He came in, wild-eyed, to find out what the hell was going on in this laboratory. We were venting a cylinder that contained fluorine which we had been using to test the inertness of samples of PTFE made under different conditions to see how they behaved in fluorine.

BOHNING: How long did your work in Teflon continue? The ACS paper and that IEC paper were after the war was over. Is that correct?

RENFREW: Well, I stayed with it until after the war. Later I was involved in product development. The paper was given in 1946, as I recall. The paper was published in September of 1946, and it was the spring of 1946 when I gave that paper (16) in Atlantic City at a national ACS meeting. (I left Du Pont in 1949.) I first had been head of the so-called process development group, and then I became head of the product development group. So I worked on evaluations of polytetrafluoroethylene and other plastics in various commercial applications. We, of course, had other products that were coming along. Polymethyl methacrylate, polyethylene, nylon moldings, various experimental polymers, etc., were evaluated for potential applications.

BOHNING: What was the impetus behind giving out Teflon information at an ACS meeting?

RENFREW: Du Pont, of course, was interested in marketing the product commercially, but the price was considered ridiculous. It was fifty-five dollars a pound at the time. But it was an extraordinary product that was obviously going to have some unusual uses in the peacetime economy. We had to start developing markets for it, Du Pont wanted to increase commercial interest. An extraordinary day of my life was the day that I gave the paper. The Du Pont advertising and marketing people got me out of bed that morning for last minute counsel. One of the men came up with something that I incorporated in the paper as a real nice touch. He offered the statement; "If anybody at this American Chemical Society meeting has come up with a universal solvent, we have the container for it."

But anyway, I was involved in a number of things all morning. I didn't get any breakfast, I didn't get any lunch. I gave the paper shortly after lunch; it was the first paper after lunch. We had a tremendous crowd. People stayed around afterwards asking me questions, and so I didn't get anything to eat. I went to the ACS News Service cocktail party where Glenn Seaborg was really the lion of the hour, but the Teflon paper had attracted quite a bit of interest. When I went to this social hour I was hungry and thirsty. I drank far too many Manhattans and I became "the worst one" in Atlantic City. I finally got to Charlie [Charles L.] Parsons' farewell banquet where I wanted to see my major professor, George Glockler. When I finally got there with a bunch of barfly newsmen, George Glockler took one look at us, got up, and left the meeting. It was a long time before I really was on good terms with him again!

BOHNING: You said he wasn't very strict...

RENFREW: Well, Glockler was academically liberal, but a bottle of beer on a warm summer afternoon was his idea of drinking. Really, I was in a disreputable condition; I don't know how I was navigating.

But I want to say one other thing. Strangely enough, although that Teflon paper attracted a great deal of interest, the next paper on the program was probably of much more scientific importance (17). Cal [Calvin E.] Schildknecht gave the paper. Do you know Cal Schildknecht?

BOHNING: I know the name.

RENFREW: In his paper he discussed for the first time the possibility that you might have isomers formed in polymerization. In a way, this preceded [Giulio] Natta and [Karl] Ziegler who were awarded the Nobel Prize in 1963. Cal didn't have it tied down; all he had were these suspicions. Depending on how he polymerized isobutylene, I think it was isobutylene, he was getting differences in properties. And so he suspected that there must be isomeric differences that were involved. I was listening to it with one ear while people were asking me questions, and I thought poor Cal had flipped his lid. This was an impossibility! Nobody was really paying much attention to him. It must have been a discouraging thing for him because here he had one of the important papers at the meeting and it was largely ignored.

BOHNING: Well, I suppose...

RENFREW: He had been a worker at the Du Pont Arlington laboratory earlier, had left, and had gone to General Aniline and Film where Butch Hanford had become research director.

BOHNING: Did he get many questions afterwards or was it just skepticism?

RENFREW: I don't think too much attention, there was just skepticism. I don't recall that he got many questions at all. He later wrote good books on polymerization. He was of German extraction and was a really rigid character, a perfectionist; not a salesman, but he had an important contribution which needed more selling then. He became the head of chemistry later on at Gettysburg College, and he ran a tight ship there. I don't know that you've had contacts with him.

BOHNING: That may be where I know the name. From Gettysburg. Was there a link between Du Pont announcing Teflon and its properties to the world and sales?

RENFREW: Oh, yes. It was basically a sales thing. Actually Bob Joyce and Butch Hanford and somebody else prepared a paper for the Journal of the American Chemical Society which was more of a scientific publication (18). But I was handed the job of giving the presentation at the American Chemical Society meeting which was the first official public disclosure. Later on when the publications appeared, it's my recollection that Hanford and Joyce came out a month ahead of our Industrial Engineering and Chemistry paper.

Perhaps of interest is this: seven years after I left Du Pont I received an "A bonus" for unusual contributions because of my

part in the Teflon development. The company had no obligation for this. I have never heard of another company doing such a thing, and I don't know how widely Du Pont had distributed such awards to ex-employees.

BOHNING: Now, you left Du Pont in 1949. You had mentioned earlier there were three major advances. I'm not sure if the other two took place at Du Pont or after you left.

RENFREW: Well, there was another at Du Pont. The photo-polymerization work took place at Du Pont. We were trying to make very large castings of methylmethacrylate polymer which would be used in Schmidt lenses that the Air Force presumably needed for photographic inspection by airplanes flying over battle areas. These had to be optically perfect, and it was very difficult to get such large castings. If you tried to make them out of molding material there was enough adsorption on the surface of granules that these introduced aberrations. We needed "perfect" castings. Well, when you are trying to cast anything this big from monomer/polymer syrups heat generated in the middle of the casting leads to bubbles, a serious defect. So we decided to do some photopolymerization studies to see if we could handle the polymerization on a longer term basis without having so much heat at the critical period.

There had been discoveries at the Experimental Station that acyloins [RCOCH(OH)R] were good photocatalysts, and I read the patent (19). We tried acyloins and they didn't solve the problem, particularly since there tended to be an obnoxious yellow color with the acyloins. Agre, that inventor, hadn't tried acyloin ethers. So I started synthesizing some ethers of acyloins to see if they would improve on what the acyloins were able to do. I made the ethyl ether of an aromatic acyloin, and this was a tremendous photo-polymerization catalyst. It really was extraordinary. But it didn't solve the problem on which I was working. There still were bubbles and some color formation. We patented it but weren't able to do anything else with it in wartime, although I think I included it later in a patent application on polymerizable dental fillings (20). I had a dental filling inserted which used the photoinitiator in the filling material. It lasted a long time, but polymethyl methacrylate isn't an ideal filling; there is too much water pickup and swelling. The initiator later was used by Du Pont's Central Research Department in developing photoprinting processes that became commercially important.

I had a personal triumph of sorts in this project; we had an assistant director of research named Robert E. Burk, who was a kind of a bull in a china shop. He'd come from Western Reserve University to Du Pont and was a very bright man. He'd been a Rhodes scholar, and modesty was not one of his virtues.

[END OF TAPE, SIDE 3]

Bob was really an aggravation. But I reported to him and also to another assistant director of research, George Graves, and they didn't get along one damn bit. Graves told me with much feeling, "If I did what Burk wanted, he (George) would get my ass, that I was not to do what Burk wanted." "Burk", he said, "was going to get no place in the Du Pont Company and we should not pay attention to him." George proved to be right, but the conflict didn't make my job easy. Burk went to England on a company mission after the war, and when he came back, he was very high on what the British were able to do. He wanted to know why here we couldn't do these things the British could. At one point he told how they'd developed this extraordinary photo-polymerization catalyst at the ICI laboratories. It was a wonderful scientific development, and how come that over here we couldn't do things like that. Why could they do it in England? And I could say, "Well, they read our reports. I have a patent on that." That was one of life's triumphs.

I might offer additional comments on George Graves. I liked him as a person, even though he was the only boss who ever made me feel consistently that I was doing less than I should. He had a reputation for speaking bluntly but was highly regarded in the company for his ability to push projects successfully. He was moved around among divisions of the company in various administrative assignments. Once when I was in Wilmington on behalf of General Mills' search for polymer films of improved toughness at low temperature for use in their high altitude balloons, my meeting with the Du Pont people in the Nemours Building finished up early, and I said that I would drop in on George, who was located on the next floor.

When I found him he was chewing up a secretary. Crawford Greenewalt had just been elevated to higher office in the hierarchy, and as an old friend from earlier times, George had dictated a letter of congratulations. The secretary had typed the salutation as Dear "Greenie" and George didn't like the use of quotation marks.

When he simmered down we had a friendly visit that ended in my embarrassment. Among other things, we talked about one of the sons of a high official in the company. George held that he was a phony who always arranged to have himself paged at conferences, concerts and other events. Shortly afterwards the telephone bell sounded. It was Ralph Manley, my boss in General Mills. The call had been transferred to George's office as my last known location. Ralph really didn't have much to say, and I never could fathom why he called. George, however, with a wicked grin, obviously was concluding that I had arranged this to demonstrate my new importance.

The mention of balloons prompts another diversion. General Mills had a division largely concerned with military developments. Among them was the construction and flying of large balloons that could serve in spying over enemy territory (some lower flying

versions were tested for getting personnel behind enemy lines and for escape. The balloons were silent and not detected by radar). There were no airplanes then capable of flying at 80,000 feet.

As a plastics man I was drafted for consulting services. A major problem involved the embrittlement of polyethylene when the balloons went through the minimum temperature zone at around 30,000 feet. This quite often broke up the balloon with the loss costly instrument packages and a danger to any citizens on the ground who might be on the receiving end of the 1000lb load. As a loyal Du Ponter I, of course, recommended Du Pont polyethylene. But we developed a low temperature brittleness test, and I was shocked to find that Du Pont film performed less well than the competitive product from Union Carbide.

At the time we were purchasing very large quantities of polyethylene, and our work encouraged some important studies by Du Pont (and others) searching for the causes of differences in low temperature properties of various polyethylenes. I was later told that our General Mills test was adopted by Du Pont as a way for measuring toughness at low temperatures.

But, to resume, Du Pont at the Experimental Station soon was starting major work on photopolymerization. A fellow named Plambeck had been working on this, and he picked up my ethyl ether of benzoin as a super initiator. Plambeck later on won the Carothers Prize. As part of his lecture, he did tell how the photopolymerization was advanced by the discovery of this "catalyst" that I'd been responsible for. Blaine McKusick of Du Pont, who is a force for good in our ACS safety activities, wrote to tell me that Plambeck had mentioned this in the course of his Carothers lecture. I hadn't kept up on this and I wrote to Plambeck to see what was going on. He was then retired, but in sending me his Carothers paper he wrote, "I tried to emphasize that the image-forming polymerization process developed was a culmination of work and observations of many Du Pont chemists over a period of years. Your early work on benzoin ethyl ether as a photoinitiator was particularly important at a critical state in photopolymer development" (21). This got work started outside of the Experimental Station, and, of course, it ultimately became a very large commercial business. I don`t know that Du Pont ever made any money out of my invention, because I think before the business became really big, the patent had probably run out.

BOHNING: Is that the patent where your name was the only name on it because the lawyers felt it was...

RENFREW: No. No, that was another Du Pont initiator patent. The photopolymerization patent clearly was mine, but the other case involving the initiator for the polymerization of tetrafluoroethylene was fuzzier.

BOHNING: Could you say something about it?

RENFREW: As I said earlier, we were looking for additional ways to polymerize tetrafluoroethylene. We were trying to overcome the product virtues that were liabilities in fabrication. We wanted a product that would mold easily. I was responsible for the group that was doing the polymerization studies. I assigned the preparation of this peroxide of succinic acid and its use as an initiator of polymerization for tetrafluoroethylene. When the people who carried out the polymerization opened the reaction bomb, the polymer was an emulsion, more accurately, a suspensoid. It was at a rather low solids concentration, five or six percent, but it was in a suspensoid stage as a colloidal dispersion which would eventually settle out but which could be redispersed. But there is the question: if Plunkett could get a patent for opening a cylinder and finding the first polymer, why shouldn't the guy get the patent who opened the bomb and found the suspensoid. But it was decided by the Du Pont lawyers that I had assigned the experiment and then, when the product was brought in, I had recognized its importance, so I was the inventor (22).

BOHNING: You say they felt that having more than one name on the patent at that time was...

RENFREW: At that time it was felt that the "flash of genius concept" was what governed patents, and it was kind of embarrassing to have two people on a patent. If you had a number of people involved, it lessened the chance of getting a patent. I think that was part of the philosophy of the time. Several of our people were there when the reactor containing the suspensoid was opened. They all couldn't be inventors.

BOHNING: When or why did you decide to leave Du Pont?

RENFREW: Well, we were about to be moved to Wilmington. I received a call from Lee I. Smith, of the University of Minnesota who'd been advising James Ford Bell at General Mills. Lee asked if I would come out and talk to them about taking a job with General Mills. My psyche had had some dents put in it by that explosion of tetrafluoroethylene, and I was not too happy about going to Wilmington. I'd liked Minneapolis when I was there as a student, and General Mills offered a good opportunity. I had great respect for Du Pont, and it was a long time before I ever ran into an alumnus of Du Pont who didn't speak well of the company. But it was a period when we felt moving to Minneapolis would be better for us than staying with the company.

James Ford Bell, who had played the key part in founding

General Mills by bringing several milling companies together, was a truly remarkable man. He had completed an undergraduate degree in chemistry, at Yale as I recall, and never lost interest in science though his genius was in business management.

He had served as president of the company and as chairman of the Board. In my time he was still a director, and he headed the committee on Finance and Research. It was a one-man committee as he didn't believe that large committees accomplished much. It was his view that business had to follow where research led, not vice versa. And whenever we had budget discussions he always was concerned lest we put too much effort into development work and not enough into basic research. He also served on the boards of the Eastman Kodak and Merck, and he loved to take publications from our research laboratory to show to their research people.

Also he was public spirited, an early environmentalist and a Regent of the University of Minnesota. I once accompanied the General Mills directors to a meeting in Oklahoma, where I talked about chemical projects. On the way Mr. Bell frequently was calling the Minnesota legislature (the legislature was then in session) promoting an expanded budget for the University. At a welcoming dinner in Oklahoma City, hosted by their Chamber of Commerce, Mr. Bell was asked to offer a response. His theme was this: "You have been taking a lot of wealth out of the ground. What are you putting back?" They don't make tycoons like that anymore.

BOHNING: I believe you said that you had one other major area that you wanted to discuss as part of what you did at General Mills.

RENFREW: Well, the contribution at General Mills that I think deserves recognition involved the use of a reactive polyamide resin as a curing agent for epoxy resins. I had attended a meeting of ASTM [American Society for Testing & Materials], in which there was great interest in the so-called potting compounds for insulation. You know, you'd take these casting materials, put electrical components into a mold, pour in the liquid reagents, and "cure" them. This was an important development. Epoxy compounds were vital to the argument, but the kinds of curing agents tended to be volatile. People also tended to become sensitized to them, so there was a toxicity problem, and the industry was looking for alternatives.

It occurred to me on the way back from the meeting that we were making a reactive polyamide in General Mills which might well be superior curing agent and component. This involved the polymer acids (made from polymerizing vegetable oils) and ethylene diamine, forming a polyamide. We also had some made with diethylene triamine, and I thought these would have unreacted amino groups in which the amino hydrogens could act as curing agents. I asked Harold Wittcoff, who was in my group, to carry out some experiments

with this. Harold did so and it gave rapid curing and a product of unusual properties. Harold was a very enterprising, bright young man. He recalled that he had tried our conventional polyamides earlier in combination with epoxies. The results then weren't promising but when he went back after some months, the samples had polymerized. Our patent man thought it would be a real good idea to go back to the initial experiment since that gave us an earlier date. So Harold and I became co-inventors (23). Harold kind of made his fortune out of this. He stayed with General Mills and our invention became very important commercially. The chief uses of the epoxy/polyamide compositions were in protective coatings where their virtues of toughness and corrosion resistance counter balanced high cost and difficulties in application. They required a two-can system. But among the publicized uses was under-water painting by deep sea divers for the protection of oil rigs in the Gulf of Mexico. Aircraft also were painted with epoxy/polyamide. The epoxy/polyamide system was ranked by Howard Gerhart of PPG as one of the important inventions in the development of protective coatings. He called it the "technical milestone event of 1953" in an historical review in the Journal of Paint Technology (24).

BOHNING: I believe you said this was the one where they did several million dollars royalty business only in the last few years of the patent?

RENFREW: Yes. Originally it had been felt that there was no point in going around telling the customer that they had to take out a license from us to use our reactive polyamides, because we were the only manufacturers. For quite a while merchandising did not call for royalties for the patents, but a woman who had sales rights in England began selling the polyamide over there. According to the story that Harold told me later, she insisted that if she was going to make money out of this in England it would involve collecting royalties on the British patent and we had to do the same over here in order to make her case legitimate. General Mills then began collecting royalties in addition to what they were making out of selling the polyamide. Harold said they made three or four million dollars out of the licensing alone before the patent expired.

BOHNING: And you were at General Mills for about five years. And then you moved to Kellogg.

RENFREW: Yes. Spencer Kellogg and Sons, Inc.

BOHNING: What was responsible for that move? Were you still doing mainly polymer work at General Mills?

RENFREW: Well, not just polymers. We had all kinds of things under investigation. Polymers were part of it, but not necessarily even the major part. My move to Buffalo: it was an unwise move. When I was with Du Pont I had bosses who were well informed. When our research supervisors took home briefcases at night with reports they read them. When I got to General Mills, I was shocked to discover that the vice president for research wanted only a three-sentence progress report once a month on each research project. Later on, when I got to Spencer Kellogg, I found that I couldn't even get the management to read the three sentences!

But this position was offered. One of these flesh peddlers came around, a very personable fellow. He offered a chance for me, not to be the director of chemical research, but to be the director of all research. I was director of chemical research and development at that time at General Mills, primarily a food company. But I could be the director of research and development if I went to Spencer Kellogg. It was largely a family-held firm, though stock traded on the Big Board. It had been a very honorable firm but it was in oil seed processing, and making little in the way of profits. The current head of the company, a grandson of the founder, was determined that he was going to modernize things. So he had hired this bright fellow who had talked to me, and we were the reform movement in the company. Well, it turned out that the bulk of the people there felt if the price of linseed oil would only go up, they wouldn't have to put up with all this new stuff. So, although we built a new research laboratory and we had some new products coming along that were really promising, it was an extremely frustrating business. Eventually Howard Kellogg, the president, concluded that I was more of a nuisance than an asset, and I was being encouraged to move. In a year or two the company was bought by Textron, who sold off obsolete properties and fired the dead wood. They kept our research lab and new product lines and it became a small but profitable operation.

Just about this time, the headship of physical science opened up at the University of Idaho. I wouldn't have known about it except for Gordon Harris, who was head of chemistry at the University of Buffalo. Idaho had sent out letters asking for nominees, and Gordon Harris turned in my name. When I received inquiries, I knew what I wanted to do. I'd always thought being head of chemistry at the University of Idaho would be the ideal job. I was in Fargo where I had been offered a professorship in coating technology at North Dakota State when the offer came by telephone from Idaho. My choice was predetermined. I went back as the head of the physical science division, administratively responsible for physics and chemistry. Then later on we split up the departments when we began to do better. Physics was at an extremely low ebb when I got there. I had hoped that I could start resuming some polymer research. It would have been difficult at best because, normally, when people move like this, they take young

people with them who've learned the art and can go in and teach others to get the program going. Well, I didn't have anybody like that. Besides I was considered a half-time teacher. I was administratively responsible for physics and chemistry, and I taught six hours a semester, full-time in summers.

We had an unbelievable situation in physics. It was in the post-Sputnik era, and physicists were hard to hire. We had one fellow teaching upper division physics when I got there who later on was a janitor on the campus. It wasn't strictly technical incompetence that handicapped him but he was emotionally incapable of really facing a class. He was assigned to both junior and senior courses in physics. We had other situations which were equally bad, and so I spent a great deal of my early time there trying to hire physicists. I learned the names of the children of many of the graduating physicists from leading institutions in the country! Physics gradually built up, and eventually we split the departments. I became head of chemistry then, and I retired ten or eleven years ago.

BOHNING: How did you feel teaching after all those years?

RENFREW: Well, I found it quite a strain. I'd wanted to teach. Actually, for one period at Du Pont, I taught a course for the Essex County Vocational School System in an apprentice training program that they had at our plant. I had enjoyed that very much, and I really felt I had skills as a teacher, but until I gave up administration, I always had to put my teaching in second place. There were always administrative assignments that absolutely required attention. It wasn't until I gave up the department chairmanship, which I did for the last few years there, that I really got a chance to teach the way I wanted to. But I had a lot of fun teaching in those last years.

BOHNING: What areas were you teaching?

RENFREW: Freshman chemistry primarily, although I ran a seminar course for seniors trying to fit them to go out into the world; I usually taught that. Sometimes I taught science courses for non-scientists. When I first went back to the University, we were getting our Ph.D. program started. The university wasn't qualified either with equipment or personnel to do it, but they'd started the program. I had to teach whatever was left over. Once I taught a graduate course in molecular spectra. Now that was really quite a chore, I'll tell you! I was more than 20 years away from my Ph.D. I taught courses in polymer chemistry for several years; we had polymer courses at that time which later were taken over by chemical engineering.

BOHNING: I guess it was when you went back to Idaho that you

became very vocal in chemical safety. Is that correct?

RENFREW: Well, yes. You know, universities hadn't caught up with the improvements that had been made in safety practices, and I was shocked by what I remembered from my own university days at Minnesota. When I came back to Idaho I was determined we were going to do better. That started me. Then later on, when I was with the Advisory Council on College Chemistry, that was the NSF-funded program set up at Stanford, I felt that this was a chance to do more on academic safety. So I kept thumping this theme while I was on the AC3 staff. Tom [William T.] Lippincott, who had been one of the directors of the Advisory Council for College Chemistry later replaced Bill Kieffer as editor for the Journal of Chemical Education. He asked me to edit the safety column and I started doing that. That led into activities in the Chemical Health and Safety Division when it subsequently was formed.

BOHNING: I don't recall the history of the division. Is it a young division? Was it founded recently or has it been around for some time?

RENFREW: It was founded quite recently. I can't remember exactly the year. I became the fifth chairman, as I recall, in the historical sequence.

BOHNING: Were you instrumental in its organization?

RENFREW: I was a charter member, but I wasn't the leading spirit. I was more interested in the ACS safety committee. Actually I was on the first continuing safety committee in ACS. A Du Pont alumnus named Livingston, Bert [Herbert K.] Livingston, who had gone to Wayne State University, became the first continuing head of the ACS safety committee. The committee had been set up by the Board of Directors and one of the directors had briefly served as the head, but Bert Livingston soon took over. He had known of me as a Du Pont employee, and he asked me to serve on this committee. Several times later on I served on the committee, and the activities of the committee later led to the formation of the division. I wasn't as enthusiastic as some of the instigators since I felt that as we had a safety committee in the ACS that needed more attention, we shouldn't dilute our efforts by forming a division. Safety activities in ACS long were regarded as potential trouble by some corporate managers who were influential in the Society. Hence for a long time this contributed to the ineffectiveness of committee programs.

There had been a hassle in ACS "management" over the Safety Committee response to the vinyl chloride crisis early in the sixties. When it was found that workers who had been working with vinyl chloride were developing a specific and characteristic form

of cancer (angiosarcoma), we members of the Safety Committee were called to Washington for an emergency meeting by Stephen Quigley, then on the ACS staff. We came up with a guideline statement for the protection of workers that later proved to be quite reasonable, but producers of polyvinyl chloride held that the recommendations would put them out of business. At the following ACS council meeting, I happened to sit next to Jim D'Ianni of Goodyear (he later became president of ACS) who was still livid.

Howard Fawcett, who was chairman of the Safety Committee at the time, was employed by the National Academy of Sciences heading a group activity on hazardous materials. His job there quite promptly was "eliminated." He has held that the termination resulted from pressure applied by industrial sponsors, who didn't like the ACS vinyl chloride action. For some years the Safety Committee then had a "temporary" status in the ACS structure. Only recently (1988) did it become a Board/ Council Committee with permanent standing.

BOHNING: What kind of impact do you think your column in Journal of Chemical Education has had in promoting laboratory safety?

RENFREW: I don't know. I sometimes feel nobody reads it. I get few complaints and fewer commendations about the columns that we've carried. Once in a while somebody comes along and says that it has been an asset to them. I get quite a few calls from people who have safety questions and I try to be helpful; I collect literature for them and refer them to "experts". I've been asked to participate in some of the hot legal cases. I've tended to ignore them. I don't think fast enough on my feet. If you're going to be a qualified expert, you must have answers right now. So, even if I'm inclined to be helpful in a case, I don't want to get into it as an expert witness.

BOHNING: Did you appear as an expert witness earlier in your career?

RENFREW: I had two experiences in Du Pont. Once where I was involved in a family row in Brooklyn between a dentist and his supplier of denture materials, who turned out to be his brother-in-law! I had to be an expert witness in this case. But I didn't really get a chance to demonstrate my expertise, the lawyer for the shop man asked me to pick out a "Lucitone" denture in a group he had brought to the hearing. I was quite sure that I could do this, but a Du Pont lawyer in the courtroom was shaking his head in warning. We had no assurances that even if I picked the right one my selection would be confirmed. Hence, I had to decline the opportunity to distinguish myself. Later, Du Pont and Rohm & Haas were accused by the government in an antitrust suit. The government brought price-fixing charges

against them in connection with denture materials, and early in the trial I had to be there for calling as an expert witness. I was extremely reluctant to get into this because I felt Du Pont was guilty as shucks and that all I could say would be embarrassing to my employer. It turned out that I got to go to the opening of the trial and was tremendously impressed with the battery of legal talent that Du Pont and Rohm & Haas had hired. The leading trial lawyers in the country were there -- individual counsels for the people who were involved in the suit, and then the Du Pont and Rohm & Haas corporations had their own battery of lawyers. There was an inept government attorney who was fighting them; and I thought that, if I were on the jury in this case, I would simply assume that that fellow had a better case than he was able to present and that the opposition was overwhelming him. But Du Pont and Rohm & Haas won the suit. The government prosecutor, who became a New Jersey politician, later on was indicted for his own peccadillos.

There were some interesting things in the trial. Old Dr. Haas was somewhat in his dotage. At one point the prosecution brought out the fact that the head of the Du Pont Division involved had called Haas and said, "I'm calling you rather than writing a letter because I don't want these damn government attorneys to get a hold of this." Dr. Haas dutifully wrote this preamble down as a memo in his file! But anyway, Du Pont and Rohm & Haas won without my damaging participation.

BOHNING: Well, we're running toward four thirty and both of us have places to be at five. And I think what I'm going to do is stop at this point.

RENFREW: I've told you some things that chemists don't need to know!

BOHNING: I really appreciate this delightful two hours and I thank you for your time.

[END OF INTERVIEW]

ADDENDUM

[Added by Dr. Renfrew on 27 July 1989.]

In the after-thought department I will add a few lines about the Advisory Council on College Chemistry. NSF had established such projects in several major scientific disciplines to improve instruction in the sciences. Our project was based at Stanford, and our staff people enjoyed cordial relationships with the chemistry department. We attended their weekly faculty meetings, for example.

I took a year's leave from Idaho in 1967-8 (when we had separated chemistry and physics), and I joined the AC3 staff. Dr. William B. Cook of Montana State University was director and his wife Merta was an extraordinarily effective business manager. The Cooks left relatively soon for Bill's deanship at Colorado State University, and William H. Eberhardt of Georgia Tech served temporarily as director, but Gordon Barrow of Case Western Reserve then was director until NSF dropped the funding for such programs.

This was a stimulating experience for me. The AC3 board members included the top chemical educators. They frequently came to Stanford for meetings with us. We all took part in conferences cross country and issued reports on the proceedings. The staff in my time included Dr. Arthur F. Isbell of Texas A & M, who had worked with me in General Mills. Also, there was Rod O'Connor, who was then in course of leaving Montana State University, Dr. Robert I. Walter of Haverford College, Dr. William F. Kieffer of the Colleges of Wooster, and Dr. Roger G. Gymer of Fort Lewis College.

One of my assignments was the development of the College of Chemistry Consultants Service (C3S). This provided a choice of thirty outstanding lecturers and administrators for visits to campuses on request to counsel departments and college "management" on such matters as curriculum, staff, and budgeting. When NSF cut off funding for AC3, I took what money was left in this project and by husbanding the resources succeeded in keeping the program going from Idaho for several years under the auspices of the ACS Division of Chemical Education. More recently, I served on the steering committee for a revival of C3S, chiefly as a help for minority colleges but with the goal of broader service for other institutions. This is now managed by the ACS Office of Education.

My experience in ACS generally has been gratifying, and at Idaho we encouraged our students to join the Society as affiliates, moving into full membership on graduating. Somehow over the years, starting in Du Pont days, I have attended most of the national meetings. Soon after that Teflon paper I moved through the offices of what was then the Paint, Varnishes and Plastics Division. Later my administrative turn took me into the Chemical Marketing and Economics Division, and I served in the offices of that division.

The most interesting meeting of that group in my time was in Kansas City where we had Harry Truman, recently out of the White House, as our luncheon speaker. This program had been arranged by a market development man employed by Union Carbide. (His name escapes me). He was startled to find that his company management was greatly displeased with his coup; he reportedly was told that this invitation was to be withdrawn "or else". He polled our divisional executive committee, who all backed him, and Harry did join us. (The arranger soon left Carbide, and it was my impression never again had quite so good a job.)

Back to the meeting, Linus Pauling was president of ACS, and many members of ACS were displeased by his friendliness with the Soviet Union as a move toward world peace. There had been at the time an aborted movement for impeaching Linus. When I was chairman of the Paint and Plastics Division one earnest member worked feverishly to get rid of Pauling despite my efforts to cool him. Pauling did come to the pre-luncheon social hour but had to leave the meeting for California before Harry talked. This led Harry to voice regrets. He had wanted to offer Dr. Pauling some good advice; "He should quit working on red corpuscles". This was in the period of Pauling's work on sickle-cell anemia.

A couple of years ago Pauling attended a NW Regional ACS meeting in Portland, and I had opportunity to remind him of this incident. He recalled with amusement that he hadn't learned of Truman's comment until getting off his plane in Los Angeles. Newspapers had picked up the story.

* * * * * *

Among honors which have come to me that don't fit into data sheets is this: I rank highly the dedication of books. I recall that on my retirement I received a dedication by Henry Eyring and Douglas Henderson of volume II in the series Theoretical Chemistry: Advances and Perspectives (25). Henderson, who is now employed by IBM was one of the first physicists I was able to hire. Eyring was his major professor at the University of Utah and was not pleased when his talented student decided to cast his lot with our developing program in physics.

I also was greatly pleased by Jean'ne M. Shreeve's dedication of Volume 24 of Inorganic Syntheses (26), on my 75th birthday. Jean'ne was then serving as head of chemistry at the University of Idaho and subsequently became director of our office of research and dean of graduate studies. Hiring Jean'ne, fresh from her Ph.D with George Cady at the University of Washington, surely is one of the best things I accomplished for our University.

My gratification in serving the University reached a peak when our Physical Sciences building was named Malcolm M. Renfrew Hall. We don't receive that kind of an honor very often without dying first.

SECOND ADDENDUM

[Added by Dr. Renfrew on 27 November 1989.]

In case you can use another afterthought, I will offer a note on the benefit of good personal contacts in professional advancement. Early in my return to Idaho it was evident that we crucially needed to expand our visits with Washington agencies (and

with other universities for recruiting), but little money was available for travel. Happily, Dr. Earl T. Hayes, an Idaho alumnus of my undergraduate days, paid me a visit. He was on leave from the U.S. Bureau of Mines for service as a civilian manager of the National Research Council's work for the Department of Defense on materials development. Earl thought that my experience in the plastics field could be used there beneficially, and he recommended my appointment as a member of the committee. The chairman of the committee at the time was Dr. C. S. Marvel, then at the University of Arizona. I had known Speed through his Du Pont consulting and ACS activities; and we shared an interest in bird watching.

Perhaps it was the latter that got me the appointment. At the time the Committee was mostly concerned with aluminum alloys for aircraft frames, and I contributed little, but the meetings took me to Washington twice yearly, and while there I could visit NSF and other agencies important to our developing physical science program. Also, I could stop off at universities along the way to contact young physicists. Knowing Hayes and Marvel turned out to be a great help in what I was trying to do at the University of Idaho. For this I still give thanks.

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