THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

ROBERT ARMSTRONG

Transcript of an Interview
Conducted by

James J. Bohning

at

Highland Beach, Florida

on

1 May 1986

Robert T. Armstrong JH 3/15/96

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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(Signature) Robert T. Armstrong

(Date) March 15, 1989

(Revised 24 February 1988)

ROBERT ARMSTRONG

1909 Born in Chadron, Nebraska on 27 December

Education

Massachusetts Institute of Technology
1931 S.B., chemistry,
1935 Ph.D., organic chemistry,

Professional Experience

Massachusetts Institute of Technology
1928-1935 Assistant Chemist
1935-1937 Instructor, organic chemistry
U.S. Rubber Company, Passiac, N.J.
1937-1941 Chemist
1941-1944 Group Leader
North American Rayon Corporation, Elizabethton, Tennessee
1944-1946 Research Chemist
Celanese Corporation of America
1946-1949 Group Leader
1949-1950 Technical Superintendent
1951-1952 Director, Technical Control
1952-1953 Associate Director of Research
1953-1956 Technical Director, Textile Division
1956-1966 Vice-President, Technical Director
1966-1975 Senior Vice-President, Research

ABSTRACT

Dr. Robert Armstrong describes his childhood in Nebraska and Arizona and how he managed to support himself through undergraduate and graduate studies at the Massachusetts Institute of Technology. He recollects the faculty at MIT and the support that some of his teachers gave him during his stay there. After graduate research, Armstrong moved to the U.S. Rubber Company, where he coupled investigations of rubber vulcanization with pioneering research on radical polymerization; he early recognized the value of systematic studies of copolymerization. During World War II he was persuaded to work at the North American Rayon Company and he briefly alludes to the conditions he found at their production plant. Soon after WWII, Armstrong started his career at the Celanese Corporation, which was to last until his retirement. He describes his functions as he progressed up the corporate ladder and also outlines his involvement with the establishment of the Research Triangle Institute.

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 chair of the Chemistry Department for sixteen years, and was appointed chair of the Department of Earth and Environmental Sciences in 1988. He has been associated with the development and management of the oral history program at the Beckman Center 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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LOCATION: Highland Beach, FL

DATE: 1 May 1986

BOHNING: Dr. Armstrong, you were born on 27 December 1909 in Chadron, Nebraska. Can you tell me something about your father and mother; their names and occupations.

ARMSTRONG: My father, William Dawson Armstrong, was born in Iowa. My mother was Alice Winifred Cole who came to Nebraska as a child and survived the Sioux wars, hidden in a cistern through many of them. I do not remember Nebraska during my childhood in as much as my family moved to Arizona in 1912, the year the state was admitted to the Union and there my earliest memories begin.

BOHNING: Did you have any brothers and sisters?

ARMSTRONG: I had one brother, Cole, three years older than I who was an MIT graduate in electrical engineering and ended his career with AT&T as chief engineer. Then he went to a government security group in Washington for his last few years. He died about ten to twelve years ago.

BOHNING: What did your father do?

ARMSTRONG: In Nebraska, my father was a county treasurer. But he was rather a ne'er-do-well character, always hunting the pot of gold at the end of the rainbow. His history was that he grew up on a farm with many older brothers and sisters who inherited the farm. He headed west and became a cow puncher, bringing cattle up from Mexico to the end of the railroad at Omaha. Then on to the Klondike and the gold rush. In keeping with that, he was hunting new goals and with a new state coming into the union, he went to Arizona, where he started a bank. There was a pretty thin population; at that time the population of Arizona was about one person per square mile. However, the bank foundered and so we homesteaded.

BOHNING: Where in Arizona?

ARMSTRONG: In the southeast corner, originally Bowie; we homesteaded eight miles east of Bowie at a section-head then called Olga on the Southern Pacific Rail line. My mother taught

school. It was a small schoolroom and a four mile walk each way. She taught all classes from first grade through high school. Since I couldn't be left at home alone, I had to enter the first grade when I was four years old.

BOHNING: How long did you have your mother as your teacher?

ARMSTRONG: I had no other teacher until we went back to Nebraska in 1918 so I would have been in sixth or seventh grade. My maternal grandmother was getting old and wanted her back there. So I then went to public school in Cody, Nebraska.

BOHNING: Where did you go to high school?

ARMSTRONG: The real answer is that I didn't. I went briefly for a month or two in Loveland, Colorado. My mother had died in Cody, Nebraska when I was twelve. Then I went for two years to a unique school. It was a prep school associated with what was then the Colorado Agricultural College at Fort Collins. They had a prep school called the Shorthorns designed for the children of farmers around the state; the school had a six month term so the children could work in the fields in the fall and spring. The curriculum offered was Agriculture and Mechanical Arts, the latter which I took included practical courses like forge, foundry, machine shop, pattern making, mechanical drawing, and things like that. Then my brother, who was attending the college, decided to go to MIT, so he went east. My father and I followed and I went to work in Boston as a stockroom boy at U.S. Rubber for several months, I guess for the better part of a year.

BOHNING: What year was that?

ARMSTRONG: That would have been 1925. Then I got the idea that I'd like to go back to school, so I went up to the State House and asked where I could go to school in Boston. They were aghast and said, "Well, why aren't you already in school?" They suggested that I go to Boston English High. That was in February of 1926. I took a rather heavy load of courses for the second term. At the end of the school year, they wanted a conference to decide what my course schedule might be for the following year. I remember replying, "Well I'm not going to be here. I'm going to MIT". I took MIT entrance exams and college boards and went to summer school. Lo and behold I was interviewed and got into MIT; the most serious problem being the square on the registration form which said `age' with a note saying that no one under the age of seventeen could be admitted. I was only sixteen but I wrote down seventeen. Then my conscience got the better of me and I wrote a six down over the seven and it ended up as a black blob. But they let me in anyway.

BOHNING: Were there any teachers, other than your mother, who influenced you in any fashion?

ARMSTRONG: Not that I'm aware of from those years. I was not interested in school. The word was; `Robert could, if he would only try.' I didn't have much interest and to this day, I have no idea of how it happened that I turned myself around and took an interest. Probably due to Max [Maxwell] Parshall who roomed with my brother. He ended up as a professor of chemistry at Fort Collins, Colorado. He's now retired and I haven't been in touch with him for many years. I think he encouraged me; I remember his bringing home little bottles of hydrochloric acid and so on. He would show me its action on metals. I kept them up on the roof of the building. I think that was the real stimulus.

BOHNING: Did you have any chemistry experience prior to entering MIT?

ARMSTRONG: No. None at all.

BOHNING: What about physics and math?

ARMSTRONG: As you gather I just didn't have proper high school. In Fort Collins they were vocational courses. Then I was only at Boston English High for a few months, where I had second term French but it was really quite hopeless. Anyway, I recall taking French, German, and solid geometry in the summer. At that time MIT gave summer courses for students who hoped to get admitted and would enter on conditions. There were two or three conditions that had to be cleared up my freshman year.

BOHNING: What was your brother studying?

ARMSTRONG: My brother was studying electrical engineering. He was in an industrial cooperation electrical communications course. So he worked alternating one term for AT&T and one term back at MIT during that period. I think he finished with the Masters degree in 1928 or 1929.

BOHNING: What goal did you have in mind when you entered MIT? Did you select chemistry when you entered?

ARMSTRONG: Yes. The classmate of my brother's was in chemistry and that's how I happened to do so. I registered in chemical

engineering to start with, but then I transferred to course five chemistry at the end of the first term.

BOHNING: Why did you switch from chemical engineering to chemistry?

ARMSTRONG: I suspect chemical engineering was an effort to be slightly different than Max Parshall and the recognition, when I got there, that science was just a little kick above engineering, after all. [laughter]

BOHNING: What were your courses that first year?

ARMSTRONG: Inorganic chemistry was the most important, of course. `Beaker' Joe [Joseph W.] Phelan is especially worth mentioning. Professor Phelan was a full professor teaching undergraduate inorganic chemistry; also there was H. Monmouth Smith teaching freshman undergraduate inorganic chemistry. I was in Phelan's classes and yet it seems to me the crucial thing is not so much that I remember what they taught me but that here was a boy with no money at all, hungry most of the time, and those two professors recognized it enough to dig into their own pockets and loan me money to get me through that freshman year. They felt responsible for me. When you think of the atmosphere today compared to that. This is extremely important: indeed they carried me along for a good many years, never at my request, but when they saw a crisis. When I left having graduated with a Ph.D. I still owed those two great professors probably over a thousand dollars apiece which in todays terms would be ten thousand. They never said a word until I had paid them both back, about five years after I finished. After the last payment both of them wrote me such nice notes.

BOHNING: That's amazing. I was going to ask you about your financial support and you just answered that. Did you take some math and physics courses then?

ARMSTRONG: Yes. We had regular freshman physics and indeed we had a special course in 1926. It was an extra course at eight o'clock in the morning given in the big lecture hall on the newest theory of the atomic structure; quite advanced for the time. Physics and of course calculus. I had to take such things as descriptive geometry, physical training--that was a killer, French, and German. Also, I remember a special course on resonance given by the then young Linus Pauling. But the real pleasure then was not in my freshman year but in my sophomore year when I could take what they called general studies at MIT. These were two hour a week courses that you really didn't get credit for except in an offhand way but they counted somehow. I

recall Professor [Archer T.] Robinson in English literature and Professor [Henry G.] Pearson on the appreciation of art. These were highlights of those early years, but the rest of it was strictly digging in the trenches.

BOHNING: What about organic chemistry?

ARMSTRONG: Undergraduate organic chemistry at MIT started in your junior year. I should precede that by saying that following my freshman year and still in need, of course, of money to eat, I was lucky enough to... Well, first of all when I shifted to course five, that required one summer session term of qualitative analysis, which presented a major financial problem. But having taken that, Professor [Leicester F.] Hamilton who was then head of analytical chemistry at MIT took me on as a laboratory assistant for the second term of qualitative analysis, so that I had a job. I taught qualitative analysis in the lab. By that time I had turned seventeen and in those days a lab assistant was a member of the faculty so I suspect that I was one of the youngest official faculty members MIT ever had, purely out of desperation to eat. [laughter]

Then during my second year I took quantitative analysis, inorganic chemistry and physical chemistry. I took organic chemistry in the third year and whilst remaining a lab assistant in analytical during summers, I then became a lab assistant of organic chemistry after I had the course. So I was always on the faculty from 1927 until 1937.

BOHNING: Who were the instructors in both physical and organic chemistry?

ARMSTRONG: Miles Sherrill taught physical chemistry and authored a book universally used then in physical chemistry (1). Professor Sam [Samuel P.] Mullikan was the head of the undergraduate organic chemistry department. The other professor at the time, Professor [H. W.] Underwood who wrote textbooks published at the time (2); [Ernest H.] Huntress and [Avery A.] Ashdown were other names at the time. Ashdown did his post-grad with Staudinger. After Beaker Joe Phelan, Professor Mullikan was the man that I looked to mostly and who was so very good to me. I think this might be of some interest. I suspect there aren't too many copies of them around. I happened to run across this. [Armstrong gives book to Bohning] Published in 1935, just fifty years ago. I was working for him synthesizing some compounds and working out his qualitative analyses. At the time this was an extremely important field and method of analyzing organic compounds. After all we didn't have mass spectroscopy and so on. I'll give that to you.

BOHNING: Thank you very much. Are you sure?

ARMSTRONG: Yes. I've kept practically no papers. It's a good place for it to be.

BOHNING: Did you do any research as an undergraduate?

ARMSTRONG: I think the most important research I did was a long project for the Wrigley Chewing Gum Company through Professor Underwood. We were seeking anti-oxidants for chewing gum. The chicle chewing gum in those days was all made out of gutta percha and it came in balls full of worms and bugs and filthy stuff. My job was to incorporate various compounds and put them in an air oven at seventy or eighty degrees and test the brittleness every day and write up reports. We found aromatic amines that retarded oxidation. I wouldn't be surprised if Wrigley Chewing Gum put those dreadful compounds in chewing gum in those days. This work was done at night for Professor Underwood, at $.20/hour. Also he had me carry out all the experiments in his proposed new edition - this was 1927-28 before I had organic chemistry - so it was a real test. [laughter] My undergraduate research, which I did at MIT as a thesis, had to do with a study of the isomerization of inorganic nitrites, also preparations of fluorine and sulfur hexafluoride.

BOHNING: Who was that with?

ARMSTRONG: It was with Professor [Walter C.] Schumb.

BOHNING: At this time the Depression came along. Did this have any effect?

ARMSTRONG: The Depression added more problems to my existing problems is all I can say. Having been hungry, I was just a little bit hungrier. I recall very well the bank holiday but that didn't pose any problems for me since I didn't have a bank account anyway. With all of the outside work; for a couple of years I looked after the telephone switchboard in the basement of a big doctor's office building in Boston where I had a bed. I kept track of twenty physicians from 7:00 p.m. to 7 a.m. and studied in between. Finally by the spring of 1930, I collapsed and was put in the infirmary. So I didn't graduate with my class in 1930; but I guess it was lucky because I might have taken the only job that was offered to the chemistry class of 1930. Standard Oil came in, I presume it was Standard of New Jersey, and guaranteed a job as a filling station attendant to any member of the class who wanted it. Not a single other job was available. These were hard times and they weren't much better

the following year when I did get my bachelors degree but good old Professor Mullikan kept me on as a lab assistant which paid twelve hundred dollars a year. That was at least as much as you could make in any other way and so I went on for the Ph.D. I think it was not out of intellectual ability but out of economic advantage.

BOHNING: You were there for four years for your Ph.D. work? You got your degree in 1935?

ARMSTRONG: Yes.

BOHNING: And your B.S. in 1931. Were you really out for almost a year because of your health?

ARMSTRONG: I was out one spring and summer but then returned the following fall. My work that summer was for the Department of Agriculture out in Colorado measuring water flow in the rivers working for Max Parshall's father and I got my health back pretty well. I came back, going after a Ph.D., and nothing remarkable about that except the choice of research topic. I really had leanings towards physical chemistry, probably because I wasn't all that good in math and wanted to be better. I got interested in spectroscopy and prepared cyclopropane and worked assiduously to get it to high purity. Then I went after the Raman and infrared spectra. Raman had come out within the previous three or four years and made his famous discovery. Cyclopropane was a simple molecule to study and the symmetry characteristics made it a rather elegant molecule for conformance to symmetry rules. This was with Gilbert King who was then taking his Ph.D. in physical chemistry. We published this work together (3).

[END OF TAPE, SIDE 1]

BOHNING: How did you select that topic?

ARMSTRONG: I have no idea now. I think the molecule was strictly my own idea and then I sought out Professor Ashdown to sponsor me in organic and Professor Louis Harris to sponsor me in physical. He had access to the big new spectrometer in the physics department. I don't think it was noteworthy work. E. Bright Wilson was very interested. He was anxious to get the results and I remember being very chary at this fellow who wanted my data before I got it published. [laughter] Over the years I've found that that is not an uncommon situation.

BOHNING: Did you continue in support of yourself? Were you still a lab assistant and still on the faculty?

ARMSTRONG: I was always a lab assistant until I got my Ph.D. and then I was an instructor in chemistry for two years after that. As a lab assistant I did not pay tuition - then one hundred fifty dollars per year - or I could not have made it.

BOHNING: Given the economic climate as you approached the end of your Ph.D. work, were you considering looking at career options at that point before you decided to stay on.

ARMSTRONG: I think I had planned to teach but the economics at the time were pretty rough. An instructor's top salary was eighteen hundred dollars so you really had to do a lot of industrial consulting work to survive. I recall a conversation with Professor Frederick Keyes, in the spring of 1937. He was head of the department and I recall his counseling with me, going over every assistant and associate professorship and trying to count how many years before there would be an opening. It looked like it was a good five to seven years. Of course we didn't know there was a war coming then. I think it was also partly a nice easy way to let me down. I wasn't all that good anyway. He was a kind fellow and perhaps it was a nice way of suggesting that it probably would be a good idea if I got a job in industry. In any event, that was the turning point and I turned to industry.

BOHNING: But you did stay on two years as an instructor after you completed your Ph.D.?

ARMSTRONG: Yes.

BOHNING: What did you do during that two year period?

ARMSTRONG: I kept on with study of cyclopropane and propylene. I don't know why isomers always had a fascination for me. Here are the publications. I see only one paper published in 1938, "Influence of Branched Chains on Optical Activity"(4). For the life of me I've forgotten what that was. But the other publication was in 1936. It doesn't look like I did very much. I really liked classroom teaching. In 1936 I was probably writing up the papers and polishing them up. Then, in 1937, I left.

BOHNING: From MIT in 1937 you went to U.S. Rubber. How did that move occur?

ARMSTRONG: I was rebellious at the time and wouldn't consider going with a company like Du Pont or Eastman from whom I had offers. It's a little hard to recreate my state of mind at the time. Perhaps I was wise. Maybe the competition was too much there. I was attracted to U.S. Rubber mostly by one man, Dr. Roscoe Gerke, who was an extraordinary research manager. He had assembled a very competent group of people in Passaic, New Jersey at their central research laboratory; utterly out of proportion to the quality of the company in general. It was through Dr. Gerke and his boss, Dr. Willis Gibbons, the director of research. They did a remarkable job. I was attracted to them and accepted a job there that paid two hundred dollars a month. That was the top salary they had ever given to an incoming employee at the laboratory. Dr. Gerke placed my desk beside his in this large laboratory. It was a converted factory with open bays and just a few partitions. He called me over to participate on every conference on any subject, so I quickly had a grasp of the whole business of the company. He let me organize and do any research that I wanted to do, and that's how I got interested in the chemistry of vulcanization.

BOHNING: Were there other Ph.D.s in this group?

ARMSTRONG: At that time, we had Dr. [John R.] Ruhoff who subsequently went with Mallinckrodt; Dr. [Melvin] Mooney, a physicist of some standing at the time, a classical physicist. Dr. [Hugh M.] Smallwood was from [Johns] Hopkins and also was a physical chemist. These three, other than Gerke, were the main strength at the time that I went there in 1937. [It wasn't long before] we were plunged pretty rapidly into the GRS program because of the war. As soon as that was wrapped up and got out into the pilot plant, then the management of U.S. Rubber said that they must have a nylon because Du Pont had just announced their introduction of nylon. I believe they said that we could spend up to a million dollars if we got them a nylon. That's where we took off with building a basic and an applied research group.

BOHNING: How many years would you have been there at this stage?

ARMSTRONG: I went there in 1937. My recollection would be that I worked simultaneously on sulfur/olefin reactions and on butadiene polymerization almost from the beginning. But the butadiene studies took precedence by 1939 certainly, maybe 1938. In that period I recall having several sessions with Bill [William O.] Baker over at Murray Hill [Bell Laboratories] who was working on polyesters as coatings and insulation materials and getting his counsel. We were trying to extract from the German patents all the information we could get in order to get on with the synthetic rubber program and Bill Baker helped us a

little with that. The process that we developed there moved into pilot plant by about 1940. When it went to the pilot plant, we in research were faced with a rather tough situation in that management said they wanted the nylon. For us young fellows this was a bit of a trying time, although it bothered some of us more than others. Nevertheless, this was when I was given the job of putting this target group effort together.

My first step was to go out to Kharasch at the University of Chicago and interview people there. One of those that I interviewed was Frank Mayo; I offered him a job and he accepted. I must say it wasn't that simple in those days. Frank Mayo came out to Passaic and candidates were grilled as they had done when I went for interview. That's one reason why I went there. The interview was at least the equivalent of the doctorate oral; it was very thorough. Frank put together the fundamental group. I hired Cheves Walling, and Fred [Frederick M.] Lewis. It was a small group in modern terms but a large group for those days. I hired Pliny Tawney from Sherwin-Williams against the advice of Gerke and put him in charge of the applied group. So we had two small groups at work. Unfortunately, both Gibbons, the director, and Gerke were requisitioned away for government munitions plant management. They each went to a different naval ordinance plant. I helped them for a good six months in recruiting for their plant needs. Then I came back and kept in touch with the group. However, it's hard to say in retrospect. I left or really was fired at issue over the allocation of resources between the applied group and the fundamental group. I had not learned by then that you had to build a power structure if you were to survive in this world. It was all my innocence of political manipulation. So I walked away from it and volunteered for the Navy. I was accepted but somehow the papers got held up someplace and suddenly I was asked if I would go to Tennessee and help build a rayon tire cord plant in a place where they knew nothing about tire cord. My rubber experience had carried through.

BOHNING: To go back to your U.S. Rubber days; did you hire Cheves Walling too or did Mayo hire him?

ARMSTRONG: Mayo hired Cheves. I assume I must have interviewed Cheves. It was a joint process but my impression was that it was Mayo's job to form his own group.

BOHNING: What was your responsibility with the fellow men on the applied group?

ARMSTRONG: At this time, Gerke was one of either two or three department heads at the Central Research Laboratories. I was an assistant department head for research, distinct from

development. They didn't call them sections and groups at that time.

BOHNING: Were you still involved in laboratory work?

ARMSTRONG: I was still very much doing laboratory work. I was puttering around with my sulfur/olefin chemistry, fitting in lab work while managing the group. I was assigned all kinds of additional work like being in charge of rubber chemical liaison development with The Naugatuck Chemical Company. I was working on anti-oxidants, and accelerators as part of the total research effort. I guess I did that mostly by myself but I never let rubber chemical work get into the two groups. They were to get a new nylon and weren't to be bothered with anything else.

BOHNING: Did you run interference for them with management?

ARMSTRONG: Indeed I did. This was the trouble. When the mature management that had built the U.S. Rubber research laboratory was called away, the second strings, Ernie Hazell and Dr. [Hubert F.] Jordan became directors and Mr. [William P.] ter Horst became department head. These were people who had no sympathy whatever for fundamental research and were seeking agricultural chemicals; indeed they turned over the laboratory for that. They couldn't get rid of the fundamental group but as soon as an issue came up such as the allocation of analytical time, between the applied and the fundamental group that's where I had the battle. I lost and so I said, "I'm out."

BOHNING: What year was that?

ARMSTRONG: That would have been 1944.

BOHNING: How much interaction did you have with Mayo and Walling?

ARMSTRONG: Scientifically speaking, I may be too humble but I think they were both so far ahead of me in their training that I didn't presume to give any guidance to them whatever. Unfortunately, perhaps, because Frank made polyethylene and Pliny made polyethylene terephthalate, neither of which were pursued for lack of personnel with specialized evaluation experience. Frank wrote to me in 1947 and said that he had sent off a bunch of papers on copolymerization saying, [Armstrong quotes] "and Mr. Hazell brought up the point that you should have credit for initiating the work at the general laboratories, a point which I would not dispute. I pointed out, however, that you should have

had the acknowledgment in the first paper, three years ago, and the credit might look peculiar in the fourth paper of the series, and that at the suggestion of Tawney and Terry all names except Mooney's were removed from the acknowledgment in the first paper. It was agreed that the enclosed statement would be sent to the Journal and that on the remote chance that you would for some reason request it, it could be deleted in proof." (5) The acknowledgment was, "The inception of work on copolymerization in these laboratories is largely due to the early decision by Dr. Robert T. Armstrong that a study of copolymerization would be one of the best approaches to a fundamental understanding of polymerization as a whole, a decision which we feel has been amply justified." Whether that ever appeared in the Journal, [of the American Chemical Society] I have no idea. I've never seen it so I wouldn't know. What I'm saying is that I was running interference for them, not directing them technically by that point.

BOHNING: You also got the whole group going in the first place back in the early 1940s.

ARMSTRONG: Under Dr. Gerke's pushing and support and defense. It was my first management job and I muffed it pretty badly in the end. But not so badly that Mayo didn't carry on for quite a few years.

BOHNING: Did you have some sort of contact with Mayo after that? Did you keep in touch with what was happening?

ARMSTRONG: No. I've been a loner all of my life. I've seen Mayo a couple of times since then. I can't remember the circumstances. That whole group got together in February of 1968. Cheves wrote a note that said, "Dear Bob, the only trouble with this splendid occasion is that you couldn't be here too. After all you had a lot to do with getting it all started."

BOHNING: Is there anything else we should add about that particular period?

ARMSTRONG: I don't think so.

BOHNING: The difficulties were compounded by the war and the top people leaving.

ARMSTRONG: Yes. There was a very uncertain feeling by people of that age of working on something for post-war commercialization. I didn't feel very right about it.

BOHNING: You said you tried to join the Navy.

ARMSTRONG: Yes. I went for my physical exam in Newark Armory. I'll never forget that. There was a big sign saying report to the examiner if you've had any of the following, and among them was sinus. So I said, "Yeah, I've had some sinus problems." "Okay bud, we'll give you a credit: Approved." [laughter]

BOHNING: How did the North American Rayon situation develop?

ARMSTRONG: I didn't know anything about power structures and elementary human relations such that a lad might get in a liberal arts college; I was a technician pure and simple. So I went down to Elizabethton, Tennessee and carried on some modest laboratory work while counseling and consulting with other producers of rayon regarding the building, the design, and so on of this plant. The government required other producers to pool knowledge because we had to have more rayon tire cord production in a hurry to go with the synthetic rubber. However, the old textile fiber end of the plant was in such bad shape it wouldn't operate successfully. It was the North American Rayon Company, a German company which had been seized by our Alien Property Custodian who had put in a New York financial type as president and had left the German management in the plant. The new tire-cord plant operated fairly well but the textile fiber end was down most of the time.

[END OF TAPE, SIDE 2]

I finally traced down the reason and on the morning I came in for my report presentation, the plant operated fine. It took a solid year of work to trace it down. There was nothing I could do about it and so I said, "I'm getting out of here."

BOHNING: How many Germans were left? Were they running the plant?

ARMSTRONG: Yes. The plant manager was a German and I think he was not even an American citizen. The technical head was a German-American citizen through his American wife. I never could be sure that the technical head knew what he had been party to. It was the manager of the plant who ordered the correction the morning I brought the charts in. It was a rigid Germanic atmosphere and I couldn't get back into the section where they adding the wetting agent. They were withholding it so that the filaments would keep breaking down.

BOHNING: The people in New York were not even concerned about it?

ARMSTRONG: No.

BOHNING: I'm still curious to know how you got from trying to enlist in the Navy to North American Rayon.

ARMSTRONG: All I know is that I received a very official looking document asking me to interview the president of North American Rayon in New York. I had no idea how it went from the Navy to the Alien Property Custodian to this company. The synthetic rubber connection undoubtedly was responsible for the government pushing it that way. I got a song and dance about how I had no business going into the Navy and should apply my talents here.

BOHNING: I see. The Navy had all of your background in their files.

ARMSTRONG: That's right.

BOHNING: When you decided to leave North American Rayon did you have a place to go to at that time? It seemed to me that you had some patents while you were there.

ARMSTRONG: I think most of my patents were with U.S. Rubber (6). I don't have a list of them. Maybe there were half a dozen. Nothing of any great consequence. There might be one or two patents from North American Rayon.

I think a colleague at North American who had worked at Celanese previously passed word along to somebody at Celanese who then invited me up to New York for an interview. I didn't look around for a job. I was ready to move and chatted with this fellow. Being of this turn of mind, having no image of great competence of myself, I thought I could do alright at Celanese, as they were so lousy that I ought to do well there. [laughter]

BOHNING: What was your first position at Celanese? Was that in 1946?

ARMSTRONG: Yes. I was hired in the New York office oddly enough by the assistant to the technical director and sent to Cumberland, Maryland which was the oldest of their plants. I was

put in a spinning pilot plant for six months and then was made manager of the pilot plant and did quite interesting work. I got credit for inventing pigmented yarn, new filtration systems, you name it. All of the typical routine industrial things and at that level, I was pretty competent at that kind of thing. Right after the war, there was a business shock for acetate fiber, particularly. Or really, what had happened was that during the war, products were so short that the plant got away with anything. Quality was unbelievably bad. The bribery I'm sure was rife between purchasers and the supplier; price controls and all of the evils that go with that kind of a situation. The moment that the economy settled down, the purchasers wanted no part of us and so the plant collapsed. That was soon after I arrived, within about a year or so. By that time I knew enough to know how the plant should run and in the course of a few months got them started and rolling again. Then they put me in quality control and I went to their new plant as their superintendent of quality control. I was briefly there in Narrows, Virginia and then I became director of quality control for the whole corporation. That was when they sent me to New York.

BOHNING: What year was that?

ARMSTRONG: 1951.

BOHNING: What does the director of quality control do?

ARMSTRONG: We never had one before so I put a superintendent of quality control in every plant in the textile division and in the major plants of the chemical division. By this time I was learning a little bit about how to deal with people and the need to build a power structure. I think today it's called networking. After much harassment, the plant managers would give way and let me have a voice in what they could ship and couldn't ship. We did a good job for several years there until a new way of life evolved and we didn't need it anymore. Then I went over as associate director of research at Celanese Laboratories in Summit, New Jersey.

BOHNING: Who was responsible for that change? Did you indicate that a director of quality control was no longer needed?

ARMSTRONG: No. It wasn't that simple. My assistant was made director and it sort of lingered on for a few years. George Schneider, who had been technical head of the company since its inception along with Dr. Camille Dreyfus, wanted me over at the Central Research Laboratory and put me there as associate director briefly. Then, almost simultaneously, the company

separated the textile division out as a different operating division. So very quickly I moved out of Summit to Charlotte as technical director of the textile division.

BOHNING: That would be 1953?

ARMSTRONG: Yes. That sounds right.

BOHNING: What were your responsibilities there?

ARMSTRONG: Continuing quality control for the division and plant development. We put a superintendent of development in each plant reporting to me for technical guidance. So I had my power structure again for quality control and development. The technical end of plant operations was really what my function was and I started up a big pilot plant operation for new product development. I also cleaned out the basement of the new office building. I put in applications and product development. The whole textile operation was put down there to develop a more technical base for the marketing of the textile division. This was still in Charlotte, North Carolina. On the whole, this was pretty satisfying and rewarding in terms of a sense of accomplishment and making it a profitable division. That lasted about three years.

BOHNING: Through 1956?

ARMSTRONG: Yes. It was while I was down there that I got involved with Luther Hodges who was governor of North Carolina then. He approached me through Romeo Guest. He had ideas about trying to get more technically oriented activities in the state. They had pretty good colleges, Duke University and U.N.C. Chapel Hill, but their graduates were getting jobs out of state. I spent a lot of time with him on this Research Triangle project. It was pretty well planned and finished when I was moved to New York.

BOHNING: How many years were you involved with the Research Triangle?

ARMSTRONG: I'm still a lifetime governor of the Research Triangle Institute. I've never been off of it.

BOHNING: Who else was involved?

ARMSTRONG: George Simpson, who is now still the head of education at the state level in Georgia, was the real intellect. He was a young instructor or assistant professor of sociology at Chapel Hill. Luther Hodges got him released from the university for a year to work on this. It's his intellectual child really. My impression at the time was of a group of people in the Durham-Chapel Hill area, Duke medical people and so on, who were enormously attractive, eager people. They were ambitious for their community and it was a perfectly delightful situation that existed there. I say all that because with overwhelming success, it doesn't have that flavor anymore. But it was everything about the community. It didn't have the southern drawbacks at all. It was an enormously eager, open-minded society there and that's why it was successful. It was just the right time and the right people and the right place that made it work. Watts Hill was the financial father, along with many big financial people of that area that Luther Hodges got into the project. I'm talking only about the technical side of it. There was a big business side of accumulating several thousand acres of land which they would parcel out and manage. The Research Triangle Institute became the part where my focus was.

BOHNING: Were you doing this as an individual or for the company?

ARMSTRONG: No, it was as an individual. The company never got really involved until later on. I was in New York and the whole program was going pretty slowly. It's a little hard to go back and analyze all of the motivations involved. At the time I didn't express it that way, but my intellectual justification for the course I took was that the South had been good to the Celanese Corporation. They had plants in Cumberland, Maryland; Narrows, Virginia; and Rock Hill, South Carolina; the whole financial base of the corporation. They had ten to fifteen thousand employees in the southern area. So I found myself in New York as technical head and as a director of the Camille and Henry Dreyfus Foundation. It seemed appropriate at the time to persuade the Foundation to aid and abet this Research Triangle effort. The directors agreed and approved a commitment of two and a half million dollars for a Dreyfus Research Laboratory in the institute. This gave an enormous boost to the whole undertaking because somebody had to go first. Then Chemstrand came in and built their research laboratory there. The reason I'm taking a little time on this is because it was a second effort to build a basic polymer research group. And it didn't work. George Herbert, who is still president of the Research Triangle Institute, prepared a history about a year ago of it and I have a copy of that that you can have. George ended his letter by saying, "Knowing your reaction to such thing. I do not expect you to be anything but displeased." [laughter]

BOHNING: Then you moved back to New York in what year?

ARMSTRONG: 1956; and stayed on. There I got Jerry Wiesner on the Celanese Board and together we got the base funding for the Dreyfus Building at MIT out of the Foundation. Tried to pay back some of my debt to MIT.

BOHNING: In 1966 you became the Senior Vice President.

ARMSTRONG: I was always fairly uncomfortable with line-administrative responsibilities. So they were good enough to create a Vice President; Technical Director, so I could be Senior Vice President, Research and wouldn't have to report to anybody except my secretary and the President. I spent those years, I hate to say, not involved in science or technology because I really was staff assistant to the President. From a practical standpoint it was dealing with partners; ICI in England who I persuaded to join us in a joint venture which became Fiber Industries. In seven years we passed a billion dollars in sales. And with Japanese and German partners in plastics and fibers. I was put in charge of the newly acquired Champion Petroleum for the brief year or so that we had it.

BOHNING: That's a very fascinating account. Is there anything else that we haven't touched on that you would like to add?

ARMSTRONG: I think we've covered my life history. I have not kept in touch with my colleagues. I don't know why. Both my wife and I are loners. Over the last twenty-five years, I've been helping her with her sculpture. I keep up with reading Science primarily. That's about the only way I keep up. We both ride horses four or five days a week. I keep my horse at the home of a professor of astrophysics. We're in the process now of moving back to Vermont. I think with age I take a fairly dim view of the world. I thought two sad things I've seen this week were the Priestley Medal address and Erich Bloch's analysis of R&D in the United States.

BOHNING: In what way?

ARMSTRONG: I guess what I really object to is that I never liked the power structure that I referred to. I dislike the old boy network. I think I can distinguish between the men of substance and those who hang on. Unfortunately, like in most other aspects of our society, the shortage of resources for science is very much the making of scientists themselves, as in other fields. Medicine is much worse than science. Medicine has run completely wild; these professions must clean their own houses up if society is to survive and be meaningful.

BOHNING: I want to thank you very much for taking your time and sharing it with us. I appreciate the information that you gave us.

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