THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

PAUL S. GREER

Transcript of an Interview
Conducted by

Peter J. T. Morris

in

Chapel Hill, North Carolina

on

13 November 1985

(With Subsequent Additions and Corrections)

P. S. Greer

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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(Date) Nov. 5, 1989

(Revised 20 February 1989)

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

PAUL S. GREER

1904 Born in Braddock, Pennsylvania on 28 November

Education

1925	B.S., chemistry, Grove City College
1927	B.S., chemical engineering, Case Institute of Technology
1932	Ch.E., Case Institute of Technology

Professional Experience

1927-1942	Chemical Engineer, Carbide and Carbon Chemical Corporation
1942-1943	Senior Industrial Specialist, War Production Board
1943-1950	Chief, Polymer Development Branch, Office of Synthetic Rubber
1950-1955	Chief, Research and Development Division, Office of Synthetic Rubber
1955-1957	Engineer, National Science Foundation
1957-1974	Physical Science Administrator, U.S. Army Research Office

ABSTRACT

Paul Greer begins his interview with a short description of his upbringing in western Pennsylvania, where his father operated a successful business college. Greer studied chemistry at the small Grove City College, but with one year at Carnegie Institute of Technology, and then continued further studies in chemical engineering at Case. The years up to the outbreak of World War II were spent with Union Carbide, working on the early development of petrochemicals. Greer then moved to Washington D.C. to join the War Production Board, but, soon after, transferred to the Office of the Rubber Director where he played an important role in process development and product quality of the butadiene-styrene rubber, GR-S. During this section of the interview technical details of the wartime program are discussed and the contributions of individuals assessed. Greer stayed in Washington after the war, eventually becoming head of research and development for the Office of Synthetic Rubber. He elaborates on the balance between natural and synthetic rubber and the effect of the Korean War. The roles of cold rubber, oil-extended rubber and masterbatch rubber are explained as well as the patent actions with General Tire & Rubber Company. Greer reviews the lessons of the government rubber program and mentions the important individual and corporate contributions. After the wind-down of the government program, Greer joined the U.S. Army Research Office and he describes his functions during this final stage of his working life.

INTERVIEWER

Peter J. T. Morris is currently at the Department of the History of Science and Technology of the Open University, where he is Royal Society-British Academy Research Fellow. Morris was educated at Oxford University [B.A., chemistry (1978); D.Phil., modern history (1983)], and he was a Research Fellow at the Open University from 1982 to 1984. During the period 1985-1987, Peter Morris was Assistant Director for Special Projects at the Beckman Center. He is author of the monographs, Archives of the British Chemical Industry, 1800-1914 and Polymer Pioneers; his volume The American Synthetic Rubber Research Program is due to be published by the University of Pennsylvania Press in December 1989.

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INTERVIEWER: Peter J. T. Morris

LOCATION: Chapel Hill, North Carolina

DATE: 13 November 1985

MORRIS: You were born on 28 November, 1904 at Braddock, Pennsylvania, which is near Pittsburgh.

GREER: Ten miles out.

MORRIS: What was your parents' background?

GREER: My father was the founder, owner, and manager of a business school which he started in 1899 and carried on until his retirement in 1940.

MORRIS: Did your mother ever work?

GREER: Not outside the home; she was a full-time, superb homemaker who was never ill. My mother was born in Johnstown, Pennsylvania and graduated from Pittsburgh Female College, which is now known as Chatham College. She married my father in 1893 in Johnstown, Pennsylvania. My father had been a teacher at the Morrell Institute there.

MORRIS: Where was your father born, by the way?

GREER: My father was born in Mechanicsburg, Brush Valley in Indiana County, Pennsylvania. He graduated from Iron City Business College in Pittsburgh, where he had taught three years. Then he went to Johnstown and with M. E. Bennett helped organize a business school known as the Morrell Institute. They survived the catastrophic Johnstown flood of 1889, mainly because the Institute building held fast against the onslaught of the torrent. During 1895-98 my father organized and conducted short courses in subjects such as business English, mathematics, bookkeeping, and penmanship in twenty six towns in western Pennsylvania. In 1898 he settled permanently in the Braddock area and founded the Greer Business College, which he directed until it closed when he retired in 1940. After retirement he

still worked as a part-time private tutor and as a professional calligrapher, engrossing diplomas, testimonials, and resolutions. He was a charter member of the Braddock Rotary Club, and the vice-president of the North Braddock Board of Education from 1925-35. He was clerk of the sessions with the Braddock United Presbyterian Church and a member of the board of missions and a delegate to the Congress of Missions in 1930. He died on 6 July 1947.

MORRIS: How old was he when he died?

GREER: He was 87.

MORRIS: Was his business school successful?

GREER: It was successful up until the time he closed it. Then he got too old to run it anymore.

MORRIS: The Depression didn't affect it at all?

GREER: The Depression took it down, but he kept it going and didn't close it until 1940.

MORRIS: Presumably you spent all your childhood in Braddock and went to school there.

GREER: My mother's maiden name was Jean Cooper Harris and she was the granddaughter of James Cooper, for whom Coopersdale, a Johnstown suburb, was named. Our family moved from Braddock to North Braddock in 1912, after which I attended North Braddock High School and graduated in 1921.

MORRIS: What kind of secondary education did you have? Was it a good education do you think?

GREER: The high school education was quite good. I had four years science and four years of mathematics. At that time I developed an interest in physical science subjects; hence, I emphasized those in my college work. I was influenced by one of my high school teachers to enroll at Grove City College in 1921. After a year there, I thought that I would prefer to go to an engineering school so I transferred to Carnegie Tech for my sophomore year, but by the end of my sophomore year I decided I would rather concentrate in chemistry so I returned to Grove City College and graduated from there in 1925. At that time I was

offered a fellowship for further study in chemical engineering at the Case School of Applied Science. So I continued my education there for an additional two years, which fulfilled the requirements for a chemical engineering degree.

MORRIS: You also have a B.S. in chemistry?

GREER: I have a B.S. in chemistry from Grove City.

MORRIS: Why did you decide to do a B.S. in chemical engineering, rather than doing a higher degree?

GREER: Because I wanted to get more basic engineering training than I would have obtained just going on for an advanced degree in chemistry.

MORRIS: You didn't feel equipped to go on for an advanced degree in chemical engineering straight away?

GREER: No, I didn't have the necessary undergraduate chemical engineering subjects.

MORRIS: What was Grove City like as a college?

GREER: It's a nice small college. It's grown steadily since I went there and it presents a more complete picture, campus-wise, with the new buildings that were provided with the help of the Sun Oil Company. They've also expanded their courses to the point that they now give a well-qualified chemical engineering degree.

MORRIS: When you went to Grove City College, presumably the chemistry department was quite small then.

GREER: Yes, they had just two chemistry teachers. The head was a very well-qualified man, Dr. Otto J. Sieplein. He had had Bachelor's Degree training at Case and had Ph.D. training in Germany [Bonn; ed.].

MORRIS: Who was the other?

GREER: The other was Creig Hoyt, who had a Master's degree from Cornell [later, a Pittsburgh Ph.D.; ed.]. He was very competent.

He taught physical chemistry. They both taught general chemistry and Sieplein taught organic chemistry.

MORRIS: What kind of reputation did Grove City College have in chemistry? Did it have a good reputation for chemistry?

GREER: It had a good reputation as I can affirm, because when I went to Carnegie to present my credits, the chemistry reviewer said, "You had chemistry under Sieplein?" "Yes" "There's no question about your general chemistry being acceptable."

MORRIS: Yes, what was your overall impression of Grove City?

GREER: It [my time] was well spent. I went back to Grove City and finished there. I thought they gave a very good undergraduate education.

MORRIS: What about Carnegie Tech, having gone to Grove City...

GREER: Carnegie Tech: there was a lot of competition there. It was considered the more disciplined, especially in the mathematics department. Mathematics was the department that they prided themselves on, having it so rigorous that they flunked out a great number of people. I did all right with it and I congratulated myself on finishing up there with a good record, with no demerits. However, I thought that since I was going to go for chemistry, and I liked the chemistry in Grove City, then I'd just go back to Grove City.

MORRIS: One question I meant to ask earlier; this teacher who influenced you to go to Grove City, was he himself a graduate of Grove City?

GREER: Yes, he was. James W. Cameron, Grove City '16.

MORRIS: You presumably went to Case because your professor at Grove City had been at Case?

GREER: Yes, he gave me a good recommendation, and he was well-known at Case and that helped me get a fellowship there. In those days, fellowships were very rare. They didn't hand them out like they hand them out today.

MORRIS: Who was the leading light at Case?

GREER: The head of the chemistry department was A. W. Smith and his understudy was W. R. Veazey. Veazey got his Ph.D. from Johns Hopkins and, I've forgotten where Smith got his. They were quite active in consulting work for the Dow Chemical Company.

MORRIS: Case, of course, is in Cleveland.

GREER: Now it's merged with Western Reserve. Those two schools were adjacent -- separated by a fence, that was torn down occasionally, fought over, but now it's all merged. Why, that's all by the board.

MORRIS: Chemical engineering was still a comparatively new subject in the early 1920's. What was the teaching of chemical engineering like at Case?

GREER: The bible of chemical engineering, Walker, Lewis, & McAdams (1), was published just about the time I was at Case, so much of the chemical engineering was concentrated on descriptive courses for scaling-up processes. There wasn't such an intense concentration on mathematics as there is now.

MORRIS: You felt you got a good education at Case?

GREER: Oh, yes. The instruction at Case was topnotch. Classes were small and the instructors knew all the students very thoroughly. The assignments were very specific and very well followed.

MORRIS: Any students who made a great impression on you while you were at Case?

GREER: One of my roommates had graduated from Grove City in the class just ahead of me and had preceded me at Case. That was Ray Boundy. On graduation from Case, he went to work for Dow and went right up the ladder to become director of research. The other one who was at Case in my class was Nicholas Samaras. After finishing at Case he took his Ph.D. at Yale and later was employed by Monsanto and rose to director of research.

MORRIS: Any interesting stories about faculty from that period?

GREER: [laughter] The only thing I can think of is one of the

well-liked professors in the mathematics department was named Thomas. Somebody had given him the name of "Bunny Thomas" and when you asked why, they said, "He's the mathematics professor who multiplies very rapidly." [laughter] He was a very well-liked professor.

MORRIS: In 1927, after graduating from Case with a B.S. in chemical engineering, you went to work for Union Carbide at Charleston, West Virginia. Did you ever consider pursuing an academic career instead of going into industry?

GREER: I thought about it vaguely but at the time it seemed that in order to get anywhere in an academic situation, you had to have a Ph.D., and I couldn't see myself going back for that so I just stuck with industrial work.

MORRIS: How did you come to be employed by Union Carbide in particular?

GREER: That was easy. There were interviewers coming around to the college, talking to graduates and this Union Carbide situation looked the most interesting, aside from the fact that it was at least as remunerative as any of the other offers. Union Carbide had just started to make ethylene glycol and they were the first in the field to see the possibilities of making chemicals out of petroleum gases. That was going to be a growing area of manufacturing operations.

MORRIS: And that attracted you, I mean that made an impression on you?

GREER: Also it had a good reputation for the way graduates got along there.

MORRIS: When you were going to Union Carbide, did you have a background in organic chemical engineering?

GREER: No better than my training in college, no.

MORRIS: Union Carbide, of course, in 1927 had only just moved to Charleston from Clendenin further into the state. Did you hear any good stories of the "back woods" stage?

GREER: [laughter] I can't think of any to quote.

MORRIS: Were there any complaints about it like "it was horrible working there."?

GREER: No, I don't remember any.

MORRIS: What were the Charleston works of Union Carbide like when you went there? Were they still in a fairly rudimentary state?

GREER: The technical people at Carbide in Charleston were a relatively small group and like one big family. It was interesting in the sense that it was a very actively developing situation.

MORRIS: What were your first impressions when you got there?

GREER: My first impressions were that the plant was certainly well-maintained. Everything was kept up very nicely. There was no old rusty junk hanging all around. There was a great deal of welded piping running all over, for long distances, showing that they had taken full advantage of their knowledge in welding construction. They were using as raw materials gases under pressure and keeping them well-contained. Everything seemed to be in a high state of maintenance.

MORRIS: What about the town itself? Coming from Pennsylvania...

GREER: South Charleston itself was just a factory town. The town of Charleston was a very presentable looking town. I liked the looks of the river flowing through it. It had a picturesque appeal.

MORRIS: Who was your immediate boss when you first went there, who was in charge of you?

GREER: My first boss was Chester Heath. He was a chemical engineer who had worked for the gas company in Chicago. People's Gas, I believe that's it.

MORRIS: I think they founded the Union Carbide set-up, didn't they?

GREER: Well, Carbide drew a lot of men from them but I don't

believe that was so. Essentially it was originated by George Oliver Curme, who was the chief chemist at Carbide, in the New York office, at the time I was hired.

MORRIS: He wasn't working at Charleston himself?

GREER: He had done some work in the Mellon Institute in Pittsburgh and got the idea for making chemicals out of ethylene and it grew from that. Heath was very well-qualified and a very considerate boss. I liked him very much.

[END OF TAPE, SIDE 1]

MORRIS: You said that George Curme was working out of the New York office when you joined Union Carbide. Did you ever get to know him well?

GREER: Yes, I got to know him pretty well. He would come down to visit the plant quite frequently.

MORRIS: What was your impression of him?

GREER: Well, no doubt he was a very competent chemist, and he was a very active, productive, and energetic individual.

MORRIS: There's another person I'd like to mention, I think he was chief engineer of Union Carbide for many years, namely John Morehead.

GREER: I never had any contact with him. I didn't know much about him until I found out about the contact here.

MORRIS: You didn't know of him?

GREER: Yes, I knew of him, but that's all about I knew.

MORRIS: Who were your close colleagues in your early Charleston days?

GREER: There were three others who came to work at about the same time as I who were technical men and who started off in similar capacities. One of them was Clark Center, who later became one of the superintendents at Oak Ridge. Another was Benjamin King who became chief maintenance engineer at the South

Charleston plant. The other was John Urban who unfortunately met with a fatal accident during the first year of his service at Carbide.

MORRIS: What was your first major project, what were you set to do?

GREER: One project I was assigned was to develop a pH meter to operate under high pressure and temperature to monitor the hydrolysis of ethylene dichloride. I was sent to Buffalo in January, 1928 to work on this at the Linde Air Products Laboratory. I returned to the plant in August with an instrument that met the specifications of being serviceable under the pressure and temperature of the process. We subjected it to extended tests in the plant, but found that it could not be perfected to the point where it retained its calibration satisfactorily. However, in the meantime, the plant operations were changing so that under the new conditions of operation, this particular instrument was no longer demanded.

MORRIS: In that period did you ever come across the National Technical Laboratory, because they developing a pH meter...

GREER: Yes, they had a glass electrode pH meter. It came into production after this but it would not have been able stand the temperature or the pressure of this process.

MORRIS: What was the difference between their meter and your meter?

GREER: Mine depended on an antimony indicating electrode and a calomel half-cell. This project, however, enabled me to write a thesis on it and send it back to Case and it got me an advanced degree as a chemical engineer in 1932.

MORRIS: What did you do after the meter, what was your next assignment?

GREER: Another thing was running a unit for manufacturing supported catalysts for the dehydrogenation of isopropanol to acetone.

MORRIS: Was that very successful?

GREER: Yes, it made enough catalysts for their requirements.

[interruption]

MORRIS: While you were working in Charleston between 1927 and 1942, you worked on a range of products, from ethylene, ethylene glycol, right through to vinyl chloride. What particular products or processes stand out in your mind? What were the most significant over the years?

GREER: I wouldn't say anything was most significant. There were so many very different things, as it was a continual trouble-shooting job. We answered queries from the sales department about our products and there were quite a number of them, and we helped new processes get under way in the development department. We had to write specifications that would fit both the customers requirements and the production department's possibilities. We'd investigate different processes. For example, when we manufactured anhydrous products we worked up different methods for dehydration.

MORRIS: Did you develop any new products yourself?

GREER: I didn't develop any new products and I don't recall whether I was in charge of developing any new products. We had by-products such as ethyl ether that weren't suitable for sale until they had been refined. I worked on developing processes for that. If customers said that any particular product had to be of higher purity, I would guide the laboratory in developing such processes.

MORRIS: Talking of refining products, it reminds me of a witticism that I read here [Morris picks up book]. I'm trying to think who said it. It says here, "To Dr. Nelson Wickert."

GREER: Yes, Wickert, I knew him.

MORRIS: He said, "Curme invents messes and we fractionate them into good solvents."

GREER: That's true. Curme would dream up a process, he'd get a product out of it, but it would be a very crude product. We would have to clean it up then.

MORRIS: When did you first become aware of the direct process to ethylene oxide. You know, using a silver catalyst?

GREER: That came into being before I left Carbide. I left Carbide in 1942, but I didn't have anything to do with that process.

MORRIS: What about your work on vinyl chloride?

GREER: I didn't have a great deal to do with vinyl chloride at Carbide.

MORRIS: You didn't take the elaborate precautions in this process that they now take?

GREER: There were no precautions taken at all, no worrying about toxicity. We'd take normal precautions as you would with any chemical but we didn't consider it as being any more toxic than ethyl ether.

MORRIS: A story I heard in Germany when I went to a town called Hüls, which has become now the biggest PVC producer in Germany, I think. They said that in the early days, in the 1930s, the workmen working with vinyl chloride used to lose the fat from their skin, it was a very good solvent for removing skin fat.

GREER: If you get exposed to it I suppose it will. An hydrolysis I worked on had vinyl chloride as a by-product. Occasionally some of that would get out in the air. You'd just get away from it and get it hooked up again. I don't know of anybody down there who worked with it that was harmed by it.

MORRIS: Talking about vinyl chloride, did you have any contact with polymers at all? Polymerization?

GREER: Our laboratory was constantly working with the polymerization unit to help them control it. I don't remember any particular thing that I had to do with.

MORRIS: When you left Charleston in 1942 after fifteen years, what were the most striking changes since 1927? In the works, for example?

GREER: Of course, when I went there in 1927, the only products we manufactured then were ethylene oxide, ethylene chlorohydrin, ethylene glycol, Cellosolve, which was the monoethyl ether of ethylene glycol; those were the only products. As time went on there, one of the first additional products that was made was

isopropyl alcohol from ethylene and sulfuric acid. We made acetone from isopropyl alcohol. Then there was ethylene transformed into ethyl alcohol the same way, using sulfuric acid. The different esters, the ethyl esters, ethyl acetate, isopropyl acetate. Then there was of course propionaldehyde, acetaldehyde. There were some fine chemicals that we made in small quantities. So there was a multiplicity of compounds.

MORRIS: The work expanded enormously I suppose since 1927.

GREER: Oh, yes. Even glycol greatly expanded in volume. They used to think that five thousand gallons a day of glycol was a big deal.

MORRIS: What about the town itself? Did it change?

GREER: The little town of South Charleston was changing. People moved in and built homes. The town of Charleston was changing too. They built a statehouse they kept adding to, with a great golden pointed dome. Not only that, they had started to change the appearance of the river, the road along the river changed it quite a bit. For the worse if you ask me. They cut down all the trees and they built a boulevard along the river so it's all short of trees and shrubbery. Made the traffic flow swifter but it didn't have the nice appearance it had with all those trees by the river bank.

MORRIS: Was pollution a big problem by 1942?

GREER: They were beginning to worry about pollution but they really hadn't done very much about it by the time I left. They did take more samples at the plant to see what the effluent contained and to try to control it from getting out of hand. They hadn't established any big disposal plants, any waste filtration plants or anything like that.

MORRIS: You became a senior industrial specialist for the War Production Board in Washington. At what point in 1942?

GREER: I went with the government in July 1942.

MORRIS: And how did you come to be appointed to this post?

GREER: I had seen a lot of announcements for government jobs and applied for one of them and eventually I got an offer. I went to

the authorities at Carbide and said, "Here's a chance to go to Washington, and it's probably a war service assignment. Could I get leave of absence with the reservation that I could return to Carbide afterwards?" They said, "All right." So I went with the distinct understanding that they would hold a job open for me when the war service appointment terminated. That was the way that happened.

MORRIS: What were your particular strengths for the position you took? They wanted people for polymers and rubbers, didn't they?

GREER: The job at that time had to do not only with rubber but with chemicals generally. I went to a section of the War Production Board that had to do with civilian supply. We were to look after the essential requirements of civilians for a civilian industry. Chemistry that supported civilian existence, you might say, to see that it wasn't being drained excessively by military requirements. It was a matter of surveying the production capabilities and the demand on certain chemicals, making a balance and reporting.

MORRIS: It was basically a desk job?

GREER: It was a desk job, yes.

MORRIS: Did you come into contact with the Rubber Reserve Company or the Office of the Rubber Director?

GREER: While I was in the War Production Board the Rubber Program itself required some surveillance of what chemicals were required. We were in contact with the Rubber Reserve and were informed by them and so I knew about things to that extent. I was then approached by the Office of the Rubber Director in 1943 because they had a job there that they thought was more essential than what I was doing on the War Production Board proper. The War Production Board was superior to the Office of the Rubber Director, but the latter was given so much authority that they operated pretty much independently. It seemed as if the War Production Board was outweighed in clout then by the Office of the Rubber Director. When they made a request for me the War Production Board released me and that's how I happened to move over to the Rubber Director's Office.

MORRIS: Who did you work for in the War Production Board?

GREER: I reported to John R. Skeen, who had worked for United Gas Improvement Company and joined the War Production Board not

too much earlier than I did. There also was a man that was above Skeen at that time, Robert LeBaron, who was in charge of the chemical section at that time.

MORRIS: You only worked there for a relatively short period.

GREER: Nine months.

MORRIS: Did you have any lasting impressions of that period?

GREER: Well, it was a hectic period. The job wasn't especially well-defined except in a broad sense and I was very glad to make the transfer to the Office of the Rubber Director.

MORRIS: Did you know Dr. [Edward R.] Weidlein in that period?

GREER: No, I had met Dr. Weidlein but I had no contact with him at that time. I was influenced to go the Office of the Rubber Director by Dr. Carl Prutton, who had been my instructor at Case, and was then on leave working as a consultant to the Office of the Rubber Director.

MORRIS: What was his particular role as a consultant?

GREER: He was in charge of the copolymer process development branch at that time.

MORRIS: And that's why you went to work for that particular branch?

GREER: I worked for him indirectly but I worked locally for Dr. [Joseph C.] Elgin, who was at the same level as Prutton but was in charge of the copolymer equipment development branch.

MORRIS: I see.

GREER: He had an office in Washington so it was logical for me to move to his office. Prutton had an office that he established in Akron and he ran back and forth from Cleveland.

MORRIS: You were based in Akron?

GREER: His office was based in Akron, but I was temporarily assigned to Elgin while I was in Washington.

MORRIS: Was that all part of the Office of the Rubber Director?

GREER: Yes, it was.

MORRIS: What were your duties originally?

GREER: My duties were to do whatever I could help with on developing a process for making butadiene-styrene copolymer, which was then called general purpose rubber, styrene-type or GR-S.

MORRIS: How long did that go on for?

GREER: I was in that copolymer process development for something over a year. During that time I was sent around to visit the different plants one by one, until I got around to all the places with co-polymer plants. My most extended tour any one plant was in the GR-S plant in Torrance, California, in late 1943 and early 1944. I was in charge of a process development program at that point.

MORRIS: Who ran that plant?

GREER: That plant was run at that time by a Goodyear agent.

MORRIS: Who on the Goodyear side did you work with?

GREER: On the Goodyear side I worked with Carl Gay and Paul Linstedt.

[END OF TAPE, SIDE 2]

MORRIS: What did you do after you got finished with that?

GREER: After the Torrance plant tour was over, I came back to Washington and resumed activities in the office there, which now included frequent visits to a pilot plant and an evaluation laboratory that had been established by the Rubber Program at Akron and operated by the University of Akron.

MORRIS: When did that come into operation?

GREER: That got into operation with the rest of the program and was running well by late 1943. I was able to pay more attention to that after I got back from the tour of California. That pilot plant was equipped to study reactions on a scale from test tubes to batches of several hundred gallons. Big formulations could be made there, polymerized, taken through coagulation, drying and finally to evaluation in their testing laboratory. When the product looked sufficiently promising, batches could be made in quantities suitable for their tests.

MORRIS: What else did you do apart from working in conjunction with the polymer pilot plant?

GREER: It was all a matter of trying to improve the quality of the product.

MORRIS: Your role was essentially what one might call an administrative role; you saw that things went well.

GREER: But I also generated ideas and pushed them through. Of course, they generated a lot in the laboratory. It was pretty much administrative work.

MORRIS: Can you think of any of the better ideas that you thought of in that period?

GREER: Most of the ideas that worked out were not thought of by any one person but were group ideas. This is an example here. [Greer takes book] This is a history of cold rubber (2).

MORRIS: I'll be coming on to that later.

GREER: That was the primary achievement of the research in the program. All this time every possible variable was being considered and tested. In other words, changes and variations in the process conversion, change of the temperature of polymerization. Changes in type of catalyst, changes in monomer type, when we tried the different comonomers instead of styrene. Anything like that; we tried to explore all possibilities and variations. Out there in Torrance we tested varying the conversion in the plant reactors. Also varying the temperature of polymerization. If you dropped the temperature down a little bit within the capacity of the plant, you'd get a slightly better

rubber, but it didn't make enough difference in tire quality to justify cutting down on production to that extent.

MORRIS: Who was particularly associated with that work on the temperature?

GREER: Various people. One fellow that was resident manager of the Akron pilot plant was William K. Taft, he's dead now, and his boss was J. W. Schade, he's dead now, he was older than Taft. In the pilot plant in California development program, James Troyan looked after some of it. He looked after it for one period and I looked after it for another. A fellow named John Fennebresque looked after another part and he was largely responsible for changing the agitation to save modifier. There was a time when the modifier was being used at a higher consumption rate than was anticipated and he did some experiments with changing the agitation in the plant to bring it down to a minimum. He's dead now.

MORRIS: Fennebresque, I've heard of him before, would you like to say a bit more about him?

GREER: That was his contribution. He went out there ahead of my stay, but wasn't there when I was. He designed some agitator blades that had a shorter arm length, had a different paddle, a different speed, and made for an economy in the modifier, dodecyl mercaptan. He later went to work for the Celanese Corporation. He was very active in one job or another. He jumped from Celanese to one of the oil companies. He had risen to a pretty high position with the Mobil Oil Company when he died in an automobile accident.

MORRIS: You were talking at lunch about this German rubber.

GREER: That was sent to the Akron laboratory. They tested it there and they also sent it to one of the tire factories. They made tires out of it and we ran it on the tire test fleet.

MORRIS: How much of it did you have altogether?

GREER: They must have had something like a ton of it. Of course this was at the end of the war.

MORRIS: After the war ended? I was under the impression you got some before the war ended.

GREER: They had little bits of it before the war ended but not enough to really get a good test on it.

MORRIS: What was the general conclusion about the quality of the German rubber compared to the American rubber?

GREER: The conclusion should have been that it worked pretty well in tires, but processing it into tires would be the bugbear because our factories wouldn't go through that heat softening process.

MORRIS: To jump ahead a little bit, were you, and the other people on the project, surprised at how little the Germans knew about synthetic rubber when the war ended?

GREER: I don't fault them for not knowing as much as we did, except possibly that business about using softeners to make the rubber more processable. In fact, our cold rubber is based on an idea that Livingston got from them. They didn't have time to go ahead and test the effect of refrigeration of polymerization, as far as it related to tire testing.

MORRIS: Did you always work for Dr. Elgin throughout the war?

GREER: For a time I was reporting to Charles W. Perry who worked for Elgin. Then, in 1944 when the Office of the Rubber Director was abolished, I was transferred over to Rubber Reserve, officially the Rubber Reserve Company. At that point I started working for Dr. Evan Boss. He died at his desk about 3 years before I came down here, which was about 1955. Then some people were brought in on a rotational basis from the rubber companies. There was Dr. John N. Street from Firestone, Dr. Harlan Trumbull from Goodrich, Morris G. Shepard from U.S. Rubber, and Dr. [Albert M.] Clifford from Goodyear. During a successive six month period I worked for them in succession.

MORRIS: Did you know William Jeffers before he ceased to be Rubber Director?

GREER: No, I never Jeffers. I've met Bradley Dewey, but I've never met Jeffers.

MORRIS: Did you know Dewey well?

GREER: No, not well.

MORRIS: After Dewey retired, or resigned as Rubber Director in the fall of 1944...

GREER: Yes...

MORRIS: What happened, did the whole business get turned over to the Rubber Reserve Company? As you said, you went to work for the Rubber Reserve Company.

GREER: You see, the Rubber Reserve Company in the meantime was operating as a unit, mostly looking after the operation of the plants, supervising operations. The Rubber Director was responsible for making sure the Rubber Reserve Company got all its necessary building materials and equipment. Also, research was under the Rubber Director. The allocation of rubber was under him. But then when the Office of Rubber Director was abolished, the research was tossed over to Rubber Reserve to carry on and the rubber allocation was shunted on to the Department of Commerce, except that a good bit of the allocation was given to Rubber Reserve also. Rubber Reserve was pretty much in charge of rubber after 1944.

MORRIS: I suppose it was a bit before your time, but did you ever know Robert R. Williams at the Office of the Rubber Director?

GREER: No, but I met Williams. There was a distinction there between research and development. I was in development rather than research. Williams was in charge of the research.

MORRIS: What about Calvin Fuller?

GREER: Fuller was under Williams in research. I knew Fuller pretty well.

MORRIS: Any impressions of him?

GREER: A very competent, likable fellow.

MORRIS: What about Ray Dinsmore?

GREER: I knew him. I had met him a few times years before and I knew him in the rubber program.

MORRIS: Anybody else like that?

GREER: From Firestone there was Ray Dunbrook. I think he's dead now. Jim [James D.] D'Ianni, who was president of the American Chemical Society not too long ago, was active in their office, in charge of research in the Rubber Reserve, on leave from Goodyear for awhile.

MORRIS: Would you have to say anything about him?

GREER: He's a very competent and likable fellow. You might like to talk to him when you're in Akron. He's got an article in here. [Greer indicates document (3)]

MORRIS: Did you think you would be going back to Union Carbide when the war ended in 1945? Did you think your government job had ended?

GREER: At that time it didn't seem like it was ending, although it was tapering down, and there was a question of what was going to happen. It seemed very interesting to stay on and see what did happen; whether this industry would fall flat on its face. With natural rubber back would anybody want synthetic rubber anymore? Or whether it would keep going. I had an agreement with Union Carbide: I could extend my leave for awhile and so I did that. That kept on and I got my leave extended from 1945 through 1946 and 1947 and 1948. In 1949 I talked to them again, and they said, "Well, it looks as if you either ought to come back now, or not." So I said, "Well, I think I'll just stay where I am." So that's the way that ended, that's how I terminated all connections with Union Carbide.

MORRIS: How do think you would have stood financially? From a financial point of view would you be better off if you had gone back to Carbide?

GREER: I don't know, it'd be hard to tell. They would have taken me back and met the salary I was getting from the government, but they didn't offer me any big increase to come back. So that was another reason...

MORRIS: In about 1946, the Office of Synthetic Rubber was created. Do you remember exactly when was that created?

GREER: They jumbled that around back in the forties. It's probably in this book (2).

MORRIS: In 1945 the one great worry was that natural rubber was going to come back onto the market in full strength and that synthetic rubber was going to have a hard time of it. Can you shed any light on that?

GREER: This sort of tells the story here, pretty much. [Greer points to a report (4)] This is the fiscal year 1945, which is the year ending June 30, 1945. From that year we had a peak production because all plants were going all out for the war effort. When the war phased out later that year, the big impulse fell off some. Natural wasn't back strong yet, so synthetic was going along pretty well. But it began to cut in here [1946, 1947] with the natural [rubber] coming back. Then in 1948 you could get all the natural rubber that you'd want. Also, here's the price of natural rubber. Once rubber came down in price, and we were selling synthetic for fourteen and a half [cents/lb.], well, people switched to natural rubber. We began to make a little bit of cold rubber and then, in 1950, natural rubber suddenly went up in price because of the Korean War.

MORRIS: I want to ask about the Korean War period later. But in 1945, was there great concern that that synthetic rubber was going to be wiped off the map?

GREER: There was still enough demand for it. Orders didn't fall off. Also, we were still operating under the emergency regulations that required people to use a certain amount of synthetic. They couldn't get the natural anyway, so I guess they had to use the synthetic. The plants were kept running just to meet the requirements.

MORRIS: We're now in 1945, and you became chief of the research and development division in 1950. What did you do during that interim period? What kinds of activities were you involved in?

GREER: At the beginning of the year that I was in charge of research and development, I had to prepare one of these to go the Board of Directors of the Reconstruction Finance Corporation. [Greer gets report requesting approval of the annual program and budget for research and development]

MORRIS: This is after you became chief of the division. I'm talking about the period before then.

GREER: Before I became chief I helped to work these things up.

MORRIS: You remained in an administrative position regarding development?

GREER: Yes.

MORRIS: Any particular highlights of that period that stand out? Particular crises, breakthroughs or developments? I want to talk about cold rubber later, but is there anything else that stands out in your mind?

GREER: In 1945 to 1950?

[END OF TAPE, SIDE 3]

GREER: I can't think of any high spots. We were just trying to make continual improvements in the rubber properties and in the plant processes, but no outstanding events.

MORRIS: Just before we move on to your period as chief of division, is there anything you'd like to say about the kind of people who led the rubber project after the war? I'm thinking of people like, for example, like Leland Spencer and E. D. Kelly.

GREER: Leland Spencer. Let's see, when did he come in? He was an experienced man in the rubber industry from Goodyear. He was a very well-qualified individual, so it was altogether beneficial that he entered the program. This thing has the organization charts for various years. [Greer gets report]

MORRIS: Spencer came in about 1947, I believe?

GREER: Spencer preceded Kelly.

MORRIS: What was Spencer like as an individual?

GREER: Spencer was very competent. He was a good manager.

MORRIS: Did you know him before?

GREER: No, I didn't know him before.

MORRIS: What about E. D. Kelly, any comment about him?

GREER: E. D. Kelly and Spencer had both been over in the Department of Commerce rubber division. I don't know for how long, but, I think, Kelly for an extended time and Spencer for less. I think Kelly worked for Spencer in Commerce. Spencer came over and took charge and then, when he left, he got Kelly appointed as his replacement.

MORRIS: You must have worked quite closely with Kelly.

GREER: Yes, I worked closely with him.

MORRIS: Interesting person to work for?

GREER: He was good to work for. This is based on a memo that I wrote to Kelly. I just made that up to give to you today. [Greer passes paper to Morris (5)]

MORRIS: Thank you. Let's move on now to your time as chief of the rubber division.

GREER: By the way, just before I became chief I reported to Oliver Burke as chief. Burke had been in the office under a different division, but when these four men left, who had been on temporary leave from the rubber companies, Burke was appointed to be chief of research and development. He left in 1950, and that's when I took over.

MORRIS: Did you take over before or after the Korean War broke out?

GREER: The Korean War broke out about July, 1950.

MORRIS: And when did you take over?

GREER: I took over in October of that year.

MORRIS: So the war had actually broken out. The price of natural rubber went up enormously. Did that cause a lot of

upheaval for the synthetic rubber program in terms of having to get the plants started up again?

GREER: It certainly did, because plants had been shut down or taken out of service to get production down to that point. Suddenly the price of natural rubber went up and the industry cried out for more synthetic but there wasn't any because the plants were out of production. That caused a big hiatus, and of course, everybody and his brother was running to buy tires because they recalled how they couldn't get tires back in the war and they thought there would be another scarcity.

MORRIS: Did you have much to do with putting the plants back into operation?

GREER: I didn't have much to do in that sense, as it was all the responsibility of the production division. I had to follow up with the introduction of cold rubber into the program. Cold rubber development came out of the research and development program, so it was a part of the story.

MORRIS: You'd been working on the project for seven years when you became chief of the division. What was it like to be in charge of the whole research and development effort?

GREER: I had to sign a lot of things that I didn't have to sign before. I was helping the chief right along, I knew what he had been doing, so it wasn't too much of a jump to fill his shoes.

MORRIS: You had been on the development side; what was it like coming into contact more closely with the research side? Or had research become very much a question of development by then?

GREER: Research and development were pretty well blended together by then.

MORRIS: When you took up the position, did you think that your job would be short-lived? How long did you think your job was going to last, given that the plants would be disposed of at some stage?

GREER: I assumed that the plants would be disposed of sometime and when they were, I would be out of that job. I'd either have to get another job in government or go back to private industry.

MORRIS: When you took the job, how long did you think it was

going to last, did you have any idea?

GREER: There was a rubber law in effect that was to run for another couple of years or so, so I knew it was at least that long.

MORRIS: Did you have enough power in your job to influence the way in which research and development was carried out? Could you impose your will, if you wanted to, on the research program?

GREER: I could do quite a bit that way, quite a bit.

MORRIS: Did you ever have to force a solution on...

GREER: It was pretty much a matter of education. By the time we had something that was pretty obvious, one could make a good case for it. You wouldn't try to force something if you didn't have some good reasons in back of it.

MORRIS: Largely a question of persuasion?

GREER: Yes, it was sometimes a matter of persuasion, of getting people used to cold rubber. It took a certain amount of persuasion. Getting the facts all together was the main thing, to organize things so that facts were developed and if you had good enough facts for your argument, why you didn't have to be heavy-handed in convincing people.

MORRIS: Did the Korean War affect the research and development side of the program? Were new demands imposed on you that you otherwise might not have had?

GREER: Yes, there were several incidents of things of that sort. One case was when the army was finding that they were having difficulty in Korea with rubber in certain military equipment that was getting too brittle for winter service. They wanted a particular kind of rubber that we had found would be more rubber-like at low temperatures than natural rubber or ordinary GR-S. The government laboratories at Akron had made some of this and it was available, so the military sent us orders for more of that. It was a high butadiene rubber and we filled orders for that. That was one of the kinds of things that we were able to help with.

MORRIS: We've already touched upon cold rubber and in fact,

you've now given me this very interesting technical memorandum about cold rubber (5). I know from my own work that it was originally developed by I. G. Farben about 1943, and also that some work had been done on cold rubber in Minnesota before the end of the war. Carl Marvel and [John W.] Livingston came across it when they visited Germany in 1945. Of course, then the American research effort went into full tilt with cold rubber. Leaving aside some of the more technical questions associated with it, would you give just a brief overview of how cold rubber was developed by the American program?

GREER: We had knowledge of the German processes that Livingston mentioned and also there had been some work done here with more effective catalysts, so it was a matter of adjusting and selecting, and experimenting with, and finally choosing the best polymerization formula to get this reaction to run at a low temperature in a reasonable time. Also not to get undesirable crosslinking and other properties in the rubber. You always wanted to get the reaction to go at these low temperatures without detrimental effects on the rubber. Most formulations, when they were run under these very vigorous catalytic conditions at low temperature resulted in poor quality rubber. Finally, what we ended up with was a good quality rubber made at low temperature in a reasonable time.

MORRIS: You mentioned for example, undesirable crosslinking, but what were the specific properties of cold rubber that made it such a good rubber? Was it the more regular polymer chain? I think the amount of stereoregularity increases.

GREER: It's a matter of chain regularity and a matter of the distribution of the high and the low molecular weight materials in the rubber.

MORRIS: What about dimer formation? I remember reading somewhere that when you polymerize something like butadiene at relatively high temperatures, you get a great deal of dimer formation.

GREER: The main detrimental factor is that you get a gel material [insoluble in benzene], which doesn't process very well into a rubber compound so that the tire quality is inferior.

MORRIS: Didn't William Baker do a lot of work on this question of gel content? What about the technical problems? For example, installing refrigeration plants, making sure things operated properly at such low temperatures.

GREER: It didn't cause enormous technical problems, just a matter of providing the refrigeration. You can do it two ways. Instead of circulating water through the reactor jackets, you can circulate brine that's cooled by refrigeration. That's the system mostly used. One of the plants uses a system with sealed reactor jackets so that they could use ammonia directly and avoid the use of brine.

MORRIS: There wasn't an awful lot of upheaval to the copolymer plants when cold rubber was introduced?

GREER: You just lower the temperature of the reactor. The recovery of the monomers would be the same, and the coagulation would be the same, and the drying and the baling would be the same. It was mainly in the reactor, in getting it cooled down. It was just a mechanical problem.

MORRIS: By the end of 1949, half of the capacity of all operating production was converted to cold rubber. [Morris quotes from paper Greer has given him (4)]

Now, of course, the other major development while you were chief of the research and development division was the introduction of oil-extended rubber. This is a very controversial area, judging by the amount of paper that General Tire deposited in the University of Akron archives. [laughter] From your point of view, working from within the research and development program, how did oil-extended rubber come about?

GREER: Mainly because it was found possible back in October 1947. There were some tire tests made and #123 used some cold rubber of high Mooney [value]. If you read anything at all about the synthetic rubber program, you will come across "Mooney" sooner or later (6). A 50 Mooney rubber is standard rubber with the desired plastic viscosity. Higher than 50 it's getting kind of tough, lower than 50, it's beginning to get more fluid and flabby. Tire plants wanted a Mooney no lower than 50, to mill it with the compounding ingredients readily. The tire factory would like to have it higher, but they can't make a tire very easily if it gets up much above that. All this rubber was made at roughly 50 Mooney. Now this test #123 was with an experimental rubber. It was brought up to about 60 or 70 Mooney, and they found it was awfully hard to process in the Banbury mill, so they added larger quantities of mixing oil. They usually put in about 5% of heavy oil when mixing of rubber with the compounding ingredients.

MORRIS: That was a process that was used before?

GREER: It was normal to use about 5%.

MORRIS: I didn't realize that.

GREER: Sometimes less, but you'd use a little bit. In this case they used something like 20% oil. That was a lot of oil. But they got a tire that tested rather well. Contrary to normal processing a lot of oil was used but it was just passed off that way. Nobody thought much about it and everybody at that time wanted natural rubber anyway, so it was overlooked.

MORRIS: Who did they work for?

GREER: The tires were made by Lake Shore Tire and Rubber Co. in Des Moines, Iowa. They made them for our tire tests and they were paid for by our program. The polymer was made by the Phillips Company in its pilot plant under our research agreement. That was the way that was done. It was tire tested in San Antonio with our tire test program. It was a good rubber but with the unusual method of processing nobody much wanted to repeat it.

When the [Korean] war came along everybody was clamoring for any kind of rubber. General Tire in the meantime, had apparently noticed this result, fooled around with it and decided that it was a way to make a cheaper compound rubber for a tire. Their representatives came into the Washington office in October 1950 and said, "We know how to make 22% more rubber without using any more critical materials." We said, "How do you do it?" and they said, "We can't tell you about that, we want to get a patent on it. But, if you pay us enough, we'll tell you how to do it." We said, "We have patent agreements, you're covered under our patent agreements. "Oh, no, we're not covered under that." So we said, "We'd like to try it and see what it is; we can't pay any money until we see what it is." "Well, we can't tell you what it is, it's our secret. We're not going to tell you until you make an agreement to pay us." So we said, "Well, how do we know it isn't something we already know about?" "Well, if you knew about it, you'd be using it." That's the way it went.

Finally in January or February, Goodyear representatives came in and said, "We have something that will apparently do the same thing as they're talking about, but we don't know whether it's the same thing or not. We just use a high Mooney rubber and put more oil in. You can do it with the latex or you can do it in a mill." So we set up and made a plant run with the stuff. It proved pretty good, especially on tires, so we put it into production as oil-extended rubber. Of course, you could sell it at a lower price so people began trying it out and finding that it would work, so they started demanding it. That's the way that went on. We asked if General Tire had filed a patent and they told us that they had. A patent was applied for in 1950, in November, and the Patent Office at first turned them down because it wasn't novel, as somebody had used the same thing before. They took it to court and a judge in Washington made the Patent

Office grant them a patent in December, 1960. General went around canvassing all the rubber companies and said, "Look, if you want to use this, and you have been using it...

[END OF TAPE, SIDE 4]

MORRIS: What happened then?

GREER: Armed with the patent, they went around canvassing rubber companies demanding that they pay a royalty for use of the patent. Firestone and Goodyear objected and some of the other ones held off and said they'd wait and see what happened. Maybe they made an agreement with them, I don't know. Firestone and Goodyear objected and said they would contest it in court. Goodyear eventually made some agreement to temporize, would pay them if the patent was held up in court. Firestone fought it to the finish. The first decision on the validity of the patent was in 1972 in the Cleveland court, which held that General had a valid patent. They also said that apparently it wasn't within the scope of the research agreement. Firestone opposed it firstly, that it wasn't really a valid patent because of prior work and secondly, that it was covered by the research agreement that General Tire had [with the rubber program]. The Appeals Court in November 1973 in Cincinnati ruled that the patent was was valid, but that it was within the scope of the research agreement that General had with the government, so the government could license others as it saw fit. That meant that the rubber companies could avoid paying license fees to General. General took it to the Supreme Court and in October 1974 the Supreme Court refused to act on the Appeals Court decision and so allowed the Appeals Court decision to stand. That's the way it ended. General had a patent (7), but it was within the scope of the research agreement with the government, so the government could license others at its will.

MORRIS: And the government presumably did so license?

GREER: Certainly, Firestone was satisfied that they would get a license if they asked for it.

MORRIS: I would have thought that a very strong case could have been made for the invalidity of the patent, given prior work.

GREER: They must have held for the point that the government may have had a prior experiment but if they didn't realize it's importance, then they didn't really have a prior claim.

MORRIS: I think I remember reading somewhere that the reason Firestone, for example, didn't press the point was because, for

various reasons, nobody wanted the patent to be declared invalid.

GREER: The issue then was nobody wanted it anyway. We had it but Firestone really didn't want to use it. Nobody else wanted it, they were all clamoring for natural rubber then. Of course, when they couldn't get natural rubber, they'd take anything; if you told them this is better than regular GR-S and almost as good or better than natural rubber, then they'd take it. Cold rubber made it possible. The lack of natural rubber in effect put it in demand. That's the way I see it.

MORRIS: What personal role did you play in all these legal proceedings? Were you ever called as a witness or an expert witness?

GREER: I sat in on most of the conferences outside of the courts. I was called in to the first court action. There was a deposition in Baltimore, I served as a witness there. The judge in Baltimore, that was about 1963, thought it all over and he decided he couldn't make a decision and referred it to a Cleveland court. I served as a witness in the Cleveland court sometime before that court decision in 1972. I also was called as a witness when they had a court test of it over in London. General had a British patent on it. That was an interesting case. When they got all through, the judge died about two months later, before he could give a decision. So they turned all the tapes and evidence, whatever they could scrape together, and gave it to another judge who reached a decision in favor of General. In the British case the element of whether it was within the scope of our research agreement didn't apply; it was just a patent matter.

MORRIS: What was your impression of the British legal system compared with the American legal system?

GREER: I thought it was better in a way. It seemed a little more ponderous, but on the other hand it seemed more direct.

MORRIS: Any amusing stories as a result of your entanglements with the workings of the law?

GREER: Well, there was one thing. Over there, they don't call the judge "Your Honor," they call him "My Lord." In one case I said "Your Honor" and some people snickered and I corrected myself.

MORRIS: Let me ask one or two technical questions. What's the

technical relationship between cold rubber and oil-extended rubber? Can you only extend cold rubber?

GREER: When you're polymerizing butadiene-styrene the polymer isn't entirely uniform. You have some big butadiene-styrene copolymers but others haven't grown as much. Instead of being a hundred thousand molecular weight they're only ten thousand molecular weight. If you alter polymerization conditions so that the smaller molecules get bigger and bigger, so there are more of big particles, then adding oil will replace the needed small molecules that you don't have any more. What you're doing is to replace low molecular weight butadiene-styrene with oil of about the same molecular weight, but you don't have the cost of the butadiene or styrene.

MORRIS: In other words, there's no point in using oil in hot rubber?

GREER: You can't do it in hot rubber. If you run hot rubber in the reactor longer to increase the average molecular weight, you form undesirable gel. Cold rubber was relatively free from gel and this makes oil extension possible.

MORRIS: What is masterbatch? I've heard a lot about oil-extended masterbatch rubber.

GREER: Originally, masterbatch simply referred to the rubber, the sulfur, and the accelerators, the processing oil, the carbon black; the whole mixture. That is a masterbatch. In the synthetic rubber program, it came to mean... Somebody came up with the idea that we're making all this rubber in a latex form, then we coagulate it and dry it and we have a solid rubber. Then we take it over to the tire factory and put the carbon black in it. Why don't we just add the carbon black to the latex? So they would take the latex, with the incorporated carbon black and run that through the dryer and bale the rubber with the carbon black already mixed in. They'd call that masterbatch rubber because it had the carbon black in it. When the carbon black is mixed into the latex, would also be called masterbatch. If a factory makes masterbatch rubber, that means the carbon black has been added before the latex has come out of the drier. But it can mean the other thing. If you talk to a tire factory man, he'd still probably just think of it as putting all the compounding ingredients together and mixing it up.

MORRIS: Why is it particularly used in oil-extended rubber?

GREER: Oil is the only thing that replaces butadiene-styrene. A

rubber compound can be 'extended' with other things, such as fillers like calcium carbonate in shoe soles, but these tend to depreciate the quality. Oil is an extender in the true sense that the rubber properties are not deteriorated at all, when properly used in cold rubber.

MORRIS: What are the limitations of oil-extended rubber? General Tire was boasting that with oil-extended rubber you could expand the production of converted rubber, but in practice I think the original amount of converted rubber...

GREER: You get that much more volume out of the reactors.

MORRIS: Quite so, I think that only about 25% of synthetic rubber could originally be oil-extended. Why can't you extend all synthetic rubber?

GREER: When you talk about all synthetic rubber, you mean all butadiene-styrene rubber?

MORRIS: Yes.

GREER: You may have places where the oil would introduce a color, you might want a white rubber. The oil might make it harder to mask the color. [Greer gets some papers] Here's the total SBR. Oh, the figures are not broken down to separate out the oil-extended rubber.

MORRIS: The other major development of the period, were the stereoregular rubbers. What was the advancement of stereo-regular rubbers like from your side of the fence, how did you see it?

GREER: That was announced during the last year I was in the rubber program proper, just before I transferred to the National Science Foundation. It was while the synthetic rubber program research and development agreements were still holding. In December 1954, Goodrich announced that they had found a synthesis of what was essentially a duplicate of natural rubber. We wrote them and asked them about it; their reply was that they had done it under their own resources and outside the scope of the agreement. Later they told us it was a matter of polymerizing isoprene with the use of catalyst developed by Ziegler. They had applied for a patent on it. Of course, they had a license from Ziegler to use the catalyst for the preparation of synthetic natural rubber. Anyway, they told us that they had been experimenting with the catalyst to make an improved plastic, from

an isoprene copolymer. When they looked at the product with infra-red analysis, the spectrum included absorptions characteristic of natural rubber. That was the basis for their going ahead with the work. While I was at the National Science Foundation, there was some discussion with them about it. Indeed, National Science Foundation said they would refer the matter to the Department of Justice to see what the situation was but I never heard anymore about it.

MORRIS: Legality apart, do you feel it ought to have been a part of the general agreement?

GREER: Oh yes, I feel that ought to have been part of the agreement. There were several kinds of agreements. One was based on research work that was done with government finance and there was no question about that. Then there was research work that was done by the participant that which may or may not have been financed by the government. It was still within the scope as defined in the contract, and that included polymers of dienes, and polyisoprene is certainly a polydiene.

MORRIS: What about the work done by Firestone?

GREER: That was announced in the early spring of 1955. The plants were all sold in late spring of 1955. Firestone said their process, which did roughly the same thing, made an isotactic polyisoprene and was accomplished with a metallic lithium catalyst. Firestone said, "We don't know whether this is within the scope of the agreement, but anyway, we're going to publish it and not argue about it." That's the way they went along with it. I don't know whether they did anything else with it or not.

MORRIS: When the program was drawing to a close, in the period we are talking about now, the technology section in the official history high-lighted several practical problems that were still outstanding at that time (2), including the difficulty of making a large synthetic rubber tire, or an aircraft tire that could withstand both freezing temperatures up at 20,000 feet and a very hot temperature when it hit the runway. To what extent have these problems been solved since then?

GREER: The difficulty of making large tires in the absence of natural rubber has been solved in the sense that we can make a duplicate of natural rubber synthetically now. I'm sure the work has been going along making more cold-resistant tires in the meantime. I'm pretty sure progress has been made.

MORRIS: On one hand you have the chemists, working on the large-scale things like polymerization kinetics, and elasticity theory and so forth. But they didn't seem to be well-coordinated with the pressing concerns of the technology, in other words, there's a bit of dichotomy here between the two groups. Is this true, or have I drawn a false picture here?

GREER: There was a certain gap there and also an occasional linkage. The situation might be something like Einstein's $E=mc^2$ and the atomic bomb. There was some gap before anybody was able to do anything with mc^2.

MORRIS: How did the research and development of the program connect to the disposal of the plant?

GREER: It was decided that the government would drop the tire testing because they had assumed the rubber companies would pick it up. Of course, government wouldn't do any more financing of the rubber companies' research since they were going to buy the plants. As for the basic research in universities and the other independent centers, including the government laboratories operated by Akron University, the government would fund it for a year while it was in the jurisdiction of the National Science Foundation. So that's what they did.

MORRIS: Did you have to do any specific research work in conjunction with the hand-over of the plants? There was no specific sort of work that had to be done by your side?

GREER: No.

MORRIS: Did you ever fear that the disposal program might collapse if no one offered to pay enough money for the plants?

GREER: There was always a possibility that no one would want to pay enough money for them. The fact that rubber was in such demand meant that somebody would probably [buy them]. It would be crazy to build a new plant when one could be bought. If the government didn't get a fair price, they could just close them off, and they did that. Two plants didn't command a high enough price the first go-around so they froze them for a period and then opened them up to bids again; then we got a lot of money for them.

MORRIS: A lot of arm twisting went on with the various companies to get them to pay more, I know.

GREER: They tinkered with each company separately and then they'd tell them that, "Your bid is too low, because we know what all the others are bidding on the other plants." They came back to them and got them pretty well up in price before they'd close it off for a temporary freeze period.

[END OF TAPE, SIDE 5]

MORRIS: When the overall synthetic rubber program ended in 1955, you were transferred to the NSF to oversee the run-down of the research program. Why was the NSF chosen and when was it decided that in fact it would be turned over to them?

GREER: The NSF was a going concern in sponsoring basic research. Since, except for the government laboratories, it was going to be largely a matter of supporting university research, it just seemed logical to turn it over to them, since they were supposed to be the stronghold of research.

MORRIS: When was that decision made?

GREER: The Federal Commission decided that. [As set up by the 1953 Rubber Producing Facilities Disposal Act]

MORRIS: Would it have impossible to have terminated the research program in July 1955 and forgotten about the run down?

GREER: Just chop it off and sell the plants? Well, I suppose, but with some of those projects it was better to taper them off. Give industry a chance to decide whether it wanted to pick up any of these university research groups and keep on supporting them rather than just throwing them off to potluck with the government.

MORRIS: Did anything particularly noteworthy happen in these last eighteen months, from your point of view?

GREER: No.

MORRIS: Let me ask another question which might express it slightly better. Were there any developments that were coming down the pipeline in 1954 or 1955 that you would have liked to have seen completed under your direction?

GREER: There wasn't anything then that looked like it need any

attention other than what it would get from the rubber companies.

MORRIS: Would you have liked the synthetic rubber research program to have continued, at least in a modest way, after 1956?

GREER: Well, it could have been done. As a matter of fact, I proposed one way of doing it. Firestone and Goodrich having come out with those syntheses of cis-polyisoprene, that opened a whole new field for polymer research, which was sufficiently attractive that the industry would certainly push forward, as they did. I don't think they needed any more.

MORRIS: I was thinking more of university research. Has there ever been any sort of body overseeing that?

GREER: The National Science Foundation can pick up and support university research and they should have funding enough to pick up the most noteworthy proposals.

MORRIS: How did it finally end? Was there an informal ceremony in either 1955, or 1957 when the whole thing finally ended?

GREER: [laughter] It went very smoothly. I don't think there was any celebration.

MORRIS: They didn't all meet in your office...

GREER: No. No hand-wringing, tear-gripping ceremony. [laughter] Just packed up and left.

MORRIS: In 1957 you just simply cleared you desk...

GREER: When I left in 1957, it had all dwindled down, I was the only one left.

MORRIS: Were you? Let's have a look at one or two general aspects of the synthetic rubber program. What would you nominate as the three or maybe four most important research centers in World War II, not distinguishing between the academic and industrial labs? Who were the biggies, the most important ones?

GREER: I think you would have to class all the universities as one. The government laboratories at Akron as another. The tire

test lab is another; of course, it's not a research center but without it we wouldn't have had any feedback into the program. All the companies had their research. General Tire was less active but they had sort of a bob-tailed research agreement that didn't encompass to the full extent as the other companies did.

MORRIS: You wouldn't say that the lab at such and such and the lab at so and so were really key research centers in World War II?

GREER: The U.S. Rubber laboratory at Naugatuck did a lot, as did Goodyear, Goodrich, and Firestone. The government laboratories did an awful lot. They did more pilot plant work than any other one laboratory. Some of the plant laboratories had pilot plants that did a certain amount of work, although that wasn't as research oriented as the head office research labs. It's awfully hard to pick out one or another. The whole thing was a joint program.

MORRIS: What about the personalities involved? You've talked about some of the people who worked on the project. Are there any other key personalities that you particularly remember and were particularly important?

GREER: Amongst the university people, Dr. Marvel comes to mind right away and, of course, Peter Debye was a Nobel Laureate. Harkins and Morris Kharasch at the University of Chicago. Piet Kolthoff at the University of Minnesota, George S. Whitby of course, at the University of Akron. Avery Morton at MIT had a synthesis that was not that far away from a Ziegler synthesis but one that didn't quite make the grade. It was a non-aqueous system, not an emulsion polymerization.

MORRIS: What about the post-war period? Who would you say were the key people in the post-war period, apart from the people that we've already mentioned?

GREER: Phillips Petroleum, I should have mentioned them before; they were very active. They supplied rubber for the first fully fledged cold rubber tire test. Lake Shore Tire Plant was active amongst the tire plants, testing new polymers for us. The other tire companies did too -- Goodyear, Firestone, U.S. Rubber. Copolymer Corporation did a nice job among the plants that had their own pilot plant laboratory, down at Baton Rouge. Goodyear at Akron had a pilot plant. Naugatuck had a pilot plant.

MORRIS: As a matter of interest, we have decided to concentrate our own historical research efforts on the University of

Illinois, Bell Laboratories, and the Esso Chemical Labs at Linden during World War II. I would like to give you some names and see if you can shed any light on them. Of course, the first person I have down is Carl Marvel.

GREER: Bert [Herbert A.] Laitinen was one too, at Illinois. He went down to Florida later.

MORRIS: Anything about the Illinois work?

GREER: A large part of Marvel's work was centered on testing out the various monomers, the various kinds of soap, and polymerization recipes generally. That pretty well characterizes it.

MORRIS: Would you say that perhaps Urbana was one of the most important research centers during World War II?

GREER: It was one of the most important research centers. You might make a distinction between basic research and applied research. All universities might be considered basic research.

MORRIS: What about William Baker? Did you come across him much?

GREER: He contributed a lot to this business about gel and the structure of the polymer. I don't remember any particular laboratory he was affiliated with in the program.

MORRIS: What about Bell Labs?

GREER: That was pretty early in the program, I don't think it carried on into...

MORRIS: No, they didn't carry on after the war. What about Paul Flory?

GREER: He was one of the strongest chemists on the the physical chemical side of the program. He was an investigator in the program for a long period. I think he was at Goodyear at one time, but then he was at Cornell and then at the Mellon Institute.

MORRIS: He ended up at Stanford eventually.

[pause while Greer is looking up information]

GREER: Kolthoff was very active at Minnesota. [Greer pauses to look up information]. Chicago: Kharasch is the one that comes to mind although Harkins is good there too. At Case there was Sam Maron; at Akron, G. S. Whitby; Debye at Cornell. In Delaware we had a program by Allan Colburn for a while on distillation. At MIT we had Avery Morton. The National Bureau of Standards was active in a lot of things.

MORRIS: Can we move on to a slightly different aspect, the research side of the synthetic rubber project. As the graph you gave me illustrates well (4), it was always small in terms of overall funding. This is in contrast to say the Manhattan Project or even in a commercial company like Du Pont, where I'm sure the percentage figure is higher. Why was the funding always kept at a comparatively low level?

GREER: It was around two percent. That's about the same as it is for most chemical companies, but Du Pont may spend a little more. The electronics companies spend a lot more.

MORRIS: You mean you don't think it could have been any bigger?

GREER: It could have been bigger, sure, everybody in charge of research usually tried to get more money, but the program thought they couldn't justify any more.

MORRIS: One reason why the Center for History of Chemistry is producing a history of the synthetic rubber wartime project is because so many scientists who worked on it, [William O.] Baker for example, insist that it was the key period of their life in terms of their work. What would you say?

GREER: It was a key period in my life, there's no doubt about that, it sure was.

MORRIS: These polymer scientists also argued that the wartime research laid the foundation of modern polymer science, not only intellectually, but also in terms of facilities and qualified personnel. What would you say to that?

GREER: I think that's absolutely true, yes. Cold rubber, oil-extension, and cis-polyisoprene, especially the last two, opened up a whole new field. Industry has been running out of breath for that ever since.

MORRIS: Are there any other long-term results of the program that you'd like to mention that you feel were important?

GREER: It showed that you could start almost from scratch and build a whole new industry that was operating within an eighteen month to two year period. When you consider what was done in such a short time, just to build all those plants is just amazing. When you consider you are handling things like butadiene, a gas under pressure, and styrene, which is an organic chemical that has some problems, and all these things have to be put together to make a product that you want. A productive capacity of roughly a million tons a year, it's an amazing undertaking. Somebody compares to the problem we had with fuels, but that's a different thing. It can't really be compared. We thought that was a big program, but for synthetic fuels to replace our consumption of gasoline would take a thousand times as big a program.

MORRIS: One of Maurice Morton's stories is when he says, "In World War II we produced synthetic rubber. By the end of the war we could produce a million tons a year." Then he says, "If you want to do the same with synthetic fuel, you would have to produce three million tons." Then he hesitates, smiles and says, "A day." [laughter]

GREER: Yes, yes, you can't make a parallel on the basis of quality or size.

MORRIS: How would you rank the synthetic rubber project along with other famous scientific projects like the Manhattan Project, the space program, things like that, what would you say?

GREER: I think it's at least equal to any of the others in terms of quality and quantity of accomplishment.

MORRIS: Let me act for a moment as a devil's advocate. I would like to raise a point made by Professor Robert Solo, of whom you're familiar, who argues that the rubber companies deliberately refrained from creating wholly new developments in polymer science while the exchange agreement was in force (8). Then suddenly, when all the restraints were off, they started producing new developments all the time. What would say to that?

GREER: I don't think that's true. The only basically new things since then are the Ziegler-type catalysts. They wouldn't have come along as soon if the companies hadn't had all this

background work in synthetic rubber and their organization, all of which originated within the scope of the program.

MORRIS: Solo goes on to argue from that debatable premise, that the government post-war research and development program was doomed to be an expensive failure.

GREER: [laughter] They sold all the plants, some people say for more than they cost. This book says they got all and a little bit more back from the sales. [Greer motions to book (3)]. Here it suggests that we ended with 10 million dollars to the good.

MORRIS: Because of all the research and development that was done people were willing to pay more?.

GREER: Oh, yes. If it hadn't been for cold rubber and oil-extension, we couldn't have been able to sell the plants for much more than the scrap value.

MORRIS: It increased their value.

GREER: Yes.

MORRIS: That certainly is a good point. From your experience of the research and development program, what general lessons, if any, would you draw about government-industry research programs? Do you think they're a good idea?

GREER: If you had to do another job like this, I think you ought to do it just about the same way.

MORRIS: You've mentioned the synfuel program, is there anything more you want to say about the apparently ill-fated synfuel program, or do you think we've covered that enough?

GREER: That's a mind-boggling thing. The quantity required and the trouble trying to get it out of shale, and the water requirements... We should do everything we can to economize. We need liquid fuels for cars, we assume we have to keep plenty of cars. We probably ought to replace burning liquid fuels in stationary equipment, we ought to change over to gas or coal and wherever we can replace liquid fuels with some other liquid like methanol.

MORRIS: What about economization?

GREER: We could economize by more heat exchangers and things of that sort.

[END OF TAPE, SIDE 6]

MORRIS: Was your position at the U.S. Army Research Office, which you took up in January 1957, arranged for you or did you simply apply for a job there as your employment at the National Science Foundation was obviously temporary?

GREER: I was scouting around for what other opportunities would surface. I heard that they were looking for someone down here so I applied for it, got a transfer and that's the way that came about.

MORRIS: Was your work here similar to your work in Washington, broadly speaking?

GREER: It was similar in the sense that it was reviewing research proposals and reports from universities and other institutions; deciding and recommending which ones would be the most fruitful to support, keeping in contact with them and offering suggestions. It differed in the sense that I had no contact with production facilities or applied research except insofar as findings could be transmitted to army laboratories or something of that sort. It was pretty much like the relationship I had with the university research projects but on a broader scale -- more universities and beyond polymers.

MORRIS: Was it perhaps more "hands-off"?

GREER: Yes, it was more hands-off. In other words, in this type of operation we tended to let the university project directors have their head pretty much. Also, they could select things that might not have any tangible application.

MORRIS: Was there a good relationship between the Army and the universities when you joined in the late 1950s?

GREER: Yes, all the university contacts expressed satisfaction with their relationship to the Army. They said they appreciated the type of relationship that existed.

MORRIS: The kind of work you were supervising was largely basic

research. How defense related was that?

GREER: The Defense Department decided before establishing the office here... Well, the Army followed the Navy's example of supporting some basic research projects that might not be directly connected to military application. If they supported enough well-chosen projects, they ought to produce gems of wisdom that would be applicable in some circumstance perhaps unforeseen at the time. The Navy got set up and the Air Force did something similar and then the Army decided they ought to do the same thing. They said, "We're going to have this office that supports basic research. If it's anything that's directly obvious to the military laboratories then they should support it themselves. We will support research of good scientific merit, but you have to be able to make a good case that the results will give rise to something beneficial. But must not to be to make a better grease for a wheel, or something trivial like that."

MORRIS: Can you give a particular example of the kind of research they funded?

GREER: There is one thing that comes to mind in the rubber field. Professor [Arthur V.] Tobolsky at Princeton was working on the preparation of polymers and how to control their polymerization, so that polymerization stops at a certain point. He found a method of giving very precise control of polymerization to get any particular kind of quality in the product that you wanted. The Army was using synthetic rubber-type polymers in a binder for a solid propellant, but they weren't able to get the right kind of a composition and they read one of his reports and found just what they wanted. It was the ideal thing.

MORRIS: Did the good relationship between the Army and the universities deteriorate as a result of the anti-Vietnam War protests in the middle to late 1960s?

GREER: Not from our standpoint in this office here. We still received proposals and evaluated them. Of course there were discussions about military problems in the Vietnam War. We would send our proposals to the Army laboratories for comments and if they saw something that would be beneficial to them they would support the proposal. We didn't get any case where they said, "This wouldn't help us in Vietnam and we don't support it.

MORRIS: William Baker has criticized today's young scientists for their reluctance to do defense-related research. Do you think that young scientists are less willing to carry out defense-related research, compared with their counterparts in the

1930s or 1950s?

GREER: We haven't seen it here. When they send a proposal into the Army, that shows that they want to work with us. If it's basic research it isn't really geared into to working on a new explosive or a new tank, and we wouldn't see it that way. Our office got into a hassle during the sixties when everybody was waving banners against the war. They just didn't want the Army office on the campus. The Army finally moved the office off the campus. The funny thing is that they didn't move the office off campus until the year I retired and the fuss was all over by then. Nobody was jumping up and down or shouting about it. Apparently somebody generated the idea that they'd better get off the campus. It hibernated and incubated and finally it came to a head and they moved off the campus. The chemistry division professors and most of the others who knew us didn't want us to go. Some of the higher-ups, some of the political people, did.

MORRIS: What kind of links did you have with the faculty at Duke?

GREER: Very good. Some of the chemistry professors served as consultants to the chemistry division and the physicists had contacts with the physics division, engineering with engineering, very, very close relationships. We used to eat in the faculty dining room.

MORRIS: Given the present Congressional concern about getting the maximum "bang for their buck," do you think that the U.S. Army Research Office spent their funds wisely in that period?

GREER: Well, it is Army, part of defense and Reagan is sticking up for defense. I don't think the Army have had a squeeze on their funds.

MORRIS: No. But, do you think the money was spent efficiently.

GREER: In this office?

MORRIS: Yes.

GREER: Under their program it was about as good as it could have been.

MORRIS: Why did you carry on working until you were seventy, did you like your job so much?

GREER: I felt in good health and able to carry on and didn't feel any pains. In fact I don't think I would have quit then if it hadn't been that seventy was mandatory retirement age at that time.

MORRIS: Were you happy enough to carry on working?

GREER: I can be happy not working but I can be happy working too. I've enjoyed my work or I wouldn't have stayed as long as I did.

MORRIS: What have you done since your retirement? Have you done any consulting?

GREER: I did a couple of consulting jobs for the office. Otherwise I keep busy within the local sections of the American Chemical Society and the American Institute of Chemical Engineers. The local chemical section has a polymer subgroup that I've been active in. I've been chairman and treasurer of that, so I go to those meetings and I go to the meetings at Duke, seminars and things. They have good libraries here: the city library, I can also go to the UNC library or the Duke library and the N.C. State library in Raleigh anytime if I choose to.

MORRIS: Have you kept any close friendships up since your synthetic rubber days?

GREER: Friends with rubber people?

MORRIS: Yes.

GREER: I talked to Jim D'Ianni about a week ago on the phone and I told him about you coming over here to see me.

MORRIS: Did he make any comment on that?

GREER: He said he had known about the project but he hadn't heard from you as yet. Of course, in this area Bill [William D.] Krigbaum of Duke is in polymers; he used to be in Debye's section at Cornell, working for his Ph.D. I know him real well. There's another fellow at Duke in biomedical that's working in polymers

that I know real well -- Howard Clark. One guy who I know works for IBM, Don Preiss, used to work for Shell Chemical, he knew about the Shell plant out on the West Coast. Over at N.C. State University there's a fellow who was at Naugatuck early in the rubber program, Richard Gilbert. Joe [Joseph H.] Faull is a fellow I've kept in communication with. He was a consultant for the Office of Naval Research and also for the Office of the Rubber Director and also for General Tire for a while.

MORRIS: One final question we can put on the tape for the record. Who do you feel I should interview now? I'm already down to interview Willard Asbury and Donald Green from Baton Rouge, and I may also be talking to Howard Brown, who used to work with Semon at Goodrich. Can you think of anyone else who might be worth interviewing?

GREER: How about somebody at the Bureau of Standards?

MORRIS: Can you name anyone in particular?

GREER: Bob [Robert D.] Stiehler would be very good. He's still working there.

MORRIS: Anyone else?

GREER: You ought to get somebody from the production division. How the plants were built, how many plants went in; things like this that.

MORRIS: Anybody...?

GREER: Walter Munster. He has a farm out in the Shenandoah Valley. I don't know whether you can get him into Washington. Dick Harmon, who edited this thing [Greer points to book (3)]. In the Bureau of Standards, I guess Lawrence Wood is still in the Bureau. He'd be very good. Of course, Charlie Perry who wrote a review article in the Rubber Group book (3). Jim Sears would know about how they restricted the use of natural rubber through the war period. About the disposal program, Joe Faull went around and briefed members of the Disposal Committee and got them acquainted with the problems and with the values of the plants. He's up in Boston.

MORRIS: Well, thank you very much. It's been very worthwhile.

NOTES

1. W. H. Walker, W. K. Lewis and W. H. McAdams, Principles of Chemical Engineering (New York: McGraw-Hill, 1923).

2. B. J. O'Callaghan, Rubber in World War II (Washington, D.C.: Reconstruction Finance Corporation, 1948). Copy in BCHOC Oral History file #0021.

3. --, World War II Synthetic Rubber Program: Mission, Record, Mechanisms, Significance and Its Messages For Today (Washington, D.C.; Washington Rubber Group and Rubber Division, American Chemical Society, 1979). Copy in BCHOC Oral History file #0021.

4. Bar Chart: R&D Cost compared to Product Value of GR-S, cold rubber and oil masterbatched GR-S, 1943-1955. Copy in BCHOC Oral History file #0021.

5. Summary of memorandum RDDR-150 on cold rubber, P. S. Greer to E. D. Kelly, 17 February 1954. See BCHOC Oral History file #0021.

6. See M. Mooney, "A Shearing-Disk Plastometer for Unvulcanized Rubber," Industrial and Engineering Chemistry, Analytical Edition, 6 (1934): 147-151. The Mooney value is the force, measured on an arbitary scale, required to rotate a serrated plate in a rubber specimen held in a shearing disc viscometer and kept at a specified temperature.

7. Emert S. Pfau, Gilbert H. Swart and Kermit V. Weinstock, "Polymerization of Conjugated Diolefins in the Presence of a Heavy Hydrocarbon Oil Plasticizer and an Alfin Catalyst for Making Tire-Tread Stock," U.S. Patent 2,964,083, issued 13 December 1960 (application filed 20 November 1950).

8. R. Solo, Across the High Technology Threshold: The Case of Synthetic Rubber (Norwood, Pennsylvania: Norwood Editions, 1980).

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