THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

# CALVIN S. FULLER

Transcript of an Interview
conducted by

James J. Bohning

at

Vero Beach, Florida

on

29 April 1986

Fuller

Calvin S. Fuller THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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# CALVIN S. FULLER

1902 Born in Chicago, Illinois on 25 May

## Education

1926 B.S., chemistry, University of Chicago
1929 Ph.D., physical chemistry, University of Chicago

## Professional Experience

1920-1922 Analyst, General Chemical Company, Chicago
1924-1930 Chicago Tribune
1930-1942 Physical Chemist, Bell Telephone Laboratories
1942-1944 Chief, Polymer Research, Office of the Rubber Director

Bell Laboratories
1944-1950 Plastics Chemist
1950-1967 Chemical Physicist
1945-1950 Consultant, Research and Development Board, Department of Defense

## Honors

1956 John Scott Medal, City of Philadelphia
1963 John Price Wetherill Medal, Franklin Institute
1964 Fellow, American Association for the Advancement of Science
1981 Krupp Prize
1985 Photovoltaic Founders Award, IEEE

## ABSTRACT

In this interview Dr. Fuller traces his early years and the development of his interests in radio and in chemistry. Encouraged by an outstanding high school teacher, Fuller wins a scholarship to the University of Chicago. Economic pressures force him to break studies for periods of employment in the analytical laboratories of the General Chemical Company and as a photoengraver at the Chicago Tribune, but Fuller persists with his studies and completes his doctorate under W. D. Harkins. Dr. Fuller enlivens the interview with recollections of Harkins and Julius Steiglitz. Appointment as a research chemist under R. R. Williams at Bell Laboratories introduces Calvin Fuller to the infant science of synthetic polymers and to x-ray crystallography. World War II sees Fuller in Washington, D.C. heading polymer chemistry research as part of the synthetic rubber program. On return to Bell Laboratories after the war, Fuller decides to move to solid state chemistry and describes his work on semiconductors, leading to the development of the photvoltaic cell.

## 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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INTERVIEWER: James J. Bohning

LOCATION: Vero Beach, Florida

DATE: 29 April 1986

BOHNING: Dr. Fuller, you were born on May 25th, 1902 in Chicago. Can you tell me about your father, Julius, and your mother Bessie?

FULLER: My father was one of six children, five boys and one girl - a moderate income family. My mother's name was Souther - also a large family. They were six boys and one girl, the girl being my mother, who preferred the name of "Bess". In spite of similar backgrounds, both families came from England as early settlers and shared similar political beliefs, there were striking contrasts. The Fullers were inclined to be literary and reflective whereas the Souther's were more practical, more tuned to everyday concerns and proper etiquette. The Southers did much better financially. This is an interesting distinction to me because, although I was closer to the Southers and spent more time with them, I was able to compare these two families over a period of almost 20 years. Two of my uncles, one in each family, were to have considerable influence on my later life.

My father was a bookkeeper and was very good at numbers. In those days there were no computers, so my father served essentially as a human one. He tried to teach me how to do rapid arithmetic calculations and various tricks with numbers but I have forgotten these now. My mother was a tomboy as a young girl. After graduating from Calumet High School she was sent to the Art Institute in Chicago to study painting and design. She painted in oil and made designs on fine china which was baked in a kiln. She sold both dining and bedroom sets to the Souther's friends because she was married at 18 and from then on the family frequently needed extra cash.

Both my mother and father were dedicated to the welfare of my sister June and me. Besides our nutritional needs, which were well attended to, we were instructed daily on good manners and proper speech. My father was an avid reader and never tired of reading to us. However, it was my youngest uncle - he was only four years older than I - who had the greatest influence on my development up to the time I became a sophomore in high school.

BOHNING: What was his name?

FULLER: His name was Norman G. Souther. Norm never graduated from high school. That was too bad because he was very bright. He gobbled up all he could read about technical subjects and should have gone on with his formal training. But in spite of that, Norm became a radio ham and early led me into the mysteries of wireless.

Norm was a most unusual boy. Although he played with other children he seldom engaged in sports or other games. He would rather play with his cat whiskers and galena crystals. But he also raised and sold baby chicks. If there was anything wrong with Norm, it was that he was overly serious in everything he did. Years later he operated a radio store in Hamilton Park. After that he established one of the first radio technician schools located in Kansas City. Once he flew in a plane which served as navigating guide to Wiley Post on one of his trips. It is said that Norm was the first to receive such radio navigation signals. On that flight to Minneapolis he was in an open cockpit and nearly froze.

Because of Norm I also became involved in radio and cat's whiskers. I became inoculated with the same bug as Norm and was delighted when he could be caught in his lab tinkering. I think it is an interesting coincidence that almost 40 years later at the Bell Labs I was to return to the probing of semiconductor crystals, much purer ones to be sure, in connection with our solid-state investigations of the 1950s and 1960s.

In those early days Norm and I were finding hot-spots on galena and iron pyrites crystals with phosphor bronze wires in order to get the time signals broadcast from Arlington, Virginia. Later on, we communicated (using Morse code) with other hams, exchanging information on our sets. The detectors were improved. We built loose-couplers and other devices from telephone parts in the attic belonging to Norm's older brother, Sid, who had put up the big antenna in the backyard with Norm. Sid, who was also my uncle, had gone to the Armour Institute (now the Illinois Institute of Technology). Although a graduate electrical engineer he was not as much interested in radio (wireless) as he was in telephony, because he had gone into the business and that absorbed him full time. Without a doubt Sid had inspired his brother Norm in his technical bent. Who knows where the spark that initiates the interest in science arises? My grandfather on my mother's side, Calvin Nathaniel Souther, himself started out at 18 years old as a telegrapher for the Chicago, Milwaukee and St. Paul railroad in Milwaukee, Wisconsin. So he may have passed some of his expertise on to his offspring. Maybe Norman inherited the key we used in our wireless from him, who knows?

BOHNING: Did you go to school in Chicago?

FULLER: When I was about five we moved away from the house, directly across from the Souther's on Emerald Avenue, probably 7645 Emerald Avenue (the house in which I was most likely born), to a newly-built apartment building about one mile from my grandfolks in a section called Hamilton Park. The address was 7300 Harvard Avenue. It was only one block from the public park (Hamilton Park) which came to mean so much to me.

BOHNING: Were there any teachers there that had any influence on you at that early stage?

FULLER: Yes. I attended Harvard Grammar School located at 75th and Harvard Ave. We had excellent teachers but they were not responsible for my interest in science. This came later in high school. They were a very capable group of teachers. Back in those days, it was common for a woman, who was a housewife to teach school. They usually lived near the school so they had time to go home and make supper. They were well educated women teachers. There were no men teachers in our grammar school.

Nor did I receive any science influence at home. I was influenced a great deal by the excellent (for those days) technical book shelf in our branch library nearby. Early ability to read was probably a big factor in making the technical books available to me. My father was interested in literature and wrote fair poetry. He went through high school, as did my mother, and he read widely of the classics, no doubt influenced by his mother, my grandmother Fuller, who also was an avid reader.

My interest in chemistry was developed independently of my uncle for the most part. That happened more through my own experimentation, beginning in 7th or 8th grade. Chemical reactions were always fascinating to me and I often mixed chemicals in our medicine chest to see what might be produced. Explosives like potassium chlorate and sulfur, homemade gunpowder, zinc and sulfur for rockets, and nitrogen iodide crystals were always interesting, not to forget calcium carbide. From high school chemistry on I always had some sort of a chemistry lab space in the basement. But my early interest in physics and electrical experimentation was with Norm.

BOHNING: When did you start helping your Uncle Norman? How early was that?

FULLER: I guess when I was in the fifth or sixth grade, about 1912. This probably continued up until my sophomore or junior year in high school. I went back and forth between my home and my grandfolks home often during those years, 1910 to 1920. I spent a lot of time there simply because I found it more interesting. There were also some influences from my friends at our new location. I started to play games with the kids on the street. There were Swedish, German, Scottish, Irish and English kids in our neighborhood. So we had a great variety of youngsters to play with. Some were a bad and some were a good influence. We formed quite a gang on that street. Incidentally, another event which boosted my interest in chemistry was the annual fireworks display put on by my grandfather, who, because of his position with the railroad, received gifts of fireworks from wealthy Chinese who he had helped with transportation when the Chinese were seeking refuge in our country. Huge boxes would arrive several days before the Fourth. Each box would contain a different variety of device, from long strings of firecrackers, an assortment of pinwheels, varieties of flower-pots, rockets, Roman candles, to a finale of a large burning image or cross.

BOHNING: This was your grandfather Souther?

FULLER: Yes. He would put on a display in his spacious yard every Fourth of July which attracted many of the residents of Auburn Park, as that part of Chicago was called. Those displays had quite an effect on me too. So chemistry seemed more exciting to me in many ways than wireless or automobile engines, although things reversed later on when I really got to study physics as a junior at Parker High School.

BOHNING: Was that prior to taking chemistry in high school?

FULLER: Yes. I took chemistry as a senior. We had chemistry "sets" in those days which were much better in many ways than the chemistry sets on sale today. Those were the times of "buyer beware" and many dangerous chemicals were allowed; so more exciting experiments were possible. A few years ago, I was helping one of my grandsons do experiments with his chemistry set and I was amazed at the restrictions imposed. I doubt whether I would have persevered long with a modern set.

BOHNING: Were you doing any scientific reading?

FULLER: Not really scientific, but I did enjoy the popular science type of reading when I was in this stage of my life. I was not looking for fiction, although I did read some Sherlock Holmes and books of that sort. But I was mainly interested in the new books on the engineering shelf, the ones that carried the electrical experiments. There was a magazine (I believe, called The Electrical Experimenter, published by Gernsbach) which had rather fantastically presented subject matter, but it did have instructive parts too. Norman subscribed to it, as well as to radio publications. They were accessible to me through him, but I cannot name them now. Most of our interest was in what parts we could buy with Norm's money; we used a lot of enameled wire.

BOHNING: Where did you go to high school?

FULLER: I went to Francis Parker High School at Sixty-eighth Street and Stewart Avenue. It occupied quite a campus because the same buildings were used by the Chicago Normal College. It was a small high school by the standards of 1916, when I went there. The graduating class was probably about 50 at that time. We did have excellent instructors, some from the college. When we got into the junior and senior years, the science teaching was probably better than any place in Chicago. We had a Dr. Wigger in chemistry who was a Ph.D. from Northwestern. He was a very serious person and a good instructor. He was not excessively hard on the students and I think some took advantage of him. Wigger also coached the football team. The tough one was the physics teacher. She was responsible for my going to college.

BOHNING: What was her name?

FULLER: Mabel Walbridge. Her father was a professor at Cornell University, I think in humanities - not in science. As a young girl she lived near the Cornell campus. She was in her late fifties or early sixties when she was teaching physics at Parker High. She had a masters degree in physics from the University of Chicago and had taken courses from Michelson, Millikan and a few other professors of renown. She was a very demanding teacher and was not popular with the students because of the stiff homework she assigned. For me, she was really a windfall because I wouldn't have gone to college if it hadn't been for her. In my senior year it was decided that four students would take the University of Chicago competitive exams in science and math. These were given every year for entries from colleges and universities all over the Middle West. They provided full tuition for the first year and, if you were among the top twenty-five students in your class at the university, for the following three years. Mabel Walbridge had two students to enter in physics. I was one and a fellow by the name of Philip Rudnick was the other. Luckily for me Phil (who was good in physics and math) decided to go for the scholarship in math. But what Mabel Walbridge did for me was beyond the call of duty - she tutored me nights on her own time, so that when I took the exam I was prepared for almost every question. Phil won his scholarship in math. I came in tied with a boy from Indiana. My luck held again because for some reason he dropped out and I got the scholarship in physics. But I guess I am getting ahead of my story.

BOHNING: What year did you finish high school and when did you start working at the General Chemical Company?

FULLER: I graduated from Parker High in the winter of 1920. Immediately after that (in February, 1920) I was ready to go to work. My family was in need of my help. My sister, June, who was a year or two behind me in the same high school, had a girl friend whose father, Mr. Mattison, worked for the General Chemical Company. He was chief engineer of the Calumet plant in Hegewisch, Illinois, south of Chicago. The Calumet plant was a heavy chemicals plant turning out large quantities of sulfuric, nitric, hydrochloric acid, and zinc chloride, sodium sulfide, and salt cake, i.e., sodium hydrogen sulfate.

I do not remember the details of how I got the job, but I know that I must have impressed Mr. Mattison with my interest in technical things. Besides, I think his daughters told him about my lab in the basement at our Princeton address where we rented. That was not a chem lab but an electrical workshop copied after Norm's. I had microphones, receivers, transformers, batteries, capacitors etc. Later, after joining General Chemical, I did have a chemical lab. I could get small amounts of regents then, so I had some nicely labeled bottles and some simple chemical glassware. We had moved and were now living at the 73rd Street address - almost next door to my other lab.

The experiences I had that spring and summer of 1920 in my first real chemical job had a big effect on me. Again it was in large part because of one person - a fellow named Albert W. Wahlgren. Wahlgren was about 30 years of age and was soon to become head of the analytical lab consisting of about five chemists and two of us assistants. The present head was a middle-aged fellow named Fahrbach who, aside from approving our analysis forms, seemed to be mainly engaged in testing for methyl alcohol and fusel oils in the ethyl alcohol beverages of his friends - it being the time of Prohibition!

Wahlgren was a self-educated fellow who was dedicated to his job and was now head of the analytical lab, for Fahrbach had disappeared. I was new and Wahlgren immediately made me feel at home. Pleasantly, but firmly, he explained the seriousness of the work of analysis. I had had no previous experience at all in analysis but knew quite a bit about chemical reactions. The philosophy behind analysis, the need for precision, sources of error, cleanliness were all carefully explained. All of this was a great help when I got to these subjects again at the University.

Heavily tutored by Mabel Walbridge, I had taken the competitive exam in physics at the University of Chicago early in 1920. News that I had won a year's scholarship came to me that spring. That meant that I would be able to leave my job October 1st and become a real college student. It also meant that I would lose 15 dollars every week.

So in October 1920, I went to the University. I stayed there for two years because I won the scholarship the second year also. I took all the courses I could get in chemistry, physics and math during those two years. I got permission to do this because I figured that I would only go to college for two years and I wanted to get as much science as possible. I couldn't graduate with that, but I could get by with it. I didn't win the scholarship for the third year. There were no loans in those days, so in 1922 I was forced to go back to work.

BOHNING: Who were some of your instructors?

FULLER: [Julius] Stieglitz, who wrote the qualitative text (1), which was a real modern text then and was very popular all over the country. I had [Herman J.] Schlesinger in inorganic chemistry. He too, was well-known. I had [J. W. E.] Glattfeld in organic chemistry. Mary Rising was one of the few women there. She was a very good instructor in general chemistry and qualitative analysis. For physical chemistry we had Fraser Young and [William D.] Harkins, although I didn't have Harkins at that time. He was my thesis professor later, but I did not know him during my undergraduate years at all. Then there was W. Albert Noyes, Jr. He was about my age and was one of the youngest instructors. There were others; some whose names I have forgotten. Some of these came later when I returned to the campus, about 1925.

BOHNING: Did you take any physical chemistry during the first two year period? Or was it just through organic and quantitative courses?

FULLER: I thought that I did have a course, but on checking back I find the book we used was Eucken, Jette and LaMer with a preface date of 1925 (2), so I could not have had such a course. I was taking physics courses from Professors Monk and Henry G. Gale and math courses from Walter Bartky, Slaughter and Logsdon. Math came the hardest for me. I got B's in math, spending a lot of time on it, but had no difficulty with physics or chemistry. Those two years went pretty quickly. During summers (1921 and 1922) I worked for my uncle who was manager for the James Levy Aircraft Co. I also worked for the B and G Storage Battery Co. (formerly Harshaw Battery Co.) During my first two years at Chicago, except for English each quarter, I took only chemistry, geology, physics and math.

BOHNING: You didn't go back to General Chemical?

FULLER: Not right away. But I did go back at the end of my sophomore year when my funds ran out and I had to get a full-time job.

BOHNING: Can you tell us what you did in that intervening period--in the summer of 1921?

FULLER: As I said, I worked for James Levy both the 1921 and 1922 summers. After the first world war Levy, convinced that the time for the airplane was near, bought up millions of dollars worth of planes in their original crates. There were Canucks from Canada, Avros from England, Jennies and MF Navy flying boats from the United States. He even brought back a De Haviland bomber. This, and a good sampling of the others, was set up in the abandoned First Presbyterian Church of Chicago, which served as our showroom. Levy was the owner of the largest Buick Agency in Chicago. My uncle Eugene W. Fuller had been a flier in the United States Air Force during WWI. I was shipping clerk and parts boy. My uncle was a good businessman and I learned a lot from him. He never touched an engine or got dirty.

BOHNING: Was this your uncle Norman?

FULLER: No. This was my father's brother. He was ten years older than I. His photographic company became Chicago Aerial Industries which he later sold to the Loeb family - the Loeb of Sears. My uncle retired and died at age 79 in 1967. He was Eugene W. Fuller.

Our customers were barnstorming pilots, many out of WWI service like my uncle. Most were very poor and they often flew in airplanes of questionable reliability. Levy also ran a air service using his Navy Flying boats to take passengers, only two could go at a time, from Chicago to Michigan points across the Lake. One of the pilots from the Chicago area (Ashland Field), named Ralph Diggins, is mentioned in connection with the air shows held in Chicago during this period. This is in the National Space Museum in Washington, D.C. Diggins was a stunt flier and a very good one. I had a chance to fly free with him once but my mother wisely put her foot down. I would have come back very sick. I did fly with some worse pilots who worked for Levy on contracts, but that is a separate story.

Late in the summer of 1922 James Levy Aircraft Company was failing. Some say Levy lost over a million dollars which would be equivalent to over ten million now. My uncle then decided to form his own company, one whose business would be taking photographs from the air. This was the beginning of the Chicago Aerial Survey Company.

BOHNING: Yes. You said you started at the University of Chicago on the first of October 1920. So that would be the summer of 1922?

FULLER: Yes. My recollection is that I worked part-time for Levy during both the summers of 1921 and 1922. That was a matter of 3 months each time. The summer quarter at the University was when the professors and teachers from the hinterland came flocking into the City to take summer courses so as to establish credits for higher degrees.

At the end of my two years of regular attendance, in the Fall of 1922, I ran short of cash and had to have full-time work. Besides, my family needed help, so I answered a blind ad in the Chicago Tribune. Imagine my surprise when, after a few days, I heard a familiar voice on the telephone say, "So, you would like to come back?" It was Al Wahlgren of the General Chemical control lab. He continued, "I have about 200 answers to my ad in the paper for your old job, but if you will come back I would very much like to have you." I was still too stunned to react but managed to get out, "How much do I get?" Well, instead of 15 dollars per week, I got 18 dollars. I was reluctant to take the job because I did not think that what chemists did at General for so little pay was what chemists everywhere did, and I was sort of hoping for a new experience. I remembered my acid-blackened hands from my eight months previous service as Baumé boy and hinted I would like to learn more about quantitative analyses. But Wahlgren agreed I was "experienced" now, and able to move up a notch. Yet the pay did not reflect it.

However, I was in a corner and really had no alternative. So I returned to Calumet Works in the Fall of 1922 to the long train and trolley rides as I had done two years before. But there was one difference, I would ride the night train all the way to the Chicago Loop and attend the University night classes and continue toward my degree.

Most of the important analyses in the control lab at the General Chemical Co., were performed by experienced chemists or technicians. This was because the results determined the price for the whole tank-car load of the product. For sulfuric acid this would be 144,000 pounds of acid. Therefore the analysis had to be accurate to the second decimal. Even when the acid was selling at 5 cents per pound an error could be important.

BOHNING: Were you the only one doing the specific gravities?

FULLER: I was the only one doing them the first time I worked at the plant. At that time I was also learning to do other analyses, both gravimetric and volumetric. This experience had proven very useful later in my courses. I was hoping I would learn more this time.

BOHNING: Was this at the Calument plant south of Chicago? Where were you living?

FULLER: I was living at home, 248 W. 73rd Street, about 10 miles to the north. The plant was at 122nd and Carondolet Avenue almost in Hegewisch, Illinois, which was a troubled Polish town. The men and women who worked in the plant lived there. The women packed the platinized asbestos used as catalyst and, in those days, no one was aware of any hazard. There were some company houses next to the plant supposedly at bargain rentals. The sulfuric fumes were so dense that no one could live in them for more than a few weeks before moving out. There were some pitiful tennis courts for employees too, but the galvanized wire surrounding them had long ago succumbed to the acid. We, Howard O'Brien, and I were supposed to check the accuracy of the electrical-conductivity device controlling the absorbing acid strength and the plant engineers were supposed to maintain the acid at 96% continuously so that the $SO_3$ would be absorbed. If the acid dropped a few percent from 96% sulfuric it wouldn't absorb the $SO_3$. This would then go off into the atmosphere and then there would be hell to pay because the $SO_3$ would absorb water and you would have a dense, acid fog. They were very particular about the monitoring. There were no automatic instruments in those days. The big plant was fascinating to me. There were monstrous blowers circulating these noxious gases through the catalytic converters. The concentrated sulfuric acid would run into huge storage tanks about two blocks away from the sulfur burners. Of course a large supply of good water was essential.

BOHNING: How much of it were they making?

FULLER: I don't know. We were shipping about two to four tank cars of sulfuric acid a day, each weighing over 100,000 pounds and maybe one or two cars of Sugarhouse acid (20% HCl). Also every day some cars of saltcake [$NaHSO_4$] and depilatory [$Na_2S$]. The nitric acid was shipped in carboys, mixed acid (60% $H_2SO_4$ 40% $HNO_3$) was shipped in tank cars. We made "electrolytic" sulfuric for Exide Storage Batteries. That too was shipped in carboys. Sulfuric acid was also supplied to the muriatic and nitric acid units for their own production. All of these products had to be analyzed by our control laboratory. A rough answer to your question on sulfuric production might be about 300,000 to 500,000 pounds per day.

BOHNING: How many people were working in the control lab?

FULLER: About five to ten. Some were college graduates, but not very many. There were two or three college graduates. They usually didn't last too long because the company would prefer to train youngsters from high school and they could do an excellent job. You know chemists. They are (or were) so considerate and excited about their subject that they wanted to educate everybody about chemistry. This, however, didn't help the wages that chemists could command. In particular, it was awfully tough to work for a company like General Chemical. The militant unions were determined to come in because of the way the employees were treated.

BOHNING: They took advantage of the employees?

FULLER: No question. It wasn't the local bosses, but the executives in New York who set the strict rules. They would say, "Our competition (Grasselli, Hercules, Du Pont, Stauffer, and others) requires us to be tough."

BOHNING: As far as I understand they had plants in most of the major cities.

FULLER: Perhaps a few dozen plants. Not sure. General Chemical was taken over by National Aniline and later by Allied Chemical. But I never saw such a bunch of pinch-pennies as General was in the 1920s. If you broke almost anything you were expected to pay for it, even though you only made but fifteen dollars a week. If you broke a thermometer or a hydrometer it was a very serious matter. I lost a small platinum cone once that was worth about fifteen dollars, and the boss said I might have to pay for it. All of the refuse had gone to the power plant where it was burned. The power plant ashes were dumped on an ash pile two or three times each day. I said, "It's probably somewhere in those ashes." So I was allowed to go over to look for it. I began sifting and was as surprised as anybody when I found it.

BOHNING: Did you have to do that on your own time?

FULLER: No. I did it on their time but you were hired twenty-four hours a day. If anything went wrong at the plant you were expected to come and help. If you worked overtime -- which we did quite often -- you were entitled to a piece of pie and a glass of milk or a sandwich and a glass of milk. And you had to pay your own carfare.

BOHNING: Obviously this was something that you did not want to continue doing.

FULLER: I took the job again anyway knowing all the bad things about it. I liked the people, and the work was interesting. It was fascinating seeing the sulfur ore (pyrites) come in the large ore ships, conveyed to the big burner, and the $SO_2$ collected, catalyzed with air to $SO_3$, and this absorbed to produce oleum for the oil industry. At home I tried to set up a miniature sulfuric plant, burning sulfur, but I couldn't get the catalyst to work.

Out of plain curiosity I had drawn flow charts in a notebook I kept of the sulfuric process in the plant - the location of valves, strengths of acids, pressures, etc. Every few weeks I had to go into the plants to measure moisture contents of the flowing gases. These were somewhat dangerous visits, if the jokers in the plants decided to gas you by opening a $SO_2$ valve as you passed by a converter. It was a noxious jet that would almost knock one over. Unfortunately, my notes on the plant and its operations were lost when all of our household belongings were confiscated for non-payment of storage fees.

BOHNING: Did you progress into any other kind of work there before you left?

FULLER: My work was promoted, but not my pay. I was taught how to do more complicated analyses, for one thing. There was always the ore pile to analyze, coal to be checked. Special analyses for acids going into storage batteries or food products. I learned a lot of the kinds of analyses that went on in those days, but are obsolete now. Probably what I learned in the way of the philosophy behind analytical thinking and the statistical treatment of data was the most important.

But things were about to change for me rather abruptly again. Howard C. O'Brien, whom I have already mentioned, had come to the General Chemical control lab during my absence in 1921. He had taught high-school chemistry in Deerfield, Illinois, although he had never graduated from college. He was an expert at analyzing some of the important acid mixes that General marketed as well as in telling the nitric and sulfuric plant engineers how to use up acids on hand to make salable mixes. But Howard was unhappy. He and I became very close friends. One day he came to me and told me he was thinking of leaving and I should leave also. He said in effect, "The powers that be here have twice failed to come through with raises that they promised and it doesn't look like they are going to. I'm leaving at the end of next week." Well, they didn't come through the following week so he quit and he again said, "You'd better quit too." I said, "Well, maybe I will." At that point I had a brother-in-law who was working for the Chicago Tribune and I called him up and asked him if there were any jobs there. He said, "Sure, I'll talk to Mr. Park at the Sunday Tribune plant on Ontario Street and have him see what he can do for you." Well, I quit on a Friday and started the following Monday at over double what I was getting at General, $44 per week.

BOHNING: What year was that?

FULLER: I started at the Tribune on 17 May 1924. I had gone to night school when I was at General Chemical. Because I couldn't go to day school, I went to night school. I would go downtown to the loop on a little two-car train, switching at Englewood Grand Junction at 63rd Street to take the Rock Island train into the loop. Then I would walk over to the Lakeside building on Michigan Ave., where the University had its night classes. Since I had already taken all the science courses required for graduation, I could take almost anything I wanted and it would apply for my degree. I was taking various courses -- social sciences, psychology, astronomy, philosophy, and subjects like that. Now, with my new job at the Tribune, it was much easier to get to night classes, because I worked right downtown where the school was. And I had time to use the John Crerar Library which was a beautiful library. The University of Chicago has now bought the contents of that library (1987) and they have built a building on campus to house this valuable collection. Crerar was one of the most stimulating places. I could spend a whole day there. Twelve stories of nothing but books -- all non-fiction. You could get the latest books on technology, science and medicine. But you could not take any books out. It was a reference library. All you needed was the Dewey code number and the book would be delivered promptly at the desk unless it was in use. It was particularly convenient with my new job at the Tribune because I could stop there after work, grab a little supper at the Ontra cafeteria and go to my night classes in the Lakeside building a few blocks to the south. I did miss going home for supper but I had the week ends.

BOHNING: Where is Crerar located?

FULLER: At that time the Library was near the corner of Randolph Street and Michigan Boulevard in Chicago. The Lakeside Building was at Monroe and Michigan, I think. The Tribune plant was at Ontario and the Lake, about a mile hike from Crerar. When I was working days, I would walk directly to the Library where I could put in about 2 hours of study and reading before my evening classes. Later, after I had joined the night shift, I had time to study on the job, but that was not possible on the day shift. I learned enough German that way to pass my requirement for my doctor's. I had had enough French in high school so I was all right there.

Often during these study periods in the Library I would reflect on my life and especially on the striking contrasts between the work environments of the two jobs I had had -- but that's another story.

BOHNING: Do you want to tell about that?

FULLER: I think I have brought out the contrasts between the two strikingly different environments I had in my two jobs. What worried me was what would happen to me if I were to get my graduate degree and have to stay where I was. The Tribune had solved my money problem for the time and it was a pleasant place to work, if unchallenging. But it hardly fit in with the career I was still hoping to turn my full capabilities to. It could become a way of life. After all, I was 23 already. Out of this kind of thinking I decided to push my present strategy a notch further. I would change and go to the University during the daytime like a regular student and work the 4 p.m. to midnight shift at the Tribune. That way I could return to the campus and get the graduate courses I needed, and be with the students, a lack I had not been happy about. Since there were a number of my fellow workers anxious to get off of the night shift I had no difficulty making this switch. I think this was the late Fall of 1925.

The working atmosphere at the Tribune was a great improvement over that at the General Chemical Company. You were treated like a person -- as part of the enterprise. One made out his own time card, for example. You were given two weeks vacation with pay even though you were union and you were paid usually more than the negotiated wages. On the night shift, on which I worked most of the time I was at the Tribune, you were allowed to leave early if your work was done, provided you worked overtime (for which you were paid) when the workload demanded. Our quitting time was midnight. I had 8:00 a.m. classes much of the time so it helped a lot to get out early the night before. Sometimes we had to work until the sun came up the next day in order to get out something urgent and there was little or no sleep for me that night. But things usually balanced out in my favor.

In order to squeeze in the required courses I needed for the year, I attended classes during the summers. I was able to get some well-known professors in physics and chemistry because the University often got special lecturers in the summer. Among some of the professors were the following: Arthur Compton, A. A. Michelson (I audited what must have been his last lectures), R. S. Mulliken (a former Harkins' Ph.D) and A. J. Dempster in physics. In chemistry there was G. N. Lewis in thermodynamics, and Thorfin Hogness in photochemistry. Mulliken was teaching in physics although he was a chemistry Ph.D. He had spent two years abroad mastering quantum mechanics, so was far ahead of most physical chemists in the USA at this time, about 1924. He was one who really understood quantum chemistry, advanced band spectra in particular. I remember taking a course by him in which he explained the quantum theory of the hydrogen molecule and its band spectra. It was a completely new approach for the students and even to some of the professors. [Werner] Heisenberg came in 1928. Mulliken was about the only one who was able to talk to him on his own terms because Mulliken had studied and mastered this new material. Mulliken was later a Nobel Prize winner.

BOHNING: Did you hear Heisenberg when he came?

FULLER: Yes. The lecture hall in Ryerson was packed with professors and others eager to listen to him. Here was Heisenberg, who was only about twenty-eight years old, who had learned English, some said, on the boat coming over and he was talking in understandable terms. I was standing in the back with other graduate students. Heisenberg was very confident before this august group and put on a remarkable performance. I did have a chance to hear other well known professors lecture, but the names escape me now. I have already mentioned Gilbert N. Lewis. After about six months or a year of courses, I spent nearly all of my time on my thesis work. This lasted for the next two and a half years.

BOHNING: When did you get your undergraduate degree?

FULLER: That was in 1926. I should have been in the class of 1924 but I was delayed by that hiatus at the General Chemical Co. The night classes slowed down my progress. I kept on working because I needed money for school and home expenses. I became a Union member (International Photoengravers), received my card and became a night foreman. We were doing 4-color reproductions, a process that the Tribune named "Coloroto", also "rotogravure". The Company pioneered in that process although it was a natural outgrowth of the sepia photogravure printing. The Company decided to put out a four color magazine they could market on the news stands, as it did the newspapers. They offered $25,000 for a name for the magazine. The winning name was "Liberty" which was published at our plant for about four or five years. The early success was offset by a serious technical error. The beautiful prints which were obtained on the old presses unfortunately could not be reproduced on the new wide presses. Those beautiful ads which caused Colonel McCormick to distribute cigars, candy and bonuses and which led to the installation of the new presses taking the double-wide sheet, were never again achieved, at least not consistently. The reason was that the expansion and shrinkage was much greater on the wider sheet. The red lips would often appear on the cheek instead of where they were supposed to be. Although losses were incurred, rotogravure was a success, even counting the estimated millions spent on the new presses.

To help pass the time at work, I sometimes reflected on ways physics or chemistry might be applied to help out in the photogravure process, for it does involve much of both. Most of those in our union thought of themselves as artists. In fact the artists and retouchers were in charge. However, what can happen when the basic science behind a process of this kind is violated was illustrated one time. Photogravure is a process which holds the ink in microscopic depressions all of the same area but of varying depths. The different depths are produced by the length of time the etchant, ferric chloride of various concentrations, is allowed to eat out the depressions in the copper surface of the cylindrical printing surface. The varying strengths determine when a given area of the surface will start to etch out the micro-pits. Well, one time, one of the etchers, who were the highest paid in the Union, came running to the boss saying the entire edition was ruined -- the humidity was too high or more probably the carbon paper used for the photographing of the news copy was faulty. Actually, something very simple had happened. The company which supplied the etchant had sent ferric sulfate solutions instead of ferric chloride. A simple test for chloride or sulfate would of course have prevented the loss. The fact was the etchers never understood the basic chemistry of the process they were using.

I made several suggestions to one of the Company bosses, who I must say was quite receptive, on some of the applications of technology that might be made. But, I think, to little avail; probably what was needed was a Research Institute which could serve the printing industry as a whole. Maybe it still would be a good idea. But that was nearly 60 years ago and look what has happened to that industry since.

BOHNING: They didn't really pay attention to your suggestions?

FULLER: They did - some. But I think they were more concerned to have me take over and apply my abilities to the electroplating processes, which were in more serious trouble. Already a lab room had been set aside for me where I did set up to control the plating solutions. I was glad to advise them, but I had had my experience with control labs and wanted no part of doing the work needed, even with assistant help.

BOHNING: Why did you continue with graduate work?

FULLER: I got sort of swept into it. I knew I eventually wanted a job in research, preferably in chemistry. But what I was doing was interesting and provided me with needed money. So I went on.

I mentioned before that I attended summer quarters in order to fill out my program. So the time I put in was nearly equivalent to a year of regular attendance. I did this for four summers as I recall (1925 to 1929). I have already mentioned how the composition of the student body changed during the summer quarter. Many men and wives came. I found a number of friends among these groups. Two in particular, Joe J. Jasper from Wayne State U, and his wife, Grace. Joe was working for a Ph.D. under Harkins, I think. I spent one vacation with them, I remember, in Holland, Michigan. Grace also typed my dissertation for a very nominal charge. They are both gone now.

During summers I also found graduates whom I knew from undergraduate days. In physics I knew Gerald W. Willard, who later came to Bell Labs and also became my best man. Al Shaw was Dempster's assistant then. It was he who during my visit in 1944, let me in on the successful chain reaction of Fermi. Five of us struck together for a long time in chemistry: Hubert F. Jordan from Northwestern, Roy Dahlstrom from Nebraska, who later became vice president - research, for Titanium Pigment Company, Albert W. Meyer, who like Jordan joined the U.S. Rubber Labs in New Jersey, and Arthur E. Schuh who worked in the next lab to mine in Kent. Schuh also had taken a job with Bell Labs about six months before I did. Some of the five worked in the Kent labs during the summers.

BOHNING: How did you select Harkins as your thesis advisor?

FULLER: That's a good question, but I don't remember how that happened. My recollection is that we enrolled for research much as we did for other subjects. Chairs and tables were set up in the Bartlet gym. Students knew what branch of chemistry they wanted to work in and sought out that professor. He would have a number of problems on which he would like to advise. In my case I showed an interest in the nitric oxide structure problem and after thinking it over a few days and getting a better picture of how I could proceed agreed to give it a try.

Although Harkins was a hard person to catch because he was often on consulting trips or in someone's office talking, and seldom in his own office unless he was working on a paper, I saw him enough to get to know his peculiarities, and like most professors, he had a few. Harkins had broad interest in science. He was all scientist. He slept, ate, and drank science. He enjoyed arguing with his students about nuclear structure, surface films and interfaces, isotopes; almost any question having to do with the areas between physics and chemistry. You would get him in the corridor or he would get you and talk to you as long as you would listen.

One of his students told me, I think it was Hubert Jordan, that he stopped to talk with Harkins after class one time. It was late in the afternoon and after a while Hubert became hungry and apparently W.D. did too because he reached in his pocket for a package of peanuts which he proceeded to eat between sentences and became so engrossed that he failed to offer any to Hubert. There were a lot of stories circulating about Harkins, most with a germ of truth in them.

BOHNING: What kind of stories?

FULLER: For example, there was a rule at Chicago that if a professor didn't show up for his lecture within 10 minutes, the class was honorably excused. But Harkins would always make it five or eight minutes after the hour. You could hear him coming down the long hall in Kent because he always wore shoes with hard leather heels. He also had a regularly paced walk. He never hurried. So the class could usually get a warning, that is unless he stopped to talk. Then no one dared walk out in front of him.

Harkins's punctuated gait had also been noted by his graduate students as a handy warning of his approach. There were about five labs in a line in the basement of Kent, all for Harkins's students. Art Schuh, who was studying nuclear disintegrations, probably understood the boss best and had worked out procedures for avoiding his visit if Art was feeling unprepared for it. Schuh had a large Wimshurst machine in his lab capable of producing high voltage discharges. He knew that Harkins was deathly afraid of high voltages, so when he didn't want a visit, he would start his big static electricity machine up and the noise of it when Harkins came to the door would be enough to cause him to go on to someone else. If this wasn't sufficient, Art could always add a loud cough. W.D. had a fear of colds, too. Harkins had warned me many times about the 220 volts on my magnet, but he never in all my time with him went near the apparatus.

BOHNING: Your thesis had three sections to it.

FULLER: We did not plan it that way but the capabilities of the apparatus, after I had worked with it for a while, determined this. The magnet I used was sort of special. It was a large magnet for those days, capable of producing magnetic fields of approximately 30000 gauss over areas of about 20 square centimeters and a gap of about 1 to 2 cm. Compared to today's field strengths this is not impressive. At that time it was an intense field. Most important for me was that the field was uniform over the entire face of the poles because I would need to maintain a temperature difference in the gasses I was to examine and this gradient had to be all within a uniform field.

My first job was to measure the field uniformity over the surfaces of the pole pieces. This is when I invented the water manometer which became the first part of my thesis. The second part was really what we had set out to do, namely to measure the magnetic susceptibility of nitric oxide as a function of temperature.

BOHNING: It was the magnetic susceptibility of nitric oxide gas?

FULLER: Yes. I used a method that the Germans had used on other gases, but found it impracticable for NO. Originally I tried making the apparatus out of glass but this proved too fragile and besides, its low heat conductivity made it difficult to maintain the required temperature gradient. So I went to an all-brass and copper system made of small tubing. Here I had to worry about interaction of the metals with the NO gas. Fortunately, the reaction passivated the interior surfaces of the tubing so that it quickly stopped. However, I was still unable to make the absolute measurement of the susceptibility I had hoped for and resorted to relative measurements, using oxygen gas as a standard of reference, since its magnetic properties are well known. When the magnet is switched, on the field causes a pressure difference in the O-system. This is measured by means of an ingenious device called a Heiss manometer (also of German origin). I was fortunate in having our top chemistry-shop mechanic (Mr. Kittel) construct this manometer for me. But I had the job of attaching "invisible" quartz fibers to a micro-mirror of almost zero weight which was the working element in this manometer. This was a very sensitive device and reliable, once you had it working. It had to be standardized by pressures created in a vertical part of the O-system by thermostats kept at known but different temperatures.

The result of this work in part 2 of the thesis was that we found a low ratio for the Curie constant of nitric oxide relative to oxygen - lower than classical theory would predict, but in agreement with predictions of van Vleck based on band-spectra calculations. The increase in this ratio with temperature was also in agreement with band-spectral ideas.

The third part of the thesis had to do with the so-called Glaser effect. Walter Glaser had reported finding anomalous magnetic properties in carbon dioxide. Lehrer had failed to find these anomalies. Since we had an accurate way of checking this, using our relative measurements against oxygen, we tested for the effect in carbon dioxide and like Lehrer, also failed to find any abnormality.

This is a brief sketch of my thesis work. I would like to add a few more comments. It was getting near to the end of the Fall quarter and I hoped to get my degree. My thesis had not yet been accepted. Presumably it was going the rounds of those who would approve or disprove of it. I decided to go to Stieglitz because I had tried to see Harkins about it and was unable to find him. Stieglitz followed the old German custom of not recognizing students except during visiting hours. He seldom would stop if approached in the halls. There was one exception, Irving Muskat, his assistant. He kept Stieglitz informed on what went on in organic chemistry in Germany. I had heard on good authority that Professor Stieglitz had corks installed in the floor of his office, which was directly over the organic prep. lab, so that he could monitor whether anyone was letting fumes escape down there. He was especially fearful of methylamine. If there were such odors he would rush down and find the violator.

It was through hearsay and little observations of mine that I had developed an assessment of Stieglitz which was unjustified. As I got to know him better, I found out I had been grossly unfair to the man. Stieglitz was, in contrast to Harkins, a neat and orderly person. I found him also to be a kind and considerate one. For example, as I said before, my thesis seemed lost. But when I said to Stieglitz, "It has been two or three weeks and I haven't heard a thing." He called me into his office immediately and asked me to follow him. We headed directly for Harkins office with his pass key and after knocking, Stieglitz ushered me into Harkins office, which I had never been in before. "Look at that desk!" he said standing and looking at the piles of papers. He had apparently faced the same situation before, because he seemed to know where to head. After only a few attempts he pulled out my thesis copy. He assured me that it would be circulated, and in a few weeks it was accepted. I think Harkins had been away consulting and I don't remember his saying he was sorry for the delay. Some days after that those of us who had qualified came up for our oral exam. I was rather frightened because we had some new younger professors present who liked to trap you with their questions. But again Professor Stieglitz showed the compassion of experience and several times chased these hyenas off.

The Fall Quarter of 1929 ended and I had my degree, not knowing what to do with it. Already some of my contemporaries were having trouble locating jobs. I was delighted when I heard that representatives of the Bell Labs were coming that week (December 1929) for the purpose of holding job interviews. However, I didn't even in my luckiest state think that I had a chance. My interview had been arranged and it would be conducted by Mr. R. R. Williams, Head of Bell Chemical Labs, someone I got to know and admire in the years that followed. I remember nothing of this interview now except that it happened. R.R. also worked his way through college (U. of Kansas and Master of Science degree at Chicago). He has also received many honorary degrees since for his work leading to the discovery and synthesis of vitamin B1.

Williams evidently turned in a favorable report on me for I soon received a letter saying the Bell Labs was offering me a job. It was from Mr. George Thomas, head of the employment department. For doing what I liked, I would get paid almost what I was getting at the Tribune!

BOHNING: How much was that?

FULLER: I think it was three hundred dollars a month.

BOHNING: Did you pay your own tuition at Chicago while you were working?

FULLER: There were essentially no funds available to help in those days. I spent $40 per subject per quarter, I think, for tuition. Books, lab fees, carfare, clothes, meals, and supplies I paid for. Except for lunch and supper during the week, as I lived at home. I lived very frugally and was actually saving money.

BOHNING: So you were totally on your own?

FULLER: Yes. I received no school assistance. I was contributing at home, but not substantially. I should have provided more.

BOHNING: Was that in June of 1929?

FULLER: No. I received my Ph.D at the end of the Fall Quarter (December 1929). That is my recollection. It was the winter of 1929. After I received my degree I had a few months before I started at Bell. Needless to say I quickly accepted Thomas' offer.

This was an extremely exciting time for me. I was about to realize a long-imagined dream come true. Often I had sat in the Crerar Library reading about the people at Bell Labs and Bell Labs' contributions, both envying and admiring the lucky people there. My experiences at General Chemical moderated my enthusiasm somewhat, but I was sure that that was not typical of research in science, even in industry, and Bell Labs was an industrial organization. Besides the whole question was moot, because I would never get the chance for a job there anyway. Still it was fun to read about such things. "I couldn't be sure how things really were," I thought. I continued, however, to read about the Labs' happenings and Darrow's, Introduction to Contemporary Physics (3).

Now all artificiality was gone. R. R. Williams had come, Mr. Thomas' letter was real, my cozy arrangement with the University and the Tribune was in jeopardy. I had to decide. I would be breaking long family ties. Something I thought would forever remain a dream, had actually come true. Would I actually get paid for doing what I liked most -- to be doing exciting experiments in science?

There could be only one outcome, of course. After a tearful separation from my family, I boarded the Nickel Plate for New York with my meager possessions. It was not a finale, but a new undertaking over which I would have somewhat less control than I had had in the outcome of the past 10 years. That episode too is now over and I can say my 37 years with Bell were all, well almost all, I could have asked for during these 60 years, 23 of them in retirement. I have been most fortunate also in finding a capable and caring wife who has joined with me over this long period in raising a family who are now 14 strong.

BOHNING: When did you start?

FULLER: I started at Bell Labs on 10 March 1930.

BOHNING: What had you done from the time you finished your degree to the time you started at Bell Labs? Were you still working at the Tribune?

FULLER: Yes. I was taking things easy then. I only had to do one job.

BOHNING: The Depression started in 1929 too. Was there any immediate effect on you?

FULLER: Not a great deal except that I lost about one quarter of my savings. It was seven years before I was making as much at Bell Labs doing research as I was at the Chicago Tribune doing photoengraving. We first went from five and a half days -- Saturday was a half day -- to five days. Then we went to four days for a while. So, although our pay rate was not cut, our take home pay was. After we went back to full time, we began to make up for the difference. They weren't as bad years as they sound. You may remember it.

BOHNING: Well, I was at the end of that.

FULLER: Consumer prices were very low. Some businesses were not affected a great deal. In fact, I was married in 1932 near the bottom of the depression and we did very well on low income. My wife, Willie, was working at the American Institute of Physics doing editorial work and there seemed to be no contraction there. The editorial staff of the Physical Review had just moved to New York and, together with people from other branches of physics, they formed the Institute which, at that time, published nine technical magazines. It was through my wife and Gerry Willard, who I knew at Chicago, that I met quite a few other Minnesotans who had joined the Labs recently. Some of these, including Walter H. Brattain, one of the inventors of the transistor, lived in Greenwich Village almost adjacent to the Labs. It wasn't long before a Minnesota group formed who met for outings and parties. They even expanded to include Minnesotans at Princeton who had been Professor Tate's students. Walker Bleakney, E. U. Condon and their wives, often joined in the gatherings.

So the depression was not too bad if you had a good group and a job. After 1932 the Institute continued to grow. Willie, my wife, left to raise a family in 1935. Bell Labs established (1941) the new headquarters and laboratories at Murray Hill and absorbed our small group who had been stationed (1931) at the Summit, New Jersey location. The Institute remained on 38th Street in New York for some time. Now I think it has a building of its own?

BOHNING: Yes, they do. I was there back in the Fall. They have a very nice facility. So the first labs were in New York?

FULLER: Yes. 463 West Street. That was originally the Western Electric Company Technical Division. In 1925 it became the Bell Telephone Laboratories. I was at West Street for a year or so. Then, in 1931, I was moved with my group out to Summit, N.J. For a time I had interests in both places. Willie and I lived in East Orange at that time. The Summit Lab, not to be confused with Murray Hill where the big new laboratories were built in 1941, was where our first work on synthetic polymers began.

BOHNING: Before we get to that I wanted to ask you one other question. Before your interview with Bell Labs, had you given any thought to any other career choices?

FULLER: I had thought one time that I might stay in academe, Harkins had made me an offer, but it wasn't anywhere near good enough. The universities couldn't pay very much. So I decided I wouldn't be an assistant. [George] Jura took that job as Harkins's assistant and for many years he was doing experimental work with Harkins on surface chemistry. Harkins's work on emulsions later became useful in the Government synthetic rubber program in 1944, when I renewed my acquaintance with him. R. R. Williams had arranged research contracts under the ORD for both Harkins and [Morris S.] Kharasch. Harkins contributed some useful ideas on the micellar theory of emulsion polymerization. Kharasch, I think, was responsible for solving the problem of "popcorn" polymer. Perhaps this is a good place to insert some biographical memories?

As I have said before, Harkins was not easy to get to know. It was not until my graduate study with him that I learned more about him. Although he was pretty much of a loner, he wanted much to have faculty friends, but except for Professor Lunn in the math department, he had very few. With his students, too, he tried very hard to be liked. He had a good sense of humor and was always cordial. He was even-tempered and tolerant. My contacts with him were always helpful and friendly. My main complaint was that he did not give me enough time of his expertise. The reason, I knew, was that he was too much engrossed in surfaces and interfacial problems during the period I was there. Harkins was very conscious of credit and was not adverse to conducting public disputes if he thought his priorities were being violated. His arguments with McBain of Stanford and with Irving Langmuir at GE are two examples of this, which did not sit well with the science community. It is not that W.D. was not creative and imaginative in his researches. His papers show that he was. In fact he has made lasting contributions to many fields including not only surface chemistry and physics, but nuclear structure and the nature of the chemical bond as well. His work on isotopes with R. S. Mulliken is well known. He named the neutron after it was realized that no electrons existed in the nuclei of atoms. His helical model of the periodic table showed early originality. Finally, W.D.'s place in science is well established. He was a theorist who had many novel ideas, the latest of which were demonstrated in his contributions to the Government Synthetic Rubber Research Program which R. R. Williams initiated with him at Chicago during the last war. Harkins was not young, but his explication of how the polymerization process takes place in emulsions (1944) was done with the freshness of a young mind.

BOHNING: What was your first assignment when you went to Bell Labs?

FULLER: My first assignment really was to improve the toughness of the organic varnish-type insulation applied to magnet wires, called "enameled wire", by investigating alternative kinds of materials for this purpose. This wire was used for winding loading coils which were manufactured in increasing quantities by the Western Electric Company and the rejection rate was much too high. At first this seemed like a simple and straight-forward job. However, the compositions involved and the complex chemical reactions which occurred in the baking process, to which the coated wires were subjected, quickly proved to be suffering from complete lack of any basic understanding. To begin with I had only one organic chemist, a quiet capable chap from the University of Kansas named Charles L. Erickson and his assistant Norman Pape. These two had worked on the problem for about a year. Later on, after we had broken down the problem into its component parts, Erickson transferred to the Western Electric Company Buffalo plant where he helped to install immediate partial measures as parts of the total solution to the problem. The question of the coating materials we knew was a different matter from the problem of the wire. That would take us into what came to be loosely called "polymer chemistry". Let me clarify this further.

It turned out that at the Labs when you were given a problem, <u>you</u> were expected to find out what needed to be done. You were the expert, even though others may have been studying the problem for years. In effect the Labs said, "Here's the problem, you go ahead and do it." Actually, in this case we found that the difficulty was not so much an inadequacy of the insulating materials, as it was with the condition of the copper wire. What we were doing was obviously applied research, but it was necessary in order to define the problem. In other words, even if through research we could improve the properties of the insulating coatings, with the kind of wire we were using in the plant, there would likely be no solution to this problem. So we recommended that attention be given to the wire, and the continuation of, perhaps even expansion of, the basic program on synthetic polymers and their properties. The groups at the Labs interested in expansion of this horizon of plastics and elastomers were growing. Already Bakelite and the cellulose esters were filling voids in the telephone system. Much more work was needed. But of far greater interest to us were the fascinating polyesters and polyamides discovered by Wallace Carothers and his co-workers at Du Pont (4).

BOHNING: What year was this?

FULLER: We started immediately on this work, but I think the polymer work started probably around 1932 or 1933.

BOHNING: Oh, that early.

FULLER: Well Carothers published his paper in 1930, I think, or several years before (4).

BOHNING: The reason I asked that is that I have some Bell reports which show proposals to the company in about 1937 to start making these new polymers (5).

FULLER: Oh, yes. We were well into it by then.

BOHNING: That's what I wanted to clear up. I was looking at these documents and it seems that it started then, but you had been doing it well before.

FULLER: Certainly by 1935-6 we were well into the work on synthetics.

BOHNING: You had no real formal training in polymers because it was a brand new field; you only knew what you read from the Carothers's papers.

FULLER: No, it was not true that we were only informed by Carothers's papers. When we first went to working four days a week at the Labs, I had Fridays free to go to the New York Public Library Science Reading Room. It had a surprising number of reference books and journals on polymer researches. I set out deliberately to inform myself on this interesting field. It is true that there were no formal courses being offered. The Germans were the most active. In the period 1932-34 I translated nearly all of Staudinger, his book and papers, and all the other publications I could get. Meyer and Mark's book appeared in 1930 (6)and Staudinger's in 1932 (7). Meetings such as the Gordon Conferences and the Baker lectures by J. R. Katz at Cornell in 1934 also helped to expand the growing field (8).

Bell Labs did not get into polymer research any too soon. For one I was convinced of the rich future that waited researchers in this field. But our Director, R. R. Williams and our president, Oliver E. Buckley, if anything, were more convinced than I was. During the months of the Depression, 1932 to 1935, when we had the extra Fridays, I worked translating the German papers and books for my own edification. (Later, when I retired, I tossed all of my translations into the waste basket. They were completely out of date.) Wallace Carothers had, just a few years before, published his epoch-making papers on the condensation polymers -- polyesters and polyamides (4). This looked like a good place to start. We were still doing work to improve the natural fibers as insulations and switched this small group to make these synthetic polymers and to study their properties. Methods characterizing polymers were being developed, particularly by Hermann Staudinger, who insisted that the synthetic molecules were extended linear chains. Theories of the formation of gel were being worked out by Roy Kienle and Paul J. Flory who, like Staudinger, later received the Nobel prize for his contributions, so well exemplied in his textbook (9).

We were primarily interested in correlating the physical characteristics of polymers of known chemical composition and molecular structure, with structure. The background work both in Germany and in the United States helped to decide where we could best contribute to this rapidly growing field. In particular the simple chemical structure of Carothers's polymers, as A. W. Kenney and J. W. Hill showed, are able to form into crystalline aggregates. This made them good candidates for x-ray structure investigation and that is where we decided to start.

BOHNING: What year was that?

FULLER: It is difficult to set the exact date. Our ideas grew slowly at the start after Carothers's work became known in 1930. Our synthetic program did not really get started until 1935-36, when C. L. Erickson and C. J. Frosch entered the work for a time. Erickson and I published our first work in 1937, an x-ray study of some linear polyesters (10).

BOHNING: Oh, that early?

FULLER: Well, Carothers published his papers in 1929 and 1930. A. W. Kenney had taken x-ray photographs of polyesters at du Pont in 1932. Our more extensive x-ray work was done in 1936-37. That was after I had attended the Baker Lectures by Professor J. R. Katz at Cornell in 1934.

BOHNING: You say that was 1934?

FULLER: That was late in 1934. Professor Katz was a strange character if there ever was one. He died fairly young, in his later fifties, I think. He had an M.D. besides his Ph.D.

One time when Willie and I lived in Summit, N.J. we invited Katz to supper. It was in December of 1934 I believe. I had built a ball model of cellulose and hung it as an ornament on our Christmas tree. No sooner had he entered the door of our apartment, he spotted the model and exclaimed, "Oh, Zellulose!" When it came time for the magnificent supper that Willie had spent the day preparing, and we were seated before the sizzling roast, Katz, taking one of the serving plates and presenting it to Willie said, "All I'll have is a head of lettuce. Just cut it in two." Willie didn't know what to think but assured him that he could have other alternatives. Nothing doing, he would have lettuce. That was his supper.

He did a similar thing once at Cornell. I was going down the cafeteria line with him and noted that his tray was covered with salads -- no dressing, bread, milk, or dessert -- just lettuce salad.

Another time he almost went back home to Holland prematurely. He had packed his x-ray apparatus to be sent to Cornell, including several mercury high-vacuum pumps, in wooden boxes. When these were opened the contents was found to be a mixture of powdered glass and globs of mercury. Katz was so distraught that he walked in circles moaning. One of the students, E. W. Hughes, later a professor at USC, I believe, agreed to build him an apparatus from parts he had in the lab.

With the help of the rest of us, a working outfit, high voltage and all, was built which served very well for the laboratory demonstrations Katz had planned. I am sure Katz could not have done it without Hughes.

At that time Katz and I were interested in the extra sulfur atoms that Thiokol (polyethylene tetrasulfide) which were loosely held and the effect of them on the x-ray structure. Thiokol was an invention of J. C. Patrick of Yardville, New Jersey. We also were given specimens of chloroprene and bromoprene by Du Pont to investigate.

Katz's seminar lasted about 3 months. After that I returned to Bell Labs and set up new x-ray equipment which had come in my absence. Carl Frosch, W. O. Baker, Norman Pape and J. H. Heiss, Jr. helped on the program we had set for ourselves. This work, on polyesters and polyamides of known chemical compositions, was covered in a number of papers published between 1940 and 1946. The x-ray work was supplemented by measurements of solution and melt viscosities on polymers of known molecular weights. Bill Baker came into our group from Princeton in 1938. He and I published about 9 or 10 papers together before he went on his way to finally become President of Bell Labs. C. J. Frosch was an early contributor of merit who also went on to do important work in semiconductors. J. B. Howard, an organic chemist from Yale, did much of the preparation work. Herb Heiss and Norm Pape were the invaluable and versatile pair who kept our experiments going and who recorded the results. Some of us, including me, had to attend scientific meetings, present papers, and give speeches before technical groups. These were mainly at the American Physical Society, American Chemical Society and the Gordon Conferences.

BOHNING: In the 1940 organization chart, you were listed as a plastics chemist. I was wondering how that arrangement worked when you presented a chemical lab report?

FULLER: Actually I never approved of that term. It was chosen, I suppose, to indicate that I had responsibilities pertaining to both the plastics materials engineers and those engaged in polymer research in my organization at that time. At Bell Labs, reporting is done by writing Memoranda for File. It is unlikely that the materials engineers would report jointly with polymer chemists in this situation.

I am not informed as to what the present arrangement of duties at the Lab is. I hope that things have worked out for the best. I do know something of the history leading to the establishment of a greater chemical voice in the selection and choice of materials, however, and you might like me to discuss that briefly?

It is traditional that the persons responsible for the choice and application of materials consumed in industrial operations have a basic training in chemistry. Today a basic understanding of physical science is equally important. When I started at Bell Labs in 1930 this was not so. The Chemical

Laboratories was only partly given this responsibility. Growing out of an electrical invention, the Telephone Company was manned by engineers, mechanical and electrical for the most part. It was natural that the Materials responsibility would fall to them. Mr. Fondiller, who ran the Apparatus Development Department at that time, regarded it as imperative that if his department was going to be held responsible for the telephone equipment and devices, designed and manufactured according to their specifications, they were going to have to be in control of all the materials in them as well. It was reasonable. Still, other factors were working to make inevitable a change from the older tradition. Materials were getting more complicated in composition and structure. Personnel with chemical and physical underpinning, even advanced degrees, were forming a new discipline of materials science. Two materials groups were building up. It was inevitable that sometime these two would have to be combined. It was M. J. Kelly's job to order this move. This he did shortly after the war in 1947. R. M. Burns headed the chemical group, R. R. Williams having essentially retired into his other activities. J. R. Townsend headed the group from the Apparatus Department. Counting metallurgy, there were about 100 in all involved in the move. This is when I was labeled "Plastics Chemist". My group numbered about 20. But I was to report to J. R. Townsend and not to R. M. Burns, the former Chemical Director, even though I still retained Polymer Research under my wing. Probably it was the best that could have been done.

Unfortunately I was not well informed on all that was going on because, as I have already mentioned, I had been absent from the Labs for nearly two years (1942-1944). R. R. Williams had been asked to head a polymer research branch of the ORD (Office of Rubber Director) and asked me to go along to help. After nine months in Washington, R. R. left to continue with his vitamin activities and I took over the polymer research management. I might say something about R. R.'s other interest at this point.

The story of the synthesis of vitamin B1 by the Williams Group of seven chemists, including Williams is related in Toward the Conquest of Beriberi, by Robert R. Williams, Harvard University Press, Cambridge (1961) (11). It is an exciting tale of chemistry in action. Williams enlisted the Research Corporation, a non-profit organization, to manage the royalties from patents obtained by the Group. Except for modest sums to the members of the group for the hours they spent on the job, the money goes to charity -- "The Williams-Waterman Fund for the Combat of Dietary Disease", where it is spent on research. Robert E. Waterman, a member of the Group and a former member of the technical staff at Bell Labs was the first to join Williams in his research (1923), working outside the Labs' regular hours. Later Bob Waterman became vice president of Research for Schering Corp. His help in finding chemical expertise to join the Group was invaluable. Last I heard (1987) the W. W. Fund had provided over 17 million dollars for dietary research.

BOHNING: It must have been 1943 that Paracon was done. I know it was announced in 1943 (12).

FULLER: I was in Washington then. But I was following it at the Labs with Burnie Biggs. It was announced in Chemical & Engineering News. Burnard Biggs was an organic chemist from the University of Pittsburgh who came to my group in the Summit Lab at this time. After working to produce Paracon he went to the Livermore Lab in California.

BOHNING: But the oil resistant rubber (Paracon) was an outgrowth of your earlier synthesis and lab testing that you had been carrying on since the early 1930s.

FULLER: Yes. We never expected that it would serve as more than a speciality rubber. We were also aware of its hydrolytic sensitivity. By using carbonate fillers and maintaining neutrality we were able to make it last for many years. For example the sample I have here was made by Burnie and is still intact after 40 years. It does deteriorate, however. Some polyesters like Terylene (polyethylene terephthalate) are very resistant to hydrolysis. J. B. Howard in our group succeeded in making PET samples that almost drew into fibers about 1945. The material is mentioned in Carothers's patents but the English workers were the first to make a satisfactory polymer and fiber.

BOHNING: I remember reading that [Archie R.] Kemp had brought some -polyethylene samples back in the late 1930s.

FULLER: Yes, I remember this. It was in 1939. A fellow came in from England and had a piece of the polymer in his pocket. It was polyethylene all right. It was tough and obviously of pretty high molecular weight.

BOHNING: Did you make measurements on that?

FULLER: Yes. A number of people made measurements on it, especially electrical measurements, when we first verified that it was what it was. This was of tremendous interest to the telephone company because we were then building coaxial cables and this was a material that had the perfect electrical and physical properties for that task. Low dielectric constant, high insulation resistance and high stability, everything one needed. F .S. Malm and Archie Kemp were instrumental in adapting it to coaxial cable. However, they developed a hard rubber material which fit the plant process better.

BOHNING: Did you do anything else with it then?

FULLER: No. But Archie Kemp and the cable people were very excited. It would be an ideal material electrically if it could be made cheap enough. The English did not disclose their process but we understood it involved high pressures.

At the Labs we were interested in making polymers of known structure in order to study their solid-state properties. For instance, the poly-omega-undecanoic self polyesters that Baker, Heiss and I worked with, were pure polymers of known structure. X-rays showed the expected lining up of the extended chains in the crystalline regions of the polymer. We could also examine solutions and melts by viscosity measurements. Samples of different average molecular weights were prepared and examined. It was found that a modified form of Staudinger's viscosity relationship was required to fit our results. Also Baker's concept of segmental motion was verified.

BOHNING: What was the situation in your group? Baker came in 1938.

FULLER: Yes, he came to the Summit Lab in 1938. Baker and I worked together about three years before I left to work for the government in 1942. I enjoyed those three years a lot and I think Bill did too. Bill took over the polymer research. Carl Frosch, a versatile chemist from Union College who also co-authored several papers with me, took over any and all plastics applications problems that the war-time designers could think up. If it hadn't been for Carl's help and originality on the ASH air-borne Radar, which brought our fighters safely back to their ships, it never would have flown.

I think it is important to point out here that in spite of our concerns with the war, during much of this period we did quite a lot of publication of our researches on polymers. Most of this was directed to polyesters and polyamides, but we reported on work done on other polymers as well - cellulose derivatives, vinyl polymers, and a few others. Much of this work falls in the period 1938 to 1943. I count some 20 papers in all from my group during this time. I wrote about eight of these, mostly reviews. Three or four of the earlier contributions were by Carl Frosch and me. One on polyisobutylene in particular proposed a helical molecule structure which may have anticipated the more famous DNA double helix. This was in 1940. Bill Baker was soon adding his name to our roster. He and I published about eight papers together mostly on polyesters and polyamides. My last publications on polymers were in 1946, 1948, and 1949. As I look back on this productive period, I am surprised that we were able to report as much research as we did.

Baker worked on a government rubber contract let through the Office of Rubber Director. He too re-oriented his group direction and moved into a position of evaluating the synthetic-rubber structures present in GR-S (Government Synthetic Rubber) produced initially. In particular, his group investigated the amount of gel production and devised methods of measurement for it. The ease of milling of GR-S is greatly dependent on gel content. James W. Mullen, also a chemistry Ph.D from Princeton, joined Baker's group in 1943. Needless to say my responsibilities in Washington left little time to follow the many jobs going at the Labs. But I did look in every few weeks.

BOHNING: How did your position in Washington come about?

FULLER: I have already said a little about this, I think. It came to R.R. through Oliver E. Buckley, President of the Labs. Williams was very busy with Bell Labs as well as his own vitamin activities. But since he was needed, he went, somewhat reluctantly I believe. I'm not sure who talked to Buckley; Jeffers, who was appointed "Rubber Czar" by Roosevelt, or Ray P. Dinsmore, who was Vice President-Research at Goodyear. Dinsmore had already accepted the job of organizing the R & D effort for the government rubber program. I got my job through Williams whom I would not think of refusing. Anyway in the Fall of 1941 we landed in Dinsmore's office in the New Municipal Center Building at 4th and Indiana, in Washington, D.C. Williams was Chief of Polymer Research and I was deputy chief. I remained in this capacity (on the government payroll) until July 1944, through my recollection is that I had 2-3 weeks of finishing up to do. I had to promise Ed Gilliland who had just taken over the top job that I would be able to return if called and that I continue as chairman of a sub-panel on Elastomers under the Chemistry Division of the Research and Development Board of the DOD (Department of Defense). William Webster, I think, was head of the RDB then. As for our Office, after Jeffers left (May 1943), Bradley Dewey took over the ORD. He lasted until about September 1944 when Ed Gilliland took charge. I don't know who came in then. I was gone. Ray F. Dunbrook of Firestone took over my job. I think that the Rubber Reserve Corporation (RRC) took over the contract function when Dewey left in September 1944. After that, until June 1945, this function was acquired by Office of Synthetic Rubber under the RFC (Reconstruction Finance Corporation ) where it may still be.

The war work was winding down and I was anxious to return to the Labs and start a new program. I was still interested in expanding polymer research and there was the contract work that Baker's group had been doing that had to be closed out (13). We took up again where we had left off with our investigations of synthetic polymers of known structure and published a number of papers on subjects we had held back on account of the war.

BOHNING: I would like to talk about how things changed while you were gone, but I would also like to talk a little more about what you did in those two years with the government. When you first got to Washington what were you assigned initially?

FULLER: Our job was to muster and bind under contract all the chemical talent we could get to insure that a satisfactory synthetic rubber was produced from styrene and butadiene in the plants that were then under construction. We were a back-up program. Our specific task was to learn all we could about the polymer and the process for making it. By the end of 1943 thousands of tons of GR-S would be coming from the plants. The Baruch Report had recommended that one million tons annual capacity be installed ultimately! A secondary assignment was to investigate other compositions, including the making of natural rubber -- but the latter was not urgent. We had a free hand to set-up contracts with universities and the rubber company labs who could contribute, including the four big rubber companies and Standard Oil.

It happened that the latter had already formed a patent-sharing group called The December 19th Agreement Committee and were having a meeting at the Mellon Institute, under their chairman Ed Weidlein of Mellon. This was in December 1942. R.R. sent me to attend this meeting in Pittsburgh. When I got there I was admitted but had to sign an agreement that I would not pass on any information from the meeting to anyone, not even my bosses at the ORD. Well, when R.R. heard this he exploded and it was only a few hours before my notes were released for our use. Finally things took shape and we had a strong polymer research group consisting of chemists and chemical engineers from the four big rubber companies, about 12 universities, at least two government labs, and about five or six chemical companies. A uniform system of reporting results was arranged. What we called CR reports could be written at any time, but were usually given at meetings of the Polymer Discussion Group, consisting of representatives of the contractors. At the start, R.R. and I visited all the contractees, usually on separate routes, to help the personnel get oriented and tell them what was expected of them. But this was done in broad terms and there was much freedom to innovate.

BOHNING: Which chemical companies did you enlist?

FULLER: There was resistance on the part of most of the chemical companies, especially at first. They did not like the patent agreement whereby the Government owned the patents obtained under the program with a right to license. They, however, had access to what our research turned up. Besides our branch under Dinsmore there were three other branches who let contracts. Some of these went to chemical companies who made chemicals or pigments used in rubber compounding. If I remember correctly Cabot (carbon blacks), Witco Chemical, Columbia Chemicals, American Cyanamid, N.J. Zinc Co., Columbian Carbon, even General Electric were involved in work under these other branches. Since the properties of the polymer are greatly dependent on how the rubber is compounded, it was important that we knew what was going on in these labs also. Later on in our program we opened up the circle of participants and Du Pont, Allied, Monsanto and others came in, but mostly listened. Both Esso and Phillips Petroleum were active as chemical companies. The former mostly in butyl rubber and monomer production. The latter was especially productive in connection with the polymerization process (C. F. Fryling).

Perhaps it would help here if I spoke briefly of our system of reporting our results. One of my main jobs was to arrange for lively exchange of information among our contractees. We did this through CR Reports which I have already mentioned and through general meetings, which we called the Polymer Discussion Group, and through committees which dealt with specific topics. We had originally four special committees: Modifier Action - C. S. Marvel, Chairman; Locus of Reaction - W. D. Harkins, Chairman; Polymer Structure - H. S. Taylor, Chairman; and Research Analysis - I. M. Kolthoff, Chairman. [This is covered by Fuller in a History of the Polymer Research Branch, 12/2/42 - 6/1/44, ORD files.]

Arranging for the Polymer Discussion Group Meetings every two or three months was especially tiring. I was fortunate to have the help of my administrative assistant, Lester A. Friedman, Jr. in organizing these very useful gatherings. Making hotel and travel arrangements for about 100 persons during war time is not easy. Here, papers were presented much as is done in the ACS today. We also had the guidance of the Polymer Research Policy Committee which R. R. Williams formed for that purpose. It was part of my job to report monthly progress to this group (about 8 top names) and receive suggestions. In addition I was responsible for the bimonthly report to the Research Board for Dr. Dinsmore who, I believe, chaired this Board.

BOHNING: All of this information exchange was very helpful I would think?

FULLER: Yes, our office with Lester Friedman and the two secretaries which we maintained in the Municipal Center was essential to handle all the paper work, because Williams and I were away a lot. Often when we returned, we found no place to sleep. After some interesting experiences, which I would not dwell on here, we rented a permanent room at 1616 16th Street.

BOHNING: Did Bell Labs also have a contract? I think you mentioned that earlier.

FULLER: Yes, we did.

BOHNING: How did that work?

FULLER: Williams attended to that and saw to it that capable persons were chosen. It may look like a conflict of interest, but there was talent available that needed to be employed. The investigation of the effect of gelation on GR-S by W. O. Baker and others in my research group, as I have mentioned, proved of considerable practical importance. I might say that I have not mentioned another contract that was made later in the program and which also proved to be of much value. That was the one with Cornell under Peter Debye. R. R. and others when they heard that Debye was in this country were very anxious to get him to contribute. After some clearance delay we did succeed. Besides being a delightful person and stimulating others in our program, Debye extended light scattering into a useful tool for the rubber program. I am happy to have shared in some interesting experiences with him.

BOHNING: Tell me more about Debye.

FULLER: I think I first met Peter Debye when I was chairman of the North Jersey Section of the ACS and we had invited him as a speaker. When it came time for his talk the house committee had failed, it turned out, to provide a lantern. It was at our other location five miles away. I apologized and said perhaps he, Debye, would fill in the time with some of his experiences. "No, I will just wait", he said smiling. It was then that Victor LaMer saved the day by giving one of the most amazing biographical sketches of Debye you can imagine - a half hour of detailed biographical introduction. The lantern came and everyone was relieved.

Debye was an unassuming, down-to-earth person. This was illustrated when a number of us were waiting for the bus to take us to Gibson Island one hot July day. We were all gathered very uncomfortably in the bus depot in Baltimore which was not air conditioned. Someone looked around and said, "Where's Debye?" We looked inside the station. No Debye. Then we went outside and there leaning against the cool brick wall was Debye, comfortably reading. There were an assortment of bums laying close-by, but that did not bother him. When asked why he didn't come inside he remarked, "If you want the coolest spot, search out the bums." Many of us who attended that Gordon Conference will remember the lively after-lecture session that Debye used to hold in the library of the Symington House.

A word about the Gordon Conferences, of which I have many other happy recollections. The first of these I attended was in 1935, and I think the first one at Gibson Island. Previous ones were held elsewhere under Professor Neil Gordon whose idea it was to have scientists and wives engage in both social and serious scientific talks in a relaxed and pleasant atmosphere. The lectures were part of the Johns Hopkins University summer school. As a result they were restricted and no reporting of the meetings was permitted. The 1935 conference was devoted mainly to synthetic and natural high polymers. There were 47 attendees. I think I attended all of the meetings before they were switched to the New England colleges. By that time they had already expanded to many weeks and now they have become country-wide gatherings. The original polymer group has divided into many special groups as well. The Gordon concept has spread to other professions, it is so popular. Books could be written about these useful meetings. I think someone has attempted a history but as yet I have not encountered any. I have vivid memories of the earlier ones and still think back to the time when Tom Midgley and his attractive wife hosted the Thursday night banquets. Midgley, who became president of the ACS, with his enthusiasm and means, was never duplicated during my time. I feel very lucky to have met and known so many outstanding researchers. Neil Gordon and his wife always stayed in the background, but no one ever felt they were being offish. At heart Gordon was a quiet and unassuming man, I found, and only wanted the group to benefit from each others' presence. He looked for no praise. He did step in when a few of us became too loud in the bar. But he simply walked through impassively in his bathrobe and disappeared. Once when I was waiting to give my lecture he whispered, Give 'em hell". The one topic in those days that could generate the most heat was whether the meetings should be taken up to New England to avoid the Baltimore flies and heat. "Yes" said Abraham Lincoln Marshall of GE. "No", said Emil Ott of Hercules. An argument that the North finally won.

There was latent talent among us that only surfaced at the post-lecture gatherings in the bar. Walter R. [Smitty] Smith was one of these and with coaxing he would perform. Even if you had seen his acts before, you laughed until your sides hurt. One of his skits had to do with a college roommate who wore a truss and always came in late, intending to slip into bed quietly. But invariably he would get tangled in his truss and turn the room into a shamble. As I recall, Smitty locked him out one night and the room-mate got his truss caught in the transom. Smitty with his little black mustache was funny just to look at, but in these acts he was hilarious. Another act I remember but will not expand on, was the detailed procedure he, Smitty, went through preparing for his week at Gibson Island. It was a monologue of Epicurean fantasy with him as protagonist. After his grooming by the hotel barber, he boards the Patriot in Boston. Starting in the lounge car, he dines, retires to his compartment with a good book, lands in Baltimore in time for a gorgeous luncheon at Winter's, and finally takes a limousine to Gibson Island.

I do not want to give the impression that we lacked serious scientific discussions at these Conferences. That is what we did mostly - even while swimming in the Magothy among the jelly fish. But the good fellowship was an important factor in the success of these meetings and a little light comedy helped.

Now getting back to synthetic rubber and reviewing a bit, the Office of Rubber Director was formed late in the Fall of 1942. Calls went out from Washington for help in organizing the Research and Development talent. There were a number of fairly well-known names mentioned, but it was not made plain who was to head up the R & D Division until R.R. and I went to the Office, I think on the fifth floor of the New Municipal center building, and saw for ourselves that Ray P. Dinsmore, V.P. in charge of R & D at Goodyear, was to be this person. We were to head up polymer research and now better understood what this would entail. Except for Ray Dinsmore, there was no one from the rubber industry heading up the work under him. Joe Elgin as head of equipment development was from Princeton, Carl Prutton as head of process development and Norm Shepard as head of compounding research were all independent. Prutton was from Case and Shepardwas from American Cyanamid.

Ray Dinsmore was a pleasure. He was quiet, friendly and considerate and well-qualified and I am sure had a lot to do with choosing the four under him. R.R. and he took an immediate liking for one another. The only bad habit that Dinny had was his pipe. But he seldom puffed on it and I concluded that it was a clever device for taking care of complainers. Dinny would smile and listen quietly to these types until they were talked out and courteously escorted to the door when the visitor would suddenly realize that Dinny had not promised to do anything. From a corner office and modest furnishing Dinny soon secured desks and chairs for a string of offices next to his and we were in business. Unfortunately, in part due to ill health, Dinny had to give up the post of Deputy Rubber Director on May 1st 1943. He was replaced by Colonel Bradley Dewey, head of Dewey and Almy Chemical Co. I remember the retirement party we had for Dinny quite well in the Statler. R.R. wrote a poem for the occasion which I still have. Dinny himself was an accomplished poet of the serious kind and he was impressed. Our research branch was well organized now; in mid-year I learned that R.R. too was resigning later that month, but would remain as consultant until February 1944. I was sorry to see this because he would not be replaced and the load on our branch was already increasing. But, by working late hours, we managed.

One of Dewey's objectives was to breathe new life into the ORD. He held several general meetings of the entire staff and tried to instill a high morale. The job was still far from done. He was particularly anxious to maintain good public relations. Quite a number of citizens, some quite influential, were writing in suggestions not only on how to make natural rubber but also on how to extend the stockpile we coveted. Nearly all outside suggestions were rather weird, like the fellow who had a process for making rubber from coffee. Still, more and more of these letters were sent down to our organization for reply. Most of these letters were referred to a separate office where they would receive polite thanks. A few had to be investigated and that meant one of us got the job. I think the complaint office was headed by Hez Simmons, a former president of the University of Akron, but I could be wrong.

I remember now only two of the occasions in which I was involved. One nearly resulted in my being thrown out of the 22nd story window of a New York skyscraper. The other got me a trip to Hollywood to the set where they filmed the "Boys from Syracuse."

The movie people were represented by Mr. Blumberg, president of Universal Pictures. He, as a public-spirited citizen believed they had a solution for the rubber shortage, something that was very aggravating to the people of California. The process, invented by a man named Jean, was very simple in principle: the government gives Jean a pound of natural rubber from the shrinking stockpile. He puts the rubber through his process now operating on the Syracuse set and it is made into two pounds of natural rubber. Putting the two pounds through the process will give four pounds, continuing in geometric progression. This would result in enormous production if the raw materials available from petroleum could be produced in sufficient quantity. The raw material needed was butylene and its isomers, easily available from petroleum. What Jean believed was that the original pound served as a model or template which the butylene molecules copied to form synthetic natural rubber. Actually, in practice, a full one pound of "appreciation" would not be achieved, but even an appreciation of 50% would lead to enormous supplies of cheap natural rubber.

The people promoting this scheme were so important that Dewey said he was dispatching a pessimist (me) and an optimist to investigate. The optimist was Professor Ernst Hauser of MIT, a rubber expert. When we saw the rubber processing plant, built of scenery parts from the huge Universal Picture's warehouses, full of every conceivable item, we were shocked. Under a huge open tent on the Syracuse set there were large vessels full of rubber (released by our office for experiment only) soaking in what was essentially gasoline or worse, to produce solutions of the new product - "educated rubber." The very flammable solutions were drained out of the vats in buckets and taken to a huge rotating cylinder heated by live steam from a steam generator, in turn heated by means of a large blow torch spouting naked flames. That was a short distance away, behind the set. I shuddered when I saw the men dumping the flammable solution on the hot rotating cylinder and mopping it back and forth until the excess butylenes were removed - supposedly removed - for here was the weak spot, some of the butylenes remained in the rubber. That was the "appreciation"; it turned out to be an appreciation which became less and less the more thoroughly the butylenes were removed. Adding oils to natural rubber during compounding was a well-known practice in the rubber fabrication art. The inclination of both Hauser and me when we saw the flames was to run away as fast as possible. One wondered about a passing smoker tossing a lit stub!

The Jean story went on and expanded into the U.S. Congress after I sent in my negative report. Hauser and I agreed on the futility of turning over our natural rubber to this process. He, however, stayed longer than I looking things over. I heard that he got Universal to film the entire process and give him a copy of the film, but I never knew whether this actually took place. I wish now I had had the time to look further into it.

BOHNING: Did Hauser get a copy?

FULLER: My recollection is that Hauser did have such a film made and that he was given a copy. As I say, if I hadn't been so busy I would have asked for a copy but I was anxious to get back to Washington and my job. I might add to what I have said above that it was my opinion at that time there was no funny business on the part of Blumberg or Jean in pushing their process. They thought that they were contributing something very useful. However, there would be large profits for someone if the process had been endorsed. Whether any capital was invested by some I don't know, but the cost to Universal must have been nearly a million. There must have been contacts made through the WPB (War Production Board) to secure the loan of the natural rubber.

BOHNING: What was the other suggestion?

FULLER: The second suggestion was quite different. A Mr. Z who said he had a process for making natural rubber from isoprene was being supported by a group of investors connected with the Lion Oil Co. They had sent a sample of what presumably was their product to the ORD and it landed in our office to handle. Our x-ray examination showed that it was mostly natural rubber or something very close to it. The plant was in Newark N.J. and took time off one day in the summer of 1943 to inspect it. It was a very hot day and when I arrived Mr. Z stepped out of a small office to greet me. He was wearing a heavy fur coat which reached to his shoes, which I thought was strange, but he was pleasant and we entered his cramped quarters. I noted there were a few sheets of natural crepe rubber on his desk which had been cut in places and I assumed that was where our sample came from. He took me out in back where he had set up a small pilot plant. There were many pipes and vessels and valves such as one might expect, but Mr. Z was unwilling to explain the process in any detail except to say that glacial acetic acid was used as a catalyst. I had read that strong acids like glacial acetic, HCl, etc. had been known since before 1900 to turn isoprene into a rubber-like substance, but it was of no value. Mr. Z not wanting to divulge more, I left. I reported that without a closer inspection and without more information I did not see how the ORD could recommend that the project get government backing.

Unfortunately that was not the end. One Saturday a few weeks later, I got a call from R. P. Dinsmore, I believe, saying he would very much appreciate it if I would go into New York to such-and-such a building and discuss the project with the interested group. So I went, found a door labeled "Lion Oil Co." and entered. I found about a dozen serious-looking business men milling around inside. Without delay the spokesman for the group told me he wanted me to tell Mr. Z face-to-face why the rubber was unacceptable. At that point Mr. Z was brought in. I was struck by the fact that he was still wearing the heavy fur coat. I quickly assured the group that we were convinced the sample we saw was indeed natural rubber but that Mr. Z had refused to let me see it produced in his plant. Until then we could not be expected to endorse the process. A few were more sympathetic with me, but the majority of the group were still very angry - and angry at Mr. Z. I tried to explain that isoprene had so far never been converted to the regular cis-type structure characteristic of natural rubber. [We now know, of course, that it has been so-converted since by use of the Ziegler-Natta catalysts.] This, however, only made matters worse. When they learned that Mr. Z had refused to allow me to examine the process in action, they were furious with both of us. I was fearful that I was going to be tossed out the 22nd floor window, so I politely excused myself to the spokesman and left. That was the last I heard of the Lion Oil project!

BOHNING: I would like to ask you a little bit more about your relationship to Jeffers, Dewey and Dinsmore. You mentioned all three of them. What was your relationship with them during that time?

FULLER: Jeffers was of course the top man when the Office of Rubber Director was opened in December 1942. And Ray Dinsmore was under him as deputy in charge of Research and Development. I was deputy chief of polymer research for R. R. Williams until he left in May 1943 when I became head of one of the four departments (branches) reporting to Dinsmore. Dinsmore also left in May 1943 and Bradley Dewey took over as Acting Rubber Director. When Dinsmore left, Edwin R. Gilliland came in as Assistant Rubber Director in charge of R & D. That was the summer of 1943. He, like Dinny, was also Chairman of the R & D Board of ORD. [Not to be confused with the RDB (Research and Development Board) of the Department of Defense under which the Panel on Chemistry operated.]

BOHNING: What was the strategy of the research side of the Office of the Rubber Director?

FULLER: I mentioned that there were four branches in R & D. Our branch on polymer research which dealt with the synthetic rubber itself, also included research into the polymerization reaction; the Process Development Branch originally under Prutton with Richard Kitzmiller as deputy; the Equipment Development Branch under Joe Elgin of Princeton, which dealt with problems in plant engineering; and a branch on Compounding Research, originally under Norman A. Shepard. We all acted pretty much independently as far as meetings and reporting are concerned. However, we attended each other's meetings as often as we could in order to to keep ourselves informed of anything important. We did not want to miss anything we should be aware of. For example, Harkins at Chicago was doing important work on the theory of emulsion polymerization - determining where initiation of the reaction takes place and what the soap micelles contributed to the reaction. This picture was important for the process development chemists to know - something they would be informed about on attending our meetings. The mechanism of the modifier reaction in controlling the average chain length of the polymer, the mechanism of the formation of "popcorn" polymer, the mechanism of catalytic initiation of the reaction, all of these subjects were of crucial importance. The development of analytical tools by Kolthoff's group is another example.

Much valuable information was obtained from polymerization in bottles -- a technique worked out especially by C. F. Fryling of Goodrich (later of Phillips Petroleum Co.). C. S. Marvel's group, as well as several others, used this technique to advantage. The effect of temperature and composition on the properties of the polymer was investigated. Later the advantage of low temperatures was demonstrated and passed on to development.

BOHNING: Did you exercise much control over the contract that you had?

FULLER: As to financial control, we were able to negotiate contracts. But the Research and Development Board was the approving agency. We actually conducted a very economical program for the country. I don't remember any criticism of the way the funds were spent. Sometimes it could be embarrassing as, for example, when Debye was taken into the program at such a low figure. Williams of course was the negotiator for our group. When he asked Debye to estimate how much he required. He said, "Only enough for the services of one assistant." "What do you estimate that would be?" "I think that $8,000 for a year might do very well." We were taken back at this obviously low figure for the Nobel Prize winner. But of course we could not suggest more. I think I saw a figure once of about three million dollars a year for the entire ORD R & D program!

BOHNING: What was the background of your return to the Bell Labs?

FULLER: I simply wrote a letter to R. R. Williams, who was still my boss, that I thought I had served my time and wanted to get back in harness again. I received a letter from the Labs President, Oliver E. Buckley welcoming me back when I could get away, with praise for a job well done.

The war was not over, but the rubber industry was rapidly solving its problem of supply and there was little doubt who would prevail. By late July 1944 I was cleaned up at my office and ready to go. I had arranged matters with Ed Gilliland and he had agreed that, provided I would be open for consultation if needed, it was all right with him. He wrote a glowing letter to Buckley about my service and hoped that I would be available for consultation.

I was also tied to Washington by another string. I was Chairman of a group called the Elastomer Sub-Panel which was attempting to get some concerted action from the Army, Navy, and Air Force materials laboratories on the problem of elastomers which would be more suitable for Arctic applications. This sub-panel was not in the ORD but came under the Panel on Chemistry of the RDB [Research and Development Board] under the Department of

Defense. The RDB Chemistry Panel was chaired by W. E. Hanford of the M. W. Kellogg Company, where he was vice president in charge of research. Butch, as Hanford was called, was an organic chemist from the University of Illinois and only agreed to serve when the RDB had something they wanted done. He was right! A great deal of time was wasted because no definite objectives for the advisory group had been set. I had already (1947) served on an ad hoc committee, I believe under the Chemistry Panel, with Harry Fisher of U.S. Rubber and Norman Shepard of American Cyanamid. We reported our recommendation at that time, but I have forgotten the details now. As a result of this service, I was appointed chairman of the above Sub-Panel on Elastomers under the Chemistry Panel. The two other civilians on this Sub-Panel were Ben S. Garvey (Goodrich) and Al S. Carter (Du Pont). Representatives from the three services and from ONR made up the rest. The Army in particular was feeling very insecure about the behavior of their synthetic-rubber based items if we should have to protect Alaska and the arctic regions in a war. We civilians wrote a report after meeting together only a few times. Unfortunately I have been unable to find a copy of this in my files. What we had hoped for was the formation of a group, including civilians, the ONR (Office of Naval Research) and representatives from the three military services, which could draw up a plan for a cooperative attack on these important material problems. Unfortunately, the old rivalry, which had been evident in the older Army/Navy Joint Committees, came to the surface again. I was chairman of this sub-panel and felt particularly sorry that I was unable to control the second or third meeting of this group. The representative from the Quartermaster Corps in particular destroyed any cooperative spirit which might have taken root. We three civilians were too busy to spend time trying to settle inter-service disputes, and after writing our conclusions in a letter to the R & D Board, we turned in our resignations. This was the summer of 1951.

It was time that my service to the government ceased. I had been trying to do the government job (which the Labs approved of) when there was plenty going on at the Labs which affected my future. My superiors at the Labs were supportive of my serving the government. However, my service had gone on since 1942 and I could see that I was not helping my job by serving any longer in Washington. Besides, the work on plastics and other synthetic polymeric materials was increasing rapidly and needed all I could give it.

Let me go back in time for a moment to 1947. I have mentioned before the unsatisfactory situation in regard to the way the responsibility for the research and development work on materials was divided between the Chemical Laboratories and the Apparatus Development Department at Bell Labs. Now that the war was over it seemed a propitious time to correct this. Because of the long standing uncertainty, the task was not going to be easy. It fell upon our president Mervin J. Kelly to make this move. He called together the two departments involved and explained what was proposed and why the change needed to be made. The details of the reorganization were apparently left to R. M. Burns, who was now Chemical Director (Williams having retired) and J. R.

Townsend, head of the materials group in the Apparatus Development Department. At no time was I consulted as to where I would land in the new organization. Burns and "J.R", as Townsend was called, were to become common heads of the Chemical Laboratories, but it was plain that Townsend would be in the weaker position, except in our plastics area. And so it proved, because I was the only group head that had to share responsibility for its direction with J.R. who, as I mentioned, was also head of the Chemical Laboratories as a whole. This made me feel uncomfortable. I was on probation whereas the other group heads were not. I also felt that Burns could have prevented that had he wanted to. Gradually I was being forced out of the plastics job, but I was too busy to realize it fully. It was partly my fault because I did not break off my Washington and Society connections sooner, as I have said before.

To make a long story short, I was offered two choices: 1) to go along with the organization changes I didn't like, or, 2) to switch into a new field, leaving forever my 15-year tour with polymers. The new field being one I knew only slightly, namely semiconductors. Ralph Bown, then Director of Research, was sympathetically confident I would fare the best choosing the latter. He was right.

By 1950, however, after I had been assigned to several other groups within the Chemical Laboratories, I was well into the new field of semi-conductors and ready to do research on my own once more. I believe I have covered some of this previously, Jim.

BOHNING: What happened to the Polymer group after that?

FULLER: By 1951, Bill Baker, who had stuck with me doing polymer research during the 1947 changes, was advanced to Department status in charge of High Polymer Research and Development. Bill was on his way up. Many other changes were effected. Also in the Chemical Laboratories, Burnard Biggs was made head of Plastics and Rubber Engineering. I had lost some time, but soon was deep in my new researches, which were to give me 15 years work in semiconductors and a modicum of honors.

The way things turned out, I figured that it was better for me to switch over. The transistor had been invented in 1948 when Walter Brattain and John Bardeen put the first points down on germanium crystals. Bill Shockley added the crowning touch with the invention of the pn-junction transistor. Everyone close to the transistor project knew it was going to be a tremendous development. I had the opportunity to go into it as a solid-state chemist. I was familiar with some of the work going on, but I didn't know solid-state theory very well. I had to re-educate myself on solid-state chemistry and solid-state physics. William Shockley and John Bardeen were developing the theory (14). The British scientists had come up with the basic theory of metals, which also applies to semi-conductors. The band theory of metals was well established at that time by N. F. Mott, R. W. Gurney, A. H. Wilson, and others. Semi-conductors, which are similar to metals in their band structure, were amenable to this same theory. I worked on the theory as best I could but I immediately began experimental work relating to the surface properties of germanium. Two effects interested me there: one was the surface properties of germanium as they affected the electrical behavior of the very pure crystals that were being grown. The other was a curious property of germanium that nobody seemed to understand called "thermal conversion." This was a fascinating puzzle to me. I spent some time thinking about it and investigating it. I found out that it had to do with the way people were handling the crystals when they etched and washed them. If one took very great pains to make very pure water, better than conductivity type, and then looked for this thermal effect in the germanium crystal, it did not happen. That is, if you were good enough to use extremely pure conditions of preparation. The crystal was so sensitive that if you went into a laboratory and grabbed the doorknob and then happened to lightly touch the crystal, it would convert; that is it would change type from n to p on subsequent heating above about 500°C. With the highly purified water as a post-etchant rinse the effect would not occur.

I and some of the other chemists, including a radiochemist, J. D. Struthers, spent a fair amount of time working on this problem. We showed that there was something in the ordinary water supply which was responsible for the effect, and that it was likely to be copper. We had eliminated iron and a number of other elements, so that copper seemed to be the main agent. Obviously something was getting into the crystal extremely fast when it was heated above about five hundred degrees. So with the aid of a radiotracer ($^{64}Cu$ of half-life 12.8 h.) that Kathy Wolfstirn and Jim Struthers could provide, we did some experiments together and published a paper in which we proved that copper was the rapidly diffusing agent in the crystal. Copper was creating "acceptors" in the crystal and converting it from n type to p type. We determined the rate of diffusion and other properties of copper in germanium. People were then aware of what might happen in the preparation of transistors and diodes. What happened for example, if you heated these devices under contaminated conditions? Kathy Wolfstirn, an expert in radio-tracer chemistry and techniques, was invaluable in the radiochemical work.

This work on copper led to a search for other fast-diffusing agents and to an examination of diffusion in general. We went into the diffusion of the Group III and Group V elements and we published papers on them (15). The determination of the diffusion coefficients of these active impurities in both germanium and silicon was done with the help of John Ditzenberger. We found that lithium was an intermediate rate diffuser, not as rapid as copper but much more rapid than boron and some other elements. By contrast, lithium was a donor. Since the active elements of Group III were acceptors and those of Group V were donors in silicon and germanium, there was quite a lot of interest. Many outside companies were already making transistors and diodes and other electronic elements out of silicon and germanium. So we had a considerable demand for reprints of our publications. The Japanese, as you might guess, were particularly interested. Some work on diffusion had also been done at the General Electric Labs (16). We were not the first to show that diffusion can lead to active areas. But GE apparently did not follow up on their finding whereas we did. For some time transistors were made by diffusion and this process is still used for some devices today. More recently ion implantation and epitaxial deposition have been preferred for transistor production.

When we were first developing the diffusion method (1950), I got the feeling that the Labs was not fully appreciative of the possibilities and were slow in backing our work. I wrote a letter up the line at the time to that effect, but it apparently didn't stir up matters. I suppose one has to consider the fact that many more important decisions had to be made at that time. [Fuller refers to Letter 7 March 1951, case 37939, Bell Laboratories.]

The photovoltaic cell (solar cell), for example, is a very simple application of the diffusion method which the Japanese have exploited to their profit in the hand-held calculators and in their cameras. We were very slow in seeking out such applications. The diffusion process permits one to make large surface area pn junctions cheaply. The photo-generation of a voltage across pn junctions made by crystal growth has been known from the work of R. S. Ohl at the Labs since prior to WWII. Ohl, however, did not have a thin <u>surface exposure to the junction</u>, but rather an edgewise one. Consequently, the efficiency of his arrangement was quite low. Besides his junctions were much more costly and difficult to fabricate. Gerald Pearson was aware of these facts and, following some work of mine, was attempting to make surface junctions using lithium. Unfortunately the rate of diffusion of lithium is too high in silicon to be useful for forming diffusion junctions. So he came to see me one day to find out what might be done. We had studied diffusion rates of most donors and acceptor elements and I immediately suggested we try phosphorus and boron which I knew formed permanent junctions for room temperature use. We tried these and found that very thin pn junctions could be formed in silicon that probably would yield good photocells. Then it appeared that Daryl Chapin had also been stimulated to make similar cells as sources of power for remote telephone lines. We three joined forces and the result was that we were able to make permanent cells with an efficiency of nearly an order of magnitude better than the best selenium or copper oxide photocells in popular use (17). This finding caused our bosses to take notice and the subsequent publicity, which exceeded that of the announcement of the transistor, was unbelievable.

BOHNING: What year was that?

FULLER: That was 1954. It caused such a big public reaction because everybody was familiar with the sun and its energy whereas "transistors" was a foreign word to most people. Since we were a telephone company and a monopoly at that, we did not want to appear to be getting into the power business. But we knew that the transistor would take some time to be appreciated and when it was, its inherent value would persist for a long time. This has proved to be so beyond all expectations.

BOHNING: Could you talk about the division of development work on that solar battery?

FULLER: Yes. We obtained up to 12% efficiency on small-area solar cells. Further improvement went to our development people and involved larger-area cells, in which it is more difficult to get high efficiency. Most of this work landed in R. M. Ryder's and K. D. Smith's groups. I have here some of the early cells we made about 1952 (Fuller demonstrates). One can connect several cells in series as you do in the case of dry cells and so increase the voltage generated. Our development groups were able to get 12-15% on cells of almost one inch square in area. Even today after almost 40 years, 15% is considered a respectable efficiency. The early cells had about one micron thickness of diffused layer on the faces. Electrical contracts are made to this layer and to the base silicon, the latter by etching through the diffused layer prior to soldering on the lead.

Unfortunately the power capabilities of the solar cell have been overstated. The Energy Department led people to believe solar electrical energy would by now (1986-87) compete with electricity from coal and nuclear generation when this was not true at the time and not true now. Much money has been spent on research and development to try to make this come true.

Estimates for solar power as low as $1,000 of capital cost per kilowatt were far too optimistic. $10,000 to $15,000 per kilowatt would have been more reasonable. The cost of steam/coal generation probably averages $1,000 with nuclear somewhat higher. I don't know why the DOE put out these estimates which were obviously faulty. Perhaps they were figuring on new advances from the research in progress. Some very sizable installations of silicon solar cells have been built, but I am not familiar with what conclusions have been reached. We never believed or intended the cells would compete with coal or nuclear plants at the present stage of development. But solar cells are a great source of power if you live in the desert.

BOHNING: I think it was used in the early satellites too.

FULLER: Yes, we were the first ones to put up a satellite, Vanguard I that had a solar cell in it. And interestingly, the Russians also came along with one only about five months later. That, of course, is where the solar cell has been used successfully and where the big development has occurred--in the satellite programs. If it hadn't been for the satellite, there wouldn't have been anywhere near the amount of money invested in it. The amount of research money looking for an alternative to silicon which would do the same kind of job (or better) has been many millions and as yet, to my knowledge, has not yielded anything better than the silicon solar cell. Gallium arsenide is mentioned often, both as a photocell and as a substitute for silicon in transistors. Certainly, it has the characteristics which will promote it for certain special jobs. We did some investigating of gallium arsenide. It is a difficult material to work with compared to silicon. It is much more fragile and may have to be supported on a substrate. But some of its properties, particularly the high mobility of electrons in it and its ability to withstand cosmic radiation makes it superior to silicon in some applications. So, probably we will be hearing more and more about gallium arsenide. It's been very slow in coming.

BOHNING: This has been thirty years?

FULLER: Yes. Thirty-five years.

BOHNING: I want to ask you more about Chapin and Pearson. How did that relationship work out?

FULLER: It was very interesting because people like to speak of teamwork when they talk about research. This applies more to directed research. The solar cell just sort of happened and had none of the aspects of team effort. I had known both Gerald and Daryl for years before the solar cell brought us together. They were in different departments and doing different kinds of work. Pearson was active in much of the experimental work of Shockley's group. He was concerned with the physics of transistors, diodes and semiconducting crystals. I was in solid-state chemistry examining the properties of defects in crystals, especially defects introduced by diffusion. We were all working in separate buildings. I have already mentioned that Daryl was looking for local sources of energy that might be used for powering remote telephone lines. I knew both Daryl and Gerald, but until the solar cell came on the scene, I had never shared in a common project with either. Photocells were one energy source that Daryl's group had considered, but available cells were not sufficiently powerful. Gerald and Daryl got together when they heard that I had made pn junctions in silicon by diffusing in lithium. However, as I have said, these cells proved faulty and Gerald sought out my advice. Because I knew that lithium diffused too fast in silicon to be of value for a stable pn junction, I suggested that we try boron or phosphorous. We prepared some cells using these and Gerald tested them and found we could get 6% efficiency. That was nearly 10 times better than any other photocell up to that time and a significant advance. With a little more work we easily improved on the first cells doubling the output to 12% These were small area cells however.

Although this was not team research in the usual sense, it was possible because the Labs policy did not require us to get the permission of our bosses to cooperate - at the Laboratories one could go directly to the person who could help. Of course all of our bosses were brought in once we were sure of our results - that we had found something we thought was significant. This freedom to communicate on all levels, I am convinced, has been the secret of the Bell Labs success in research. For development work which is usually goal-directed, working as part of a so-called team may well be more effective.

BOHNING: I would like to go back if I might to the situation you found when you came back from ORD. Things had changed at Bell Labs.

FULLER: Yes. The war had caused a lot of disruption and a sorting out period was needed, which required time. R. R. Williams had retired in 1946. I was still tied to Washington part time. My two dependables: Bill Baker in polymer research and Carl Frosch, who had been so versatile in adapting plastics for military use, both had much work to finish. But by 1947 we seemed to be back. I submitted what I thought was a forward-looking prospectus for the future of polymer work. At the same time there were some behind the scene changes taking place. R. M. Burns who had been Assistant Chemical Director and who became Director when R.R. retired in 1946 now had a partner in the form of J. R. Townsend, the former head of materials for telephone apparatus development.

As I have said, in 1947 I was in charge of all work on plastics including polymer research. However, because J. R. Townsend was put in the same position with me (and also was assigned the top position with Burns, as head of the Department) I felt I was being put on probation and, while accepting my fate, was not at all happy with the arrangement. This situation came to an end during the next year or so when I made the move into semiconductor research as I have described.

BOHNING: What happened to the polymer group after that?

FULLER: Bill Baker continued to finish up the work on the contract with the ORD which he had underway and continued with his other research on polymers. When the reorganization came, his group was elevated to department status which was very nice, but I was removed from close contact with it. Mostly because I was busy in ways I have described and not because I was not allowed to be privy to what was going on in my old group. Burnard Biggs was also elevated to department status in charge of plastics applications work which was also a good move. These changes came as a result of the reorganization in 1950 in which, of course R. M. Burns had a big hand. Before that Baker and I were still working on the publication of material we had collected in 1946. I was involved with Biggs in the Paracon project in 1943. Some of this work was not published until 1947.

My last polymer publications were in 1948 and 1949. After that I was launched on my new career in semiconductors. I fear I am boring you with repetition here.

Bill Baker and I remained good friends, but we were seldom in contact except for a chance meeting in the halls. He was fast becoming a rising star and it wasn't long before he became Vice President in charge of all research and later President.

BOHNING: Did you keep in touch with him? You said you left him in charge when you went to ORD.

FULLER: I mentioned that he and I had papers to write and many things to attend to after the war. We were in touch of course during the time of the Labs' contract. But after 1947 his group was expanded. A new Ph.D. from Cornell, Field Winslow, now editor of Macromolecules, came and Bill and he went into other polymer projects. I was aware of, but not involved in that work.

As I have described, I was also trying hard to rid myself of ACS and Government obligations. I had, since the early 1930s, been active in ACS: counselor, publication committee, divisional and local section chairman. I was also involved with a Gordon Conference as chairman. Now that I was changing fields I had to establish other interests.

BOHNING: You were Phi Beta Kappa. Was that at Chicago?

FULLER: Yes. But aside from keeping up membership I have had little activity. The same is true of Sigma X where I have a sympathy for the fine GIAOR (Grants-in-Aid of Research) which they sponsor.

BOHNING: Is there anything else that we should talk about that we've overlooked?

FULLER: I think we've probably said more than we needed to.

BOHNING: Then I would like to thank you very much for your time.

FULLER: You're very welcome, Jim.

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