CHEMICAL HERITAGE FOUNDATION

MARY L. GOOD

Transcript of an Interview
Conducted by

James G. Traynham

at

Little Rock, Arkansas

on

2 June 1998

(With Subsequent Corrections and Additions)

THE CHEMICAL HERITAGE FOUNDATION
Oral History Program
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(Date) June 2, 1998

Mary Good

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

MARY L. GOOD

1931 Born in Grapevine, Texas on 20 June

Education

1950 B.S., chemistry, University of Central Arkansas
1953 M.S., chemistry, University of Arkansas, Fayetteville
1955 Ph.D., chemistry, University of Arkansas, Fayetteville

Professional Experience

Louisiana State University
1954-1958 Instructor and Assistant Professor of Chemistry, Baton Rouge
1958-1963 Associate Professor of Chemistry, New Orleans
1963-1974 Professor of Chemistry, New Orleans
1974-1978 Boyd Professor of Chemistry, New Orleans
1978-1980 Boyd Professor of Materials Science, Division of Engineering Research, Baton Rouge

UOP, Inc.
1980-1985 Vice President-Director of Research, Corporate Research Center

AlliedSignal Research and Technology Laboratory
1985-1986 President-Director of Research, Signal Research Center, Inc.
1986-1988 President-Engineered Materials Research
1988-1993 Senior Vice President-Technology

U.S. Department of Commerce
1993-1997 Under Secretary, Technology Administration

Venture Capital Investors, LLC
1997- Managing Member

University of Arkansas, Little Rock
1997- Donaghey University Professor

Honors

1969	Agnes Faye Morgan Research Award
1973	Distinguished Alumnae Citation, University of Arkansas
1973	Garvan Medal, American Chemical Society
1974	American Institute of Chemists Honor Scroll, Louisiana Chapter
1975	Herty Medal, American Chemical Society, Georgia Section
1979	Florida Award, American Chemical Society, Florida Section
1982	Scientist of the Year, Industrial Research and Development Magazine
1983	Gold Medal, American Institute of Chemists
1986	Elected Fellow, American Association for the Advancement of Science
1987	Elected Member, National Academy of Engineering
1988	Delmer S. Fahrney Medal, Franklin Institute
1990	New Jersey Women of Achievement Award, Douglass College at Rutgers University
1991	Charles Lathrop Parsons Award, American Chemical Society
1991	Industrial Research Institute Medalist Award
1991	ASM International Distinguished Life Membership Award, The Materials Information Society
1991	American Association of State Colleges and Universities Distinguished Alumnus Award
1992	American Association for the Advancement of Science Award
1992	Distinguished Public Service Award, National Science Foundation
1992	Albert Fox Demers Medal Award, Rensselaer Polytechnic Institute
1993	Ralph Coats Roe Medal, American Society of Mechanical Engineers
1994	Fellow, Royal Swedish Academy of Engineering Sciences
1995	Honorary Fellowship, The Royal Society of Chemistry
1996	Earle B. Barnes Award for Leadership in Chemical Research Management, Dow Chemical Company
1996	UCLA Glenn T. Seaborg Medal
1996	Federation of Materials Societies National Materials Advancement Award
1997	Priestley Medal, American Chemical Society
1998	Othmer Gold Medal Award, the founding members and affiliates of the Chemical Heritage Foundation
1999	Philip Abelson Award, American Association for the Advancement of Science

ABSTRACT

Mary Good begins the interview with a discussion of her family history, her childhood, and her early education. Her family moved from Texas to Arkansas in 1942, when her father was offered a principalship in Kirby. Good had very little science education in high school, and attended Arkansas State Teacher's College to become a home economics teacher. However, her interest in science was piqued by a freshman chemistry course, and Good soon became a chemistry and physics double-major. Her chemistry professor encouraged her to go on to graduate school, and she accepted a fellowship at the University of Arkansas to study radiochemistry with Raymond Edwards. She received her Ph.D. in 1955, and accepted a position at Louisiana State University in Baton Rouge. Her early work included iodine and sulfur chemistry and managing the radiochemistry laboratory. She moved to the brand-new New Orleans campus in 1958, where her research moved away from radiochemistry and into spectroscopy. In 1974, Good became a Boyd Professor at LSU. She soon returned to the Baton Rouge campus to help establish a materials science program. In 1980, Good left academia to join Universal Oil Products as vice president and director of research. Soon thereafter, corporate mergers led to the creation of AlliedSignal. Good discusses her extensive involvement in the American Chemical Society, including her time as chairman of the board and later as president. In 1980, Good was appointed to the National Science Board by President Carter, and was reappointed by President Reagan. In 1991, President Bush appointed her to the President's Council of Advisors for Science and Technology. Good discusses the support she received from AlliedSignal during this time. In 1993, Good left AlliedSignal to become the Under Secretary for Technology in the Department of Commerce. When she left the Department of Commerce four years later, Good joined Venture Capital Investors, which seeks to stimulate the creation of technology-intensive companies. Good concludes the interview with a discussion of her awards and honors, her family, and professional women in general.

INTERVIEWER

James G. Traynham is a Professor of Chemistry at Louisiana State University, Baton Rouge. He holds a Ph.D. in organic chemistry from Northwestern University. He joined Louisiana State University in 1963 and served as chemistry department chairperson from 1968 to 1973. He was chairman of the American Chemical Society's Division of the History of Chemistry in 1988 and is currently councilor of the Baton Rouge section of the American Chemical Society. He was a member of the American Chemical Society's Joint-Board Council on Chemistry and Public Affairs, as well as a member of the Society's Committees on Science, Chemical Education, and Organic Chemistry Nomenclature. He has written over ninety publications, including a book on organic nomenclature and a book on the history of organic chemistry.

TABLE OF CONTENTS

INTERVIEWEE: Mary L. Good

INTERVIEWER: James G. Traynham

LOCATION: Venture Capital Investors, LLC
Little Rock, Arkansas

DATE: 2 June 1998

TRAYNHAM: Mary, I know from things that I've read that you were born in Grapevine, Texas on June 20, 1931. Can we start off with you telling me something about your childhood and how you found your way from Texas to Arkansas?

GOOD: Sure, it's kind of a long story in a way. But as you know, by 1931 the Depression had begun, and my father was a school teacher by profession, but he hadn't actually finished his degree. My grandfather was working in a grocery store in Grapevine, Texas. His family had lived there for a very long time. In fact, both my mother's family and my father's family immigrated to Texas after the Civil War from south Alabama. My grandmother's family came to Texas in the land rush and they actually homesteaded land in east Texas, close to College Station. And my grandfather's family—my father's family—moved to Grapevine and had lived there for a very long time. They owned some property, and so during the Depression we were fortunate in many ways. We didn't have any money, but we had farmland that could grow things. Grapevine was a good "truck farm" part of the country so we always had food. My father, in particular, was a very good gardener in the sense that he could grow most anything.

So, during that period of time he continued to go to school. He went to North Texas State Teachers College, which in those days was in Denton. We lived on a small farm outside of Grapevine until I was about six, and then we moved to southwest Texas. I had an aunt, my father's sister, who lived in Sabinal, Texas, which is in Uvalde County and their only claim to fame is that Uvalde is the home of "Cactus Jack" [John Nance] Garner, vice president in [President Franklin Delano] Roosevelt's first term. My father taught school there for about five years in a little rural school called Trio Public School and I started to school there. I must have been in about the fourth grade when we moved back to Central Texas, just about the time the war started. My father had a job at the North American Aviation Plant in Irving, Texas, where the Dallas Cowboys play football today. He worked there for, gosh I don't know, it was probably a couple of years. Then we moved to Arkansas that fall because he did not want to go back to southwest Texas because he didn't like the desert. He was offered a principalship at Kirby, Arkansas, and he liked it a lot. It was nice and green, and it had wonderful places to run foxhounds. My father raised and bred registered Walker foxhounds.

So we moved to Kirby, about 1942 or so, and I was just starting in junior high school. I

must have been in the seventh grade or something. We were there until I was in the ninth grade and then we moved to Willisville, Arkansas, where my father was superintendent of schools. Willisville is down in south Arkansas—not very far from El Dorado. I graduated from high school there. So I was in several schools. I always liked school, frankly, but the biggest thing about my childhood was that we always lived in the country. We grew up playing baseball, and I mean baseball, not softball, [laughter] and basketball, because in those days, in the high schools in Arkansas, the only sport for which they had adequate equipment was basketball. It was a very lively sport, both girls and boys played, which was not true everywhere in the country. My father, as I said, raised registered Walker foxhounds, and I always enjoyed the dogs, and I used to go with him to hunt and also to show his dogs. But we really did have a very, very good childhood, I must say. My mother was a very unique person. She was one of those people who felt that you could do whatever you wanted to do and she encouraged all of us to do that, even when we were small. We really did have an unusual childhood in a sense, but were able to do things, particularly with our parents. I don't think that happens today.

TRAYNHAM: Where were you when you went through high school?

GOOD: In Willisville.

TRAYNHAM: As I recall, from something that I've read, you didn't have much science introduction in Willisville?

GOOD: No, my whole graduating class was seventeen students and that was a big class for them. First of all, I graduated in three years, because there were four of us children and we were fairly close together. So we needed to spread out our graduations a bit if we had any chance of getting through college. So I graduated from high school in three years. I took some correspondence courses and took some extra courses and things like that. But the only science classes that Willisville High School offered in those days were general science and biology. I never had any chemistry or physics, and no laboratory science at all because we had no laboratory in that school in those days. But they did have a small library and I read every book in it, I think.

TRAYNHAM: It sounds much like the high school I attended. After graduating from Willisville High School, then you went to college?

GOOD: I did.

TRAYNHAM: How did you choose where you went?

GOOD: Well, for two reasons. I was fairly young at the time, so my mother and father did not want me to go too far from home. So they moved. My father took a job as principal at Enola High School, which is not very far from Conway. In those days, the university—what is now the University of Central Arkansas—was Arkansas State Teacher's College. I started there in 1947 as a freshman. I guess we chose it primarily because it was what my family knew; it was school teaching. Secondly, it was not too far away, and thirdly, the fees were not too high. [laughter]

TRAYNHAM: All important considerations.

GOOD: Very important considerations, right.

TRAYNHAM: Well, since you did go to teacher's college, was your ambition at that time to become a teacher yourself?

GOOD: You know, it really wasn't exactly, but my father always thought that you should at least take something that would allow you to make a living if you had to. If you didn't have to, that was wonderful. So the first semester I signed up for a major in home economics and the reason for that was very straightforward. In those days, vocational teachers (as they were referred to)—who were home economics and shop teachers—got paid significantly more than the rest of the teachers because they got a supplement from the federal government. So I signed up for home economics because if you wanted to teach, that was by far one of the best things to do. But I only stayed in that curriculum for one semester. I ended up taking freshman chemistry, which you had to take. We had an elderly freshman chemistry teacher by the name of Mr. Cordrey. He did not have a Ph.D. He had done all of the research for a Ph.D. in his graduate work at the University of Chicago. Apparently, he had gotten in some controversy with the faculty at Chicago over whether or not his thesis had been superseded by somebody else's work. It was one of those classic discussions. So he never actually got his Ph.D., but he was probably the best freshman chemistry teacher I've ever seen. People just became enamored of his class—those who could handle the intellectual material. I liked it so much, particularly the laboratory, that I switched majors in mid-term.

TRAYNHAM: Tell me something about your college career after that freshman year.

GOOD: Well, I double-majored in chemistry and physics. In those days, the only criticism I would have of the undergraduate curriculum I had was that the mathematics department was rather weak. That was a handicap, frankly, for a long time. Even going into graduate school, because if you didn't have a good mathematics background, that was a problem. But other than that, the courses were really pretty adequate and I had a grand time. I just had a great time as an undergraduate. I started in 1947. That was the year that the huge volume of veterans came back to the university on the GI Bill. So the enrollment at Teacher's College, in those days, was very high compared to their actual capacity. But it was a really wonderful time because those people came to work and to study, and to move on, so the atmosphere was really very, very good. I participated in a lot of extracurricular activities. One in particular—I could not have afforded to join a sorority—was involvement in what was known as the Organized Independents. We decided that the kids on campus who were not part of the Greek system ought to have some sort of an organization, and somebody had started the Organized Independents a few years before that. That turned out to be a really great organization. We did a lot of things on campus—leadership kinds of things—and it gave you an opportunity to do some things that you wouldn't have had if you had not belonged to an organization like that. I played softball, of course. In those days, women's sports at colleges were pretty far down the pecking order, but we had fun. [laughter] Actually, I was a very good softball player. My father played in the old semi-professional Texas league, so I had learned to play baseball when I was very, very small and I had a great time with the athletic program there. I guess the other thing I remember most about my undergraduate career there was the teachers that I met. It is kind of interesting the things that you remember, but there was one mathematics professor, Dorothy Long, and there was also Lucy Savage, who taught physical education and who taught me swimming and tennis. Physical education was required and, in fact, during my final semester I had to take that swimming class because I was one course short in physical education.

We just had a wonderful time, although today people would say that we were sort of deprived. The laboratories were not as well equipped as we would expect today. Today's faculty members would think that they had been bumped off to New Guinea or something. [laughter] But the end result was that the classes were very well taught. You had to design some of your own gear, which frankly was a plus, not a deterrent. You just learned an awful lot about hands-on work. The laboratories were fun. I really enjoyed chemistry and I really majored in it because of the laboratories. I enjoyed the laboratories immensely—all the way from freshman chemistry through physical chemistry.

TRAYNHAM: You spoke particularly strongly about the introductory chemistry course that won you over to being a chemistry major. Does any other chemistry course in your undergraduate curriculum stand out in a similar fashion?

GOOD: Oh, a couple. I particularly liked qualitative analysis. In fact, I think it's been a big mistake that chemistry departments have dropped this course. It was one of the most interesting

and fun courses. I enjoyed that. I also enjoyed the course we had in organic synthesis. In those days, the safety restrictions were not quite so strict and you could still do very interesting experiments. [laughter] I also liked the quantitative analysis class. The fellow who taught our organic synthesis class was very good. Now that was an excellent class. But the one class, I guess, that salvaged my graduate career was the course that he taught in organic analysis. What was that old course called?

TRAYNHAM: Qual-organic?

GOOD: Qual-organic. In fact, had I not had that course, I never would have passed the graduate-school qualifiers in organic chemistry. There's no question about that. The only course that saved my career was the course in qual-organic.

TRAYNHAM: When you graduated from Teacher's College, had you already planned to go to graduate school by that time?

GOOD: Well, I had because this same gentleman who taught freshman chemistry, Mr. Cordrey, was chairman of the chemistry department. He had talked to me for a long time about going to graduate school. In fact, I had an interest in going to what in those days was called The Carnegie Institute, because they had a really good program there in radiochemistry, which I thought would just be fun to do. I had also thought about going to the University of Chicago, where Mr. Cordrey came from. But the University of Arkansas offered me a fellowship, and it turns out that I went to the University of Arkansas <u>because</u> they offered me a fellowship. I really could not have gone without it. We just wouldn't have had the money. Because by the time I graduated, my sister had already started to college, and two years later my second sister started. So I took the fellowship at the University of Arkansas and went immediately. The semester after I finished, I went directly to graduate school. I have to say that the four years that I spent at the University of Arkansas in the graduate program is probably the most enjoyable four years that I ever spent. I just enjoyed every minute of it. I liked everything about it.

TRAYNHAM: You had already had been inclined toward radiochemistry before you went to Arkansas, then?

GOOD: Well, because of Marie Curie, who else? I mean, you know, in those days, there weren't very many role models around and I had read her material and it was just fascinating to me. In fact, I even had thought about the possibility, at one point, of being able to go to the

Sorbonne, but my French was so bad that it was hopeless. My Texas accent and French did not agree.

TRAYNHAM: Perhaps Madame Curie's French was a little troublesome when she first came from Poland.

GOOD: Her French was almost totally nonexistent when she went to France—well, that's not quite so true. They actually taught French in the Polish schools. Her French did have a severe accent. It apparently caused her some grief early on.

TRAYNHAM: You were fortunate that the University of Arkansas had a radiochemistry program when you went there.

GOOD: Right.

TRAYNHAM: With whom did you share your graduate work?

GOOD: With Raymond [Richard] Edwards, who was from Arkansas. In fact, he was from Fort Smith. His family ran a funeral home in Fort Smith and, as far as I know, still does. But he had gone to MIT [Massachusetts Institute of Technology] and gotten his degree with [Charles Dubois] Coryell, who of course was one of the major figures in radiochemistry in the early days. Both of them were very much involved in the Manhattan Project. Ray Edwards had actually done some really interesting work on what was known in those days as "hot atom" chemistry, if you remember. He came back to the University of Arkansas after the war as a faculty member, then he was appointed chairman. He, because of his connections to the Manhattan Project, managed to get a very large research grant from the Atomic Energy Commission [AEC] and I was an atomic energy research assistant. I was on a teaching assistantship in the first year. The second year, when I started working with Edwards, I got a research assistantship from the AEC, which was very interesting in those days. What that means is that I've had a secret clearance—the old Q-clearance—from the AEC [Atomic Energy Commission] since I was nineteen years old.

TRAYNHAM: Still active?

GOOD: No, actually, when I left the government, I deactivated all of my clearance. But from

graduate school forward, I actually had serious secret clearances, even when I was in industry, because of the kind of work we did.

TRAYNHAM: What was the nature of your graduate research in radiochemistry?

GOOD: It essentially was a continuation of some of the work that Edwards had done in the early days of radiochemistry, looking at the effects of radioisotopes—not the isotopic effects, but the effects of radiation on the surrounding chemistry. One of the things that we looked at was the disassociation of iodine molecules in aqueous solution. The reason for that was that radioactive iodine had already been found to be a very good therapeutic material for thyroid conditions. One of the biggest problems, however, was that Oak Ridge National Laboratory would ship what they thought was radioactive iodide, but by the time the hospitals got it, it clearly was a different chemical species. Why was that the case? It turned out to be a dilution effect, or a Nernst equation effect. If you calculate (through appropriate measurements) the chemical concentrations of the radioisotope and apply the Nernst equation, you can predict that disproportionation of the iodine species should occur. What people didn't realize was that you needed to add just a little bit of carrier iodide to the solution as it left the separation plant and it was as stable as could be. But if you didn't do that, by the time it got to people that needed it, you ended up with an absolutely wrong chemical material. So we did a lot of work on segregating and identifying the various iodine species.

TRAYNHAM: Was it your research that identified the necessity of having that iodide additive?

GOOD: Essentially that's right, yes. It also was important in the sense that we looked at the ionization effects in the aqueous solution to determine what other kinds of chemistry were caused by the radiation. This was really kind of interesting stuff in those days. It sounds very simple today.

TRAYNHAM: While you were in graduate school, I believe, you were married?

GOOD: That's right. Bill and I got married. In fact, I married my old lab partner from physical chemistry. [laughter]

TRAYNHAM: You'd both gone to Fayetteville?

GOOD: Both of us had gone to Teacher's College, actually. Bill had come there in 1946 or 1947 on the GI Bill. We both graduated in 1950 and we both went to the University of Arkansas on graduate fellowships—he majored in physics and I majored in chemistry. We were married in 1952, the second year I was there.

TRAYNHAM: While you were in graduate school, I believe your first child was born?

GOOD: That's correct. He was born in October of 1953, and in those days it worked out really quite well because you still had large numbers of GIs and their families. In fact, we lived in a place at the University of Arkansas, which no longer exists, called "Terry Village." It was a big housing development that was built during the war for military students and their families. Then after the war, it was used for married student housing and it was full of people, so you had no problem finding a baby-sitter. Everybody was essentially in the same boat and so it worked out very nicely.

TRAYNHAM: You then graduated from Fayetteville with your doctorate degree?

GOOD: I actually finished my thesis in the summer of 1954, went to LSU [Louisiana State University], and then came back to Arkansas in January 1955. My degree says 1955 since it was January of 1955 when I received it. The reason I came back to graduate was that, in those days, the University of Arkansas had so few Ph.D. students that they would not send you your diploma unless you came for the graduation exercises. [laughter] They would send a piece of paper saying that you had completed all the requirements, but they wanted a few folks to walk across the stage. So you couldn't get your diploma unless you came to pick it up.

TRAYNHAM: Must be present to win.

GOOD: Must be present to win, right. [laughter]

TRAYNHAM: How did you happen to choose LSU as your first employment?

GOOD: Well, that's really an interesting story. First of all, Bill wanted to finish his Ph.D. in physics, and at that time the University of Arkansas only offered a master's degree in physics. During the last year we were in Fayetteville, he had actually been an instructor in physics, where he taught sophomore physics. So we were looking for a place where I could work and he could

go to school. I had an offer from Shell Oil in Houston, and he clearly could have gone either to the University of Houston or to Rice University. We looked seriously at that. I also had an opportunity to go to Hanford—the Hanford Works in Washington. The problem was that unless you went to Washington State University, there really weren't many other options. LSU was looking for a director for the radiochemistry laboratory. Dean [Arthur] Chopin and Ray Edwards knew each other as friends. Apparently Doc Chopin knew about the radiochemistry program in Arkansas and called Ray Edwards to see if he had anybody, and he said yes. So they called me and I went to LSU for an interview. Bill and I decided that LSU was a really nice arrangement because I could teach and he could go to school. He liked the physics department, so that's the option we picked.

TRAYNHAM: Now, we're back in the days when the "old boy" network worked.

GOOD: Absolutely. That's right. In fact, without those kinds of contacts in those days, I don't know how students found places, to be honest. Some of the industry would come and interview and would look for people, but the universities did not advertise in those days at all. Unless you had contacts, or your major professor helped make those contacts, I don't know how people made out.

TRAYNHAM: Tell me something about your career, your initial full-time employment as a chemist?

GOOD: Well, my first full-time employment at LSU was exciting because—you remember as well as I do—those were the days before Coates laboratories were air-conditioned. Quantitative analysis was taught on the second floor. It wasn't so bad during the winter, but I needed the money, so I taught quantitative analysis in the summertime. [laughter] That really was a trial, I can assure you, because the temperature in that laboratory, I'm sure, got to be 105 or 110 degrees in the summertime. Both students and faculty were wrung out by the time we got finished. So I remember that quite vividly. I came as an instructor because in those days, you didn't really start as an assistant professor. The other thing that I remember is that Doc Chopin was a very interesting man. He always—you know as well as I do—back in those days, ran the department as an autocrat. He might tolerate faculty participation a little, but he didn't pay much attention to it. He always tested people to see whether they could make it or not. So he assigned me, the very first semester I was there, to a freshman laboratory lecture that had three hundred freshman students in it, in a huge auditorium. The first two times I taught that thing, it was just chaotic hell. People paid no attention, nobody paid any mind to what you said. Nobody cared about that laboratory anyway. All those kids were in there because they were required to take it. So I commandeered two graduate students—two teaching assistants—and we put a seating chart together. I said, "You put them all down in a seat. They're all numbered.

When they come in, tell them what their seat is and you all stand at the ends. Anybody who gets up, says anything, or throws his book, take his name." After that, everything worked just fine. [laughter] But I also remember trying to call the role because the French names down there were very difficult. I had one class that had two students—both whose names were spelled R-I-C-H-A-R-D. One of them insisted on being called "Rishard," and the other one was "Richard," and you had to know who was who. They got very insulted if you got that mixed up. So there were some very interesting teaching assignments in those days.

I was actually able to get some research started very quickly because, once again, I had the same "old boy" network through the AEC that my major professor had; obviously, coming up through the AEC ranks. So I got an AEC grant very early to do some research. The first one was essentially to continue what we had started at the University of Arkansas. Then I moved into some new areas. I did some work early on with Seán [Patric] McGlynn. He was interested in doing the spectroscopy of some molecular complexes with very weak interactive forces. Knowing a fair amount about iodine chemistry, my students and I made a bunch of iodine adducts for him, which he then subjected to spectroscopic measurements. He also used the results for further theoretical calculations. He was also interested in sulfur compounds where you looked at the molecular complexes between organic sulfur molecules and the halogens. We made a number of disulfides and sulfur compounds and we were in a laboratory immediately above Phil [Philip William] West's laboratory. At that time, Phil was working on the very early stages of gas chromatography.

So we were upstairs and he was downstairs and his students had their detectors out the window trying to pick up pollutants because he was one of the very early people looking at environmental chemicals, as you remember. So they had their detectors out the window seeing what they could do with gas chromatography. We had an old hood on the second floor that worked—sort of, but more importantly, that hood vented out the <u>side</u> of the building. [laughter]

[END OF TAPE, SIDE 1]

GOOD: So West's group was just measuring stuff we were pouring out of our hood from upstairs. [laughter] Well, as you can understand, me being an instructor and Phil being a professor—I think he might have been a Boyd Professor even then—we got moved to a different laboratory space. [laughter]

I also managed the old radiochemistry lab in those days and that was kind of fascinating. We actually did the radiochemistry, not only for the work that I did, but also managed the space and did some of the radiochemistry for all those physics people who were doing nuclear energy levels. You remember [Max] Goodrich and that group, who did some of the very early work on nuclear energy levels. I was actually sort of overseeing their work, from a safety point of view. It was very difficult because those physicists didn't care about that sort of thing. They just

wanted to make their sources and get their results. They had worked that way for a number of years—even before I got there. However, that laboratory was the source of an awful lot of nuclear work that was done in physics, as well as some of the work that was done in chemistry.

The teaching part of the assignment I always found to be very exciting—I always enjoyed teaching, and I liked research. I liked working with graduate students, and in those days I had some really interesting students. LSU did not have a large number of graduate students in those days, but we had some very interesting ones. It was the period right after the Cuban problem, and we actually got from Cuba some very good students—people whose families had immigrated. We also had at that time a number of Palestinian students. How they got to LSU, I don't know, but we did. Some of them were just excellent. In fact, I followed the careers of two of the female Cuban students for a very long time. I don't know where they are today, but every once in a while I run into one or the other of them. Both of them now work for the industry.

TRAYNHAM: We'll get to your very significant management position later, but I'm just prompted to ask, do you think that your management of the radioisotope facility at LSU had prompted your interest in management?

GOOD: No, I don't think so. In fact, it probably should have turned it off, if anything. However, I will say it was a very good learning experience for management. Here you had a young, twenty-three-year-old instructor, who was director of the radiochemistry laboratory, telling a full professor, Professor Goodrich, that he could not do what he was doing. [laughter] This was very interesting, indeed, as you can appreciate, but we did manage to shape it up reasonably well and got them to the point where they at least did not poison themselves, which was helpful. Because in those early days, people really didn't have much of an appreciation for radioactivity; particularly, the people who just used it for sources and things like that.

So I think you learn how to handle those kinds of situations and, to be honest, the management skills are—you know, all this business about teaching management skills and all that—primarily just people skills. If you can manage the people and get them to do what it is you want them to do, that's management. I learned a great deal about how to manage that without getting fired or facing other problems, because I had to write the sheets for the AEC, certifying that we had handled this material in certain ways that they recognized as being appropriate.

TRAYNHAM: Well, it sounds as though you had a very good start to your academic career at LSU with a variety of experiences in teaching, research, and management.

GOOD: Right.

TRAYNHAM: What prompted you to cut short your career at that institution?

GOOD: Oh, that's very simple. Bill finished his Ph.D. in low temperature physics with Joe [M.] Reynolds there and then did a year of postdoc with Joe, which he enjoyed a lot. But then we needed a position where both of us could teach or have some other career. So we spent the year that he was a postdoc looking for academic positions. Clearly, it wasn't feasible for him to stay at LSU. That didn't make a lot of sense. We actually were interviewed by the University of Arkansas and that was a very interesting encounter because we were both offered positions, but with the stipulation that only one of us could have a tenure-track position. That was in the old days of rules on nepotism. So we didn't take the positions. You never know whether these things are the right things to do or not. Probably, the rules would have been changed over time, but they certainly weren't friendly at that time. One of the reasons that we could turn down the Arkansas offers was because of the other offer we got from LSU. So we had the LSU opportunity and one at the College of New Bedford in Massachusetts. They were trying to build the College of New Bedford and offered us both positions. Each of us had offers at other places, but we couldn't get two together, which by the way, is not an unusual problem today. As you know, you still have a lot of two-career families where this is the same problem.

LSU was making preparations to open the LSU campus in New Orleans. Homer Hitt was appointed the first—I guess he was dean the first year. You know, he came from the rural sociology department at LSU. He and three or four other major folks from LSU—including George Branum, who had been in the English department (and I guess had been chairman of English department at LSU), and Lamar Cooper, who came down as the business manager. They came to New Orleans to open LSU in New Orleans. Homer offered both Bill and me really good positions because he needed to get the College off the ground and he needed people, he had to hire a whole faculty. So he offered both of us associate professor positions, and we took them. Both of us got good tenure-track opportunities. The other reason we accepted was that the idea of building something from the ground up was intriguing. Our backgrounds were such that those sorts of things didn't seem like impediments. You know what I'm saying, if you have to build some of your own equipment, so what? We were used to doing that.

So, it was an interesting transition. I enjoyed the LSU Baton Rouge campus. I mean, I would have been perfectly happy to stay, but family circumstances just made that almost impossible to do. But I got a good start there. I was able to move my research, of course, almost without any problem, to New Orleans.

TRAYNHAM: You had a good career at New Orleans.

GOOD: Yes, I did. The first few years down there were truly exciting in many, many ways. We opened in 1958 and you remember the fight in Louisiana over the area where the university was going to be built. Actually, the university managed to get what is a beautiful piece of property. It belonged to the Levee Board. It had been the old New Orleans Naval Air Station and after the war it was decommissioned and given back to the Levee Board. The Levee Board is actually appointed by the governor, but the air station was within the city of New Orleans. [DeLesseps Story] "Shep" Morrison, who in those days was the mayor of New Orleans, decided that he wanted that piece of property put on the public tax role. He wanted to open it all up for residential housing because it is the most desirable part of New Orleans in which to live—out there on the lake front. Earl [K.] Long, who was governor at that time, decided that he really wanted a campus that would have his signature on it. LSU-Baton Rouge had been the pet project of his brother, Huey [Pierce Long], and so he really wanted to do this. Furthermore, he disliked "Shep" Morrison more than anybody. So he decided that he would see that LSU got that piece of land.

Well, Morrison tried to keep it from happening and he talked the Levee Board into voting against it. What he didn't remember was that he didn't control their appointments. So Earl simply un-appointed all of them, re-appointed a new board and guess what? They voted to give the property to LSU. [laughter] That's how LSU got that very choice real estate. Without Earl Long's intervention, it would not have happened. Morrison would have put them in an old piece of property in a very small space in downtown New Orleans. The growth of the campus would have been very different, indeed, had that been the case. It really would have been a very different story altogether.

So the property was wonderful, but we opened the university in the old Navy barracks because that's all that was on it. We opened it with a freshman class in 1958 and it was the only integrated—by court order—campus in the south at that time, which was an experience to end all experiences in many ways. We opened with fifteen hundred students, five hundred of them Black.

TRAYNHAM: Nearly all the students were from New Orleans?

GOOD: All the students were from New Orleans, there were almost no students from anywhere else, certainly in the first few years. They were all New Orleans students, and for all practical purposes, they were all first-generation college students. They were students whose family had never been to college, who'd never had the opportunity, kids who didn't have any money. Fees, if you remember, at LSU in those days were not very high. Kids could just come across town, and so we opened in that mode. We had nothing. We stocked the laboratories from scratch. In fact, Homer Hitt hired those of us that he could to begin on June 1. The reason was so that we could help get the laboratories and the classrooms ready for people to come into. So we spent the summer buying and stocking labs, and getting ready for the freshman classes.

The students were interesting, as well. We had some very good students who came from some of the schools in the city who just didn't have any other opportunity to go elsewhere. They couldn't afford Tulane [University]'s or Loyola [University]'s fees. Remember, at that time, New Orleans was the largest city in the United States without public higher education. So it was just natural to put the university there. We spent the summer, essentially, stocking laboratories and getting ready to go. The students that came ranged all the way from some of the brightest students that I've ever had to students who truly were illiterate. I mean, that's a fact. You remember we had no standards. Anybody who had a high school diploma could come, and clearly some of the public schools in New Orleans were just unbelievably bad. So some of these kids never had the opportunity to have any sort of an education, so the failure rate on that first class was just terrible. The faculty—and to give him credit, Homer backed it—decided that if the school was going to have any future, they had to apply standards and make them stick. Take anybody, but don't graduate everybody. Of the first class, my guess is that out of those fifteen hundred, no more than two hundred and fifty or three hundred students eventually graduated.

But those who did graduate were really good kids because they got a lot of personal attention. Any you could salvage, you tried. We also built research laboratories in those days. We managed to get cast-off equipment and cast-off furniture from LSU. LSU was extensively remodeling at that time, so we managed to get laboratory equipment, including hoods. We built a research facility in one of the old barracks. They just gave us the building. They said, "We're not going to use this building. If you guys want to do research in this building, fine. You can do whatever you want to it." Which turns out to be absolutely wonderful, because if you can do what ever you want to it, then you can set your stuff up wherever you like. You can design it however you like, and it was fun to do.

We had some very good faculty at that point, for reasons that are still unclear to me. Albert [Irving] Meyers came that first year and, as you know, became a Boyd Professor in organic chemistry and is now a member of the National Academy of Sciences [NAS]. He has been at Colorado State University for quite a long time now. He came from New York, primarily because good teaching jobs were hard to find at that moment. We just had a very interesting mix of folks. Jack [Hubert] Stocker came over from Tulane. We also picked up some people who we should not have. [laughter] There were clearly some of those, as well. Over the years, we weeded them out. I mean, it was amazing to me that we were able to get as many of what I consider to be really good people. The art of building something like that is really invigorating—you don't get an opportunity like that very often—and it was an enormous lot of fun and provided a great deal of satisfaction. And it is interesting. I still keep in touch with several of the students that were in that first class, just because we got to know them so well.

TRAYNHAM: Those students must have, in a sense, had an opportunity to experience what you did as an undergraduate—having to make do with limited equipment and make some of

their own.

GOOD: Exactly. The university began to build and we got building monies and built new facilities. Today, it's a very attractive campus. I was down there last year at the ACS [American Chemical Society] meeting. Nobody would ever remember what it looked like in 1958.

TRAYNHAM: You built a research group there and were very successful with it, I believe, but you didn't stay with the radiochemistry, did you?

GOOD: No. Well, yes and no. We still did some of the radiochemistry work when I first went down there. After the work I had done with Seán McGlynn, I got very much interested in physical inorganic chemistry, particularly spectroscopy, which in those days was a fledgling area, if you remember. In the late 1950s or early 1960s, people were just beginning to use those types of physical tools to truly understand molecular systems better. Those were the early days when people like Al [F. Albert] Cotton were looking at metal/metal bonding and other inorganic structures. Crystal field theory had just begun to catch on in inorganic chemistry. Things like molecular orbital chemistry for inorganic people had really not started—it was a later development. But the whole idea of being able to take spectroscopic equipment, and to begin to understand what the bonding in molecular systems looked like, and being able to describe that in quantitative ways, was developed during a ten-year period from the late 1950s until the end of the 1960s. It was a very, very fertile field for people. It was particularly advantageous for people who knew how to make compounds. We had always made our own materials for other kinds of studies, so we knew how to make compounds. People had also begun to look at the theory of inorganic systems. So you could actually make new compounds, make measurements on them, and make comparisons between your measurements and the theory in ways that had not been possible before the development of spectroscopic equipment after the war. You know, when I was a graduate student in the early 1950s, the only spectroscopic tool we had was an old Beckman DU, if you remember. We could do solution work, and an old Perkin-Elmer infrared machine allowed you to do a little bit of solid states work, but it was pretty primitive. By today's standards, it would be very, very primitive. But the equipment got better and we were beginning to learn how to use it and how to integrate experimental work with theoretical calculations.

One new technique was to use radioactive nuclei as a measurement source in an atomic spectroscopy called Mossbauer spectroscopy. In Mossbauer spectroscopy, you had a very narrow energy line for your spectroscopy tool that could be used to look at the very small differences in energy between electrons within atoms. For example, in an iron atom, you could look at the difference in the energy of an electron in a bonding orbital versus one that was not. In other words, you could actually see the difference in the energy levels between, say, an iron

atom in an oxide, and an iron atom in a sulfide. So all of a sudden, you actually had a tool that allowed you to really look at and measure those energy differences, which the theorists were trying to calculate. Being a radiochemist, I could do the sources. I understood how to make the sources that would allow you to do that sort of spectroscopy. So the radiochemistry was a help all through those years, because we did a lot of tracer work, as well, where you could use radioactive "tags" to follow reactions and determine reaction pathways.

TRAYNHAM: Mossbauer spectroscopy studies won rather extensive acclaim for your research, I believe?

GOOD: Yes. We got lucky, as sometimes you do, because at that time we had been very interested in ruthenium chemistry for two reasons. We still had money from the Atomic Energy Commission in those days—research money—and they were very interested in the behavior and the chemistry of the fission products that were still in those pots on those big tanks at Oak Ridge. You know all of the hoopla that's being discussed about them now. Well, if they had followed through on what some of the chemists that were working on those problems at that time were talking about, they would have been in better shape, but they didn't do that. Anyway, they were interested in those fission products. Ruthenium is one of the fission products that was always difficult. It exists in so many oxidation states that when you get ready to try and extract it out of a mass of material, it's hard to do because you get one oxidation state, but you don't get the others, and it disproportionates quickly. So we were very much interested in that. The nuclear levels for ruthenium suggested that you ought to be able to carry out ruthenium Mossbauer experiments, which meant that, all of a sudden, you could actually talk about the differences in those energy levels within the ruthenium systems.

So we set up to do that. I had a graduate student who decided we'd tackle that for his thesis. We had done a number of really good theses using iron and tin Mossbauer spectroscopy, but ruthenium spectroscopy was really difficult because it had to be done at very, very low temperatures. The experiments had to be run at liquid helium temperatures. In those days, liquid helium containment was done in glass dewars with ground glass fittings. You didn't have all these metal dewars with nice windows and all the stuff that's been invented and constructed since that time. We were still running these experiments in glass dewars. And, of course, you also had to have very thin windows because you wanted to analyze the radiation energy coming out. We decided we'd give this thing a try. Well, most people thought this was going to be hopeless, because nobody had ever seen the Mossbauer effect in ruthenium. It turned out that Rudolph Mossbauer (the inventor of the original spectroscopy) decided that he would look at ruthenium at about the same time. So we set the experiment up. We knew where the energy levels were. We knew where we were supposed to be looking, but the gear was—by today's standards—really quite primitive, frankly.

I had two graduate students working on it and we tuned the equipment. We worked on

it, got it all set up, got the source out of Oak Ridge, and finally got the experiment set up and running. We didn't find a thing! We ran the spectrum and it was flat as a pancake, no resonance peaks anywhere. So we kept at it for two or three days and finally one of these graduate students, who was a bit older, came in on Saturday morning and said, "Okay, we're going to run this thing one more time. If we don't get a signal, I'm going to go play golf." So we tuned it up and found nothing. I said, "Look guys, let's re-tune it and let's do it one more time." "All right, we'll humor you." Sure enough, all of a sudden this beautiful little resonance peak comes out of the background and we had it made. From that point on, you knew where you were and we did some really nice work on ruthenium compounds.

TRAYNHAM: What do you think was the barrier to getting the resonance peak in the earlier trials?

GOOD: My guess is that it was as much luck as anything, because the detector systems that we had, and particularly the amplifiers, to do this detection were all analog in those days. You know what I'm saying. You had to get exactly in the right position and remember, you're looking for differences in energy of about 10^{-9} ev, so you don't have much leeway. I think as much as anything, it was in the tuning system. I mean, today you'd have no problem at all. The way we tuned the equipment was to use a little Doppler motor that does the same thing that a speaker does when you put sound in it and you put a signal on it and it goes back and forth. You tune the signal through the Doppler effect. The motors (or drivers) were fairly primitive. But once you see the signal, you know where you are, you know what you've done to get there, and then you're okay.

TRAYNHAM: After that first success, you didn't have problems getting the resonance after that?

GOOD: No, after that, we didn't have problems. The only trouble we had later on with the ruthenium compounds was that many of them had such diffuse energy levels and molecular structures that they spread the signal. In other words, you'd get mixed oxidation states and therefore you'd get two mixed energy levels and sometimes, they'd be rather close together. So the quality of your spectrum was not as good as in some other cases. But on some of the compounds, we got some very nice results and we were able to resolve some really interesting questions in the literature.

TRAYNHAM: That research became internationally well-known and lead to your designation as Boyd Professor at the university?

GOOD: That's probably right. I suspect the Mossbauer spectroscopy results were what really gave us our best scientific recognition. People noticed that work outside of LSU. The other work that we did about the same time was also, I think, part of it. We did some of what I consider, still today, to be some of the very best early work on metal complexes in non-aqueous systems. From my old days of radiochemistry and using solvent extraction techniques, we were able to use large quaternary salts that were organically soluble. We'd use those positive quaternaries to extract into an organic solvent inorganic ions like $FeCl_4^=$, for example. These complexes were now in an organic solution and we could do far infrared measurements on them and get their inherent structure in solution where you could really talk about what the bonding really was. If you do the experiments in solids, you get all the crystal effects, as well, and you don't actually get the inherent rotational energies and the details of the complex's spectrum.

Some of our work was really quite well-received and pioneering, as well. In fact, we had two major successes, in my opinion—at least the students thought so. First, we got into print with our ruthenium Mossbauer experiment before Professor Mossbauer did his ruthenium experiments. We thought that was kind of nice. The other one was that we managed to extract a rhodium complex, which was a bimetallic rhodium/rhodium chloride complex, and we actually got the infrared structure for that and were able to show pretty unequivocally that it was bimetallic. The characterization of these rhodium/rhodium bonds scooped Al Cotton's group, for which I don't think he's ever forgiven us. [laughter] He had worked on this complex for a long time. But since that time, Al and I have been friends because he always remembered that paper.

TRAYNHAM: Well, he was able to go on and do some other things.

GOOD: Oh, yes, indeed so. In fact, he really is a very prolific inorganic chemist.

TRAYNHAM: Just for the record here, I'll mention that Boyd Professor is the highest, most distinguished rank in the LSU system. Were you the first person at the New Orleans branch of the university to receive that honor?

GOOD: No, Al Meyers was the first Boyd Professor. Right. I was the second one.

TRAYNHAM: The second one, all right. Well, the achievement of that highest, most distinguished rank was a real source of pleasure for you.

GOOD: Absolutely. I mean, there's no question about that. In fact, when I found out about it, it was 1974 and we were on vacation in Scotland, when somebody sent me a telex, actually, saying that they had approved it. To be honest, I really did not know that the nomination had gone up at that time and it really was—just one of those things that's extraordinary. The reason that those kinds of honors are so important and people take them seriously is that they really are recognition by your peers and that's kind of different. These are people you work with all the time and people you know and people who are quite free to criticize, also. [laughter] So it was really a very nice honor. I guess, in terms of firsts, I was the first woman at LSU to ever receive one. I think there are now several, but in those days there were not.

TRAYNHAM: Was there anything about your career at that time that was less than happy?

GOOD: Not at that time. Actually, things were really going very well. We had built a pretty decent department in New Orleans. The group there was doing some very good work.

[END OF TAPE, SIDE 2]

GOOD: You know, there was always friction back and fourth between LSUNO [Louisiana State University-New Orleans] and Baton Rouge. We had fought very hard to get the Ph.D. program in New Orleans, which the Baton Rouge campus had opposed. One would expect that, but we also had some good collaborations at the same time that all this discussion was going on, with people on the Baton Rouge campus. There was a fair amount of back and forth in those days. We also had very good working relationships with the chemists at Tulane and some at Loyola. In fact, in the 1970s there was a really—what I would consider to be—very active, very vital academic environment in New Orleans between LSUNO, Tulane, and Loyola. There was a lot of interaction between the physics departments and a lot of interaction between the chemistry departments.

That, I understand, has pretty much disappeared today. Loyola's programs have sort of dissipated. Tulane went through some very bad times. I understand they're coming back now fairly well, but in those days it really was a very vital kind of a place. There were very good symposiums going on all the time. I, at that time, was a member of the Review Panel for the General Medical Sciences of NIH [National Institutes of Health] and we were able to bring together a major conference there, sponsored by that review panel, on bio-inorganic chemistry, which was probably one of the best that's been held almost anywhere. So there was just really a lot of good stuff that went on.

Things were really going very well. Like I say, the friction back and forth with respect to the Baton Rouge campus was ongoing. About that time, the chancellor at the New Orleans

campus decided that one of the ways to get around these issues was to change the name of the New Orleans campus; he was the one who wanted to change it to the University of New Orleans [UNO]. We kept trying to insist that regardless of the problems, UCLA [University of California-Los Angeles] would not have been what it is today without having the University of California as part of its name. The faculty really wanted to keep the LSU designation, but the administration felt that they'd get better support out of the city, and they'd get better support out of the New Orleans legislators, and so forth—which is probably true. Except that the problem is that it's a state-supported school. It's not supported by the City of New Orleans.

There was a big, big discussion about all these issues. At that time, I was chairman of the Faculty Senate and we had a major discussion about it. We had a vote, and the faculty voted not to change the name, but the chancellor pursued it anyway and actually got the LSU board to change it. I still think it's probably a mistake, but I think it's a mistake for the state, not necessarily for the school *per se*. Those were some of the political issues. You know universities are hotbeds of political issues anyway.

Then, in the late 1970s, we had a group of faculty, a couple of them in chemistry, who decided that they were not getting their share of what was going on at the university. They decided that what they wanted to do was to topple some of the deans. It was not just in the sciences, and although it was a minority group, you know minority groups in faculties can create major problems. They decided that the deans had been there too long and they all needed to be booted out. So what the chancellor did was to put in a review system that said that all the deans would get reviewed every five years. What this group of dissident faculty tried to do was to be sure that the reviews all came out badly [laughter] so that you'd have a shot at taking these people out. Well, my husband was the dean of the College of Sciences at LSU-New Orleans in those days. They attacked him, the dean of the College of Business, and I guess the dean of the College of Education was the other one that was really in their headlights. But they didn't make as much headway as they'd have liked. So they decided to attack my husband by saying that my Boyd Professorship and all the other things that I had received as a faculty member were only because I was the wife of the dean. That was very unpleasant and, indeed, they actually got the Louisiana Ethics Commission involved. I don't know whether you remember that or not, but they got the Louisiana Ethics Commission involved, and that was a very unpleasant time, no question about it.

It was always interesting to me, because it's kind of like some of the accusations and the scandals that are going on today. Once accused of those sorts of things, there is no way to put them to bed. There simply is not. I mean, there is no way because you're put in a position of trying to prove what you did not do. There's no way to do that, ultimately, although the university held a hearing and no misconduct was found. Then the same group sued the administration and that finally got settled in court. It was settled in favor of the university, pointing out that there had been no skullduggery going on, but the problem is it didn't make much difference. All the damage has been done anyway. So that was unpleasant, to say the least. When I was offered a position by the dean of [the School of] Engineering to come back to

LSU as Boyd Professor of Material Science—he was trying to get a material science program started at LSU—I did. I decided that it would be better to move back to the other campus and just get out of the situation. There were two problems with that. One was that to try to develop the materials science program required real cooperation between engineering, chemistry, and physics. With all of our skills and effort, we couldn't get that cooperation going. It never got high enough on anybody's priority list to make it actually work, and I think that was a big mistake because I think it was an area in which LSU had some real horsepower if it could have been organized. There was a really high-quality group in solid state physics; you had some very good, well-known, world-class solid state spectroscopists in chemistry; and talent in the engineering school. Actually, the engineering college didn't really back it, either, because it meant that you had to have cooperation between mechanical engineering and electrical engineering. [laughter] They didn't perform very well either. There was just no way to get it done. What the university wanted to do was compete for a materials research center. Actually, if we could have gotten a proposal put together, in which we could have gotten those three groups of people to participate, I think we would have gotten it. It was in an era in which they were trying to do a better geographic distribution of some of those kinds of facilities. I think LSU would have competed very, very well. But we just couldn't get it coordinated, and whether we would have been able to, with another couple of years' effort, I don't know.

TRAYNHAM: Was your role mostly research?

GOOD: Primarily.

TRAYNHAM: Its direction or management of the program?

GOOD: Well, it was to try to manage the process. It was to try to develop the program and do my own research. I also actually taught classes in mechanical engineering. I taught their materials science classes, which are nothing more than solid state chemistry, frankly. [laughter] I taught some classes for chemistry, as well, some advanced spectroscopy graduate courses that were taken by the graduate students in chemistry.

TRAYNHAM: Did you have any feeling of discomfort of being identified with engineering?

GOOD: No, not at all. In fact, the difference between chemistry and engineering is not high, anyway. If you read the technical press—it's really funny to me—we now have this enormous argument going on about what basic research is, what applied research is, and where engineering

fits. You have people who define basic research as that research which is done with no thought about what its value is going to be. Well, I think that's hilarious because I don't believe I have ever read a chemistry proposal that does not at least in the first three paragraphs set out why, if their work is successful, it's either going to let you do something you haven't been able to do before, or it's going to give you insight into some practical problem that everybody is interested in—you know what I'm saying. When you think about it, yes, sure, there are theoretical people in chemistry, but when you compare polymer chemists and solid state chemists, there's not that much difference between chemistry and engineering. I like engineering anyway. I like the applications and the fact that you can take something that you've done and you can actually see that develop into something that the engineers can take and build something from. I think that's kind of a fun thing to do.

TRAYNHAM: Before you took that position, trying to create the material science program at the university, had you been involved in engineering applications in your research?

GOOD: We had done some. For example, we had some major research money from the Navy. We had been looking at their antifouling coatings and trying to figure out ways to improve those both from the point of view of giving them better compounds, but also figuring out better ways to make them adhere, figuring out ways to get them to release slowly. We did some of the early work on the organo-tin compounds, which, as you know, are still being used because they're the best material around, but people don't like them very much because they're an environmental problem, as well. But yes, we had done some work. We had gotten into doing some surface spectroscopy, where we could actually look at the coatings and make predictions about how to make them better. Although it wasn't an engineering project *per se*, it was awfully close.

TRAYNHAM: Well, after you had been at LSU in the appointment in engineering with material science for—two or three years?

GOOD: Yes, two years, right.

TRAYNHAM: Two years. You left academia. Was that solely because of the attractiveness of the industrial offer or was it frustration trying to bring these disparaged groups together or both?

GOOD: It would be hard to say that the frustration of trying to make that work at LSU at that particular time didn't have something to do with it. However, the most important reason for doing it was the challenge of doing something so very different—I just wasn't able to turn that down. I had no idea whether I could be successful or not. I'd certainly never managed anything

of that magnitude.

TRAYNHAM: Do you have any idea of how UOP [Universal Oil Products] happened to identify you as the one who would do that job?

GOOD: Oh, yes, I do. I had been very active in the American Chemical Society. In fact, I had been chairman of the board of the American Chemical Society. I had been elected to the board in 1972. In fact, I was the first woman ever elected to the ACS board, which is kind of interesting. I was chairman of the board in 1978 and again in 1980, and at the time I was chairman of the board in 1978, Herman [Samuel] Bloch, who was a director of research at UOP, had been president of the ACS. No, I guess that's not right, he was on the board. The year he was chairman of the board, I guess I was chairman of the Publications Committee. Anyway, I got to know Herman very well and they began to look for a new vice president for research at UOP in the late 1970s. UOP is a very interesting company. It's a company that depends on fairly fundamental work because they depend on licensing their technology, which means that you have to have the best technology and you have to have it first. There's no place for second-place players and they license process technology all around the world.

They had had a succession of people as research directors who had been chemists, or at least very chemically oriented. In the 1970s, they had hired from GE [General Electric Company] a person who was known for his management skills rather than his technical skills, because they thought they needed to manage the laboratory better. Well, it turned out to be not a very good choice. So the president of UOP decided that to cure that, he had to have a chemist. He did not want an engineer, but he wanted somebody who actually had some practical experience and who was willing to work with engineers on a day-to-day basis. Herman, actually, was the one who sent my name forward.

The first that I knew about it was from Val [Vladimir] Haensel, who, of course, is probably the most famous person ever to come out of UOP because he's the guy who invented the platforming process. In fact, he designed the very first commercial use of platinum catalysts in refining. In the late 1970s, he was the senior vice president for technology for UOP. They had a number of other companies, other activities, but the old UOP, the refining and the petrochemical part, had a vice president for research of what they called the Process Division. He called me up one day and he said, "Would you be interested in talking about an industrial job?" I said, "Val, I have no idea. I never thought about it. I'm perfectly happy with what I'm doing. I've got good research money. I've got good students." He said, "Well, I want to come down and talk with you." Well, at that time, he was doing some work with the Exxon Refinery in Baton Rouge. He said, "I've got to come to Baton Rouge next week. I've got an appointment out at the refinery. I would like to have some time with you." I said, "Fine."

I picked him up, and we spent about three hours discussing the possibility of joining

UOP. He said, "I really would like for you to at least come and interview." So I said, "Well, I'll talk to Bill." We said, "Okay, what the heck." I went up to interview and it was just kind of exciting. The UOP laboratories in those days had about five hundred people and the development laboratories probably had about another five hundred people, but the VP and director of research actually had the five hundred research people. Your job was to manage them, be sure they were doing cutting edge stuff at the same time that they passed that off, and interacted with the engineering development group quick enough that you could get it developed and ready to commercialize in a time frame that would make money. [laughter] That's what it's all about. It was just exciting. It still absolutely amazes me. I went once to interview and to give a seminar—UOP actually did most of the early work on the automotive catalysts, and they had tried ruthenium in those catalysts. Ruthenium is a very good material in an automotive catalyst. The only problem is that it oxidizes very quickly and so it's not usable. So I had done some fundamental work using ruthenium Mossbauer experiments to prove that as you cycle the catalysts, every time you cycle you got more oxide than you did the time before. Ultimately, over about a hundred cycles, you just couldn't recover the metallic activity. I decided that these guys were all experts in catalysis, and it would be a mistake to do that catalysis talk. Instead of doing that one, I did the one on the surface spectroscopy of the tin compounds because they had not at that time begun to use things like ESCA [Electron Spectroscopy for Chemical Analysis] and surface spectroscopy to look at their catalytic materials. It seemed to me this would be really a nice thing to do. So I gave the seminar and it seemed to work out very well. I had a lot of conversations with the various people. They then invited me back. I came a second time about three weeks later and after I had gone over to visit with the research folks, I came back to the president's office. He just sat down and wrote me out an offer. I said, "Well, give me a couple of weeks and I'll decide." I went back and talked with Bill. Bill, my husband, had left the deanship two years earlier and had gone back to physics. He had always wanted to paint. So he had spent two years working with an accomplished artist [Louise Beeson] in New Orleans, who is probably one of the best known restorers of oil paintings in the country. He really enjoyed the experience. He said, "Look, if you want to take it, do it and we'll move to Chicago." They worked out a way to get him a teaching position up there and he said, "No, I don't want to do that. I want to paint for a couple of years and if it turns out to be good stuff, that's what I want to do."

So I took the position. I took it primarily because it was just a challenge you couldn't turn down. I had no idea about the details. I didn't know enough to know I couldn't do it. [laughter] To this day, I still don't have any idea why the president hired me, because UOP's Process Division, in those days, was managed by hard-nosed chemical engineers, all of whom had had field work. To say the least, these hard-hat guys had never seen a woman in a position like this, and a chemist to boot. You can't imagine. I mean, this was a shock, I'm sure. I mean, give me a break—a university chemist, for God's sake? But we got along fine, actually, after I got up there. Like I said, I didn't know enough to know I couldn't do it.

TRAYNHAM: Well, apparently the president was unusually astute because after you had been

there, what, two years, you were selected as industrial scientist of the year.

GOOD: That's true, which is still a mystery to me.

TRAYNHAM: Do you have a clear idea of what activity you did that brought about that particular award and recognition?

GOOD: You know, this whole re-engineering that has been going on in industrial chemistry laboratories, and in the industry in general in the last few years—at UOP we had begun to do that much, much earlier. The reason was that UOP makes its money by designing a process that it can license and then designing the best catalyst in that process. What they do is they get you coming and going. You license their process and then you buy their catalyst. You can ask a premium price for that catalyst if it makes that process run 2 percent better. Two percent better in a refinery where you are refining millions of barrels of oil is a huge profit line. So the whole deal at UOP was to do fundamental work in the catalysis area, improve the process and get that converted to something the process guys could actually sell, manufacture, and install, as quickly as possible. We had iteration going on between some very fundamental chemists and some applications engineers all the time. We had all kinds of friction associated with that, okay?

But I did two things when I first came. One was that I was appalled that the laboratory had no computing equipment, or very little. They were still—in 1980—using punch cards that went to the mainframe IBM [International Business Machines Corporation] machine across the street in the administration building to do scientific problems. So the first thing we did was to begin to buy PCs [personal computers] and to network DEC computers, and to provide computer literacy for the lab as a whole. This approach immediately gave our scientists enormous improvements in productivity. The second was to bring in more analytical technology so that you could analyze what you had and not have to do so much empirical work. Because if I can do the analysis properly and not have to do six pilot plant runs to find that out, the savings are enormous.

What we did was to think about how you could, in research, do your work in such a way that you cut down both on time and pilot plant facilities. I think that was probably the basis of the *R&D Magazine* award. We had a lot of fun doing those new things and today UOP is probably as good as anybody at creating efficiencies and taking time out of processes. A lot of the things that we started then, they've continued, and in fact moved beyond where we were. It was an exciting place. In fact, of all the jobs that I have ever had, I enjoyed being VP and director of research at UOP better than any of the rest.

TRAYNHAM: Company mergers changed the character of your appointment and your locale.

GOOD: Lord, yes.

TRAYNHAM: Tell me a little bit about that.

GOOD: You know, the 1980s were—in fact, it looks like the 1990s are going to now be the same, or at least the back end of the 1990s are going to be the same—really a time of all kinds of reorganization, acquisitions, and spin-offs. When I went to UOP, UOP was a stand-alone company. It was one of the Signal companies, true enough, but Signal Companies managed the four companies they had as a portfolio, and each company would behave independently. Signal owned UOP; Ampex [Corporation], who made magnetic audio/video tapes; and they owned Mack Trucks [Inc.]. They also owned the Garrett Aerospace Company, which made small jet engines, and they owned 49 percent of the Los Angeles Angels [now the Anaheim Angels]. [Orvon] Gene Autry owned the 51 percent. In fact, UOP had a board of its own, which managed it separately.

In the early 1980s, business theory taught that you couldn't get any synergism between business units without a much more hands-on operating, headquarters kind of group that managed centrally. The old Signal Company headquarters in La Jolla, California, had, when I first went there, seventy-five people. It was about a four billion-dollar company in those days because each of these single companies was about a billion dollars apiece. In 1992 and 1993, they merged with a company called Wheelerbrater-Fry, whose CEO was Mike [Michael] Dingman and their principle financial officer was Paul Montrone. They were both experts at coming into ailing companies, shaping them up, sort of ruthlessly doing that, and then getting them ready to compete in moving towards a global economy.

So they merged with the Signal Companies. The Signal Company's executives' motivation for that was to get the kind of management skills that were required to manage the company as a whole rather than just have it managed at each business level. The transition was interesting, to say the least, because they did not know anything about UOP at all. They were people who had managed manufacturing companies, primarily. For example, in their portfolio, they had Fry Copy, which was an old company in Des Moines, Iowa, that started off in life making carbon paper from carbon black. At the time of the merger, they still made carbon paper, but they also made airline tickets, typewriter ribbons, and computer ribbons. The reason it was called Wheelerbrater was that there was a company down in South Bend, Indiana, called Wheelerbrater that made a big cleaning device they called a Wheelerbrater. It had a big rotational brush on the bottom and you used it to clean floors.

So they manufactured that. Here you had a group of people who were used to taking over companies that were old-line manufacturers. Taking out staff. Shaping them up. Redoing

the management. Parts of UOP were like that, too, because UOP had made a lot of money in the past and they acquired some companies they had no business having. I mean, UOP had a company that, believe it or not, made seats for farm tractors. We had one that made things for airplanes—the carts that go up and down the aisles and other similar things. But the part of the company that actually made money was the UOP Process Division, which was the one that sponsored the really fundamental research, had very high quality development, and licensed their technology. The only manufacturing that we did was to make catalysts, and the cost of the manufacturing was not truly an issue. We never priced them by cost, anyway. We priced them by what the market would bear, which had nothing to do with their cost, actually, because their market value was the intellectual property they represented, not the cost of the materials that went into them. So you have these folks who come and look at UOP and they try to do what they had done in other types of companies.

Well, this was a disaster. I mean, you just can't imagine. Fortunately, Paul Montrone, in particular, recognized that this was a different kind of an operation. Mike Dingman was a guy who was really interesting. He actually liked the laboratory and had a real interest in it. So the UOP Process Division was able to continue its culture and approach to research and the market.

[END OF TAPE, SIDE 3]

GOOD: The Signal Companies didn't have much in the way of a corporate laboratory. So we designed a plan where we could provide research services for the rest of the company. The Garrett Aerospace division had superb engineers in the Signal part of the company, but the Wheelerbrater companies were not research-oriented. So we designed a program and went to Dingman and said, "Look, this laboratory can do not only work for the UOP Process Division, but we can do things for your other divisions that will make you more competitive in those businesses, as well, without you having to build laboratory facilities in each and everyone of those other divisions." Well, we sold that and they changed the name to the Signal Research Center. At that point, we were able to branch out. To be honest, that branching out was helpful, not only to those other companies, but frankly to UOP as well, because it opened up for them some new problems in material science. New ideas came that probably they would not have looked at had we not been involved in the other company activities.

So the merger worked out and we were back doing reasonably well in 1985, when everything changed because we were essentially bought out by the Allied Corporation. Ed [Edward] Hennessey, who was the CEO at the Allied Corporation, was very much interested in the Garrett part of the company. He was the person who had provided the "white knight" in the Martin Marietta [Corporation]/Bendix [Corporation] fight and had bought out Bendix. Allied owned the Bendix automotive sector and the Bendix aerospace sector. Garrett had both an engine division and an equipment division that made air-conditioning units, avionics, and other components for airplanes. The combination of Bendix Aerospace plus Garrett Aerospace really

made a very nice match and made a very good aerospace company. So they merged—that was 1985 when the merger happened. I will always remember that because a few of us were in China when that merger happened. We were in the process of talking to the Chinese about increasing our presence in their petrochemical and refining development programs and in selling them aerospace components when the announcement was made and all of us had to come home very quickly.

Then after the merger, it turned out that we had some people from the chemical sectors in the old Allied Chemical Company who really understood the value of the UOP laboratory and the pilot plants. So the Engineered Materials Sector, which was the old chemical sector of Allied, was grouped with UOP. We became part of the Engineered Materials Sector and so the laboratory at UOP became the laboratory for that sector of AlliedSignal. They already had a small corporate laboratory in Morristown, New Jersey, at the Allied Corporation's headquarters, which became the headquarters for AlliedSignal. So the laboratory then began to serve not only UOP, but several of those chemical companies, as well—the fibers division, the plastic division, and others. That worked very well. We had some things going that, really, I thought, worked out very well. It was beneficial, again, to both sides because UOP was able to see opportunities that they'd not seen before, and clearly we were a help, particularly in the pilot plant area, for the chemical businesses.

Then in 1988, Alan Belzer, who had been the president of the Engineered Materials Sector at AlliedSignal, and to whom I reported, became the president of AlliedSignal, Inc. At that time, two things happened. One was that they made the decision to create a joint venture between UOP and the Molecular Sieve Division of Union Carbide [Corporation]. We had tried back in the early 1980s to get the Molecular Sieve Division from Carbide because one of the things that UOP had not done—they had missed the window on zeolite development for catalysts. They still used them, but they missed the window to really be a big player in that. They tried to buy the Molecular Sieve Division in the early 1980s. Carbide wouldn't sell. We tried again after the AlliedSignal merger, and they still wouldn't sell. But in 1988, when Carbide had some big problems, they came back and said, "Look, we won't sell, but we would consider a joint venture." So they put UOP and the Molecular Sieve Division together in a joint venture which is now UOP. The UOP of today is the joint venture between Carbide and AlliedSignal. It's a 50/50 joint venture—probably one of the most successful of those that I know. Several joint ventures have been done around the industry. Some have worked, most have not. This one worked very well because it was really appropriate. I mean, they do have major synergisms because UOP has all the knowledge about how to apply their zeolites and the Molecular Sieve Division of Carbide was probably a very big player in new zeolitic materials.

So the joint venture was created and Alan Belzer became president of AlliedSignal, Inc. He wanted me to come to Morristown to be the senior vice president for technology for the company as a whole and manage the corporate laboratories rather than just the UOP ones. To accommodate, we split the laboratories in Des Plaines and gave a big chunk of them to the joint venture, and we kept a small segment of AlliedSignal people there who were working on the

other AlliedSignal projects.

We had laboratories in Morristown, which had been the corporate labs for the old Allied Chemical Company, plus Allied had kept a laboratory in Buffalo, New York. That had been part of the corporate laboratories, as well. We put these pieces together to form the Corporate Research Labs of AlliedSignal.

TRAYNHAM: This merger-changing identity of the parent company—was there any disruption of the scientific personnel?

GOOD: Clearly, there's always some. For example, after the Wheelerbrater merger, we were forced to reduce staff and we managed to do that in ways that did not hurt the long-term viability, but that's always tough to do. What we decided to do was that since we had to do it, we would truly take out the lowest performers and the people who were at the fringes. By doing that, it helped the morale a lot because some people had not been pulling their weight all that much anyway, and so people who had been working hard kind of appreciated your noticing who was doing what. So we managed to survive that okay.

Then after the AlliedSignal merger, because of the fact that we could pool our resources with the old Allied Chemical group, we could make that work. By the way, after that reduction in staff, we built up fairly quickly, as it turned out. After the AlliedSignal merger, we had to regroup, if you will. But that wasn't so bad because we had things to offer that the Allied Chemical people really wanted. That was not such a disruption. In fact, we managed that one, I thought, in a pretty decent way. With the creation of the UOP joint venture, personnel issues were really pretty traumatic because this meant dividing up some people who had worked for UOP forever—some stayed with AlliedSignal and some went with the joint venture. But I think we handled that in a way that most people didn't feel too badly. But you always had some people who were concerned, and there's always some trauma associated with actions like that.

TRAYNHAM: I believe I read that one of the achievements at UOP during your time there was in very successful anti-fouling agents for ocean-going vessels?

GOOD: No. That was some of my old LSU research. We never translated that work into work at AlliedSignal.

TRAYNHAM: All right.

GOOD: We looked at it at one time, but we decided because of all the environmental issues that were associated with it, it probably was not something we should be doing.

TRAYNHAM: I would like to back up a bit now, before we go on to your later and current employment opportunities, to take a look at your service to the profession and your roles in the American Chemical Society and the government.

GOOD: Yes. [laughter]

TRAYNHAM: Could you give me a synopsis view of that activity?

GOOD: Oh, gosh. Well, that's interesting. Actually, I was fairly active in the local section of ACS in Baton Rouge when I first came there because it was a way to meet people and I kind of enjoyed it, anyway. Then when I joined LSU in New Orleans, there was a fairly active chapter in New Orleans. Again, as I said, at that time there was a really vital academic chemical group there, and some companies participated, as well. The old Freeport Sulfur Company and Shell Oil had a number of chemists there in those days, so there was industry activity as well as university activity. The New Orleans section of the ACS was a pretty vital group of folks.

So the interaction with the ACS section went very well and I just enjoyed it for lots of reasons. I always thought it was good to get your students involved if you can, because they need to get some professionalism as well as some technical training. We used the section for student programs and all kinds of things. I had been appointed to a couple of national committees and, I guess, the first real attention I got in ACS was in 1970 when I was chairman of the Meetings and Expositions Committee. I don't even remember how I happened to get appointed. However, the Society met in Miami Beach. I don't remember whether you went to that meeting or not, but that was one of the worst meetings the ACS ever put on. Housing was atrocious. I was active in the Inorganic Division and I was asked to go and make a presentation to the Meetings and Expositions Committee about how badly the arrangements had been handled. We had people that didn't get their rooms—it was just a disaster. So we made a major presentation to the committee and apparently that came to the attention of several governance people. [laughter] So what happened was that I got appointed to the committee. It's what usually happens—you should keep your mouth shut. But anyway, then in 1972, Hap Fisher, who was the director of the Regional Agriculture Laboratory in New Orleans, was running for the ACS Board of Directors as a regional director. In those days, the Southern Regional Research Facility was a really good lab and they contributed in a big way to the New Orleans section. We had put a lot of effort into getting Hap nominated for that seat on the board.

So the Nominations and Elections Committee called me up and wanted me to run for the

board and I said, "I can't do that because I've actually worked to get Hap on the ballot." They called back and said, "No, we really would like you to run because we'd like to have two good candidates on the ballot." I said, "Well, I'll think about it." So I went over the see Hap Fisher and I said, "Hap, what should I do? We worked very hard to get you on this ballot." He said, "Well, you don't have any choice but to run." I said, "What do you mean?" He said, "Well, they have never asked a woman to run before. You don't really have a choice." I said, "Well, if you think that way, I'll do it. But I'm not going to do anything. I'm not going to campaign. I'm not going to do anything." He said, "Fine, neither will I." So to this day, I don't know how I got elected. I think part of it was that I had been the program chairman for the last joint southeast/southwest meeting in 1970, and so a lot of people in the region knew me from that activity. I suspect it was because of that. That was my introduction to ACS governance and I was elected to the board in 1972. I was put on the Publications Committee and Bryce [Low] Crawford [Jr.] from the University of Minnesota was chairman. I got to know him very, very well through the years. He's just a wonderful person.

So I stayed on the board for, I guess, three terms and was elected chairman of the board in 1978. That was back in the days of all of that nonsense from the "grass-roots group", if you remember. We had several people trying to dismantle the ACS because they thought the governance was unfair and undemocratic. There were major political battles in the board itself on these things, but I was re-elected chairman in 1980.

Then when I moved to Chicago, in 1980, I was asked to run for the ACS presidency, but that was the year that I was appointed to the National Science Board. I was going to this new industrial job and I just couldn't see that I could do both the ACS presidency and the National Science Board. So I withdrew from the ACS presidential race. That was the year that Bob [Robert Walter] Parry won and everybody thought that I withdrew because of the write-in candidate, so it would leave Bob a clear path to win. But that really was not the case. I really did withdraw because I couldn't see how I could do both that and be a new member of the National Science Board. I did agree to run in 1986 for president and I won. Had I known that I was going to be promoted and reassigned to Morristown as the senior vice president for technology for AlliedSignal, no way would I have done that because it was really more than anyone should have taken on.

TRAYNHAM: But you seemed to manage to do both?

GOOD: Well, yes, I did. But like I said, had I known that was going to happen I would not have stood for election, but you're stuck. I mean, you're already there. You have to do the best you can. The AlliedSignal people were very supportive, I must admit, as well. So I had help and it wasn't all that difficult.

TRAYNHAM: Like a lot of the presidents of the American Chemical Society, you had experience being chairman of the board, as well as president of the society.

GOOD: Right.

TRAYNHAM: Did you find those executive positions satisfying?

GOOD: Oh, yes. In fact, I really enjoyed being chairman of the board. Actually, being chairman of the board of the American Chemical Society probably was another big help when I was interviewed by the president of UOP. I went for the interview and he said to me, "Have you managed a budget?" I said, "Well, yes. I managed my own research budget and I have about fourteen or fifteen people. In addition, I am chairman of the board of directors of the American Chemical Society, the management group for the society. We manage a budget of about a hundred and sixty million dollars a year." I think that probably did make a difference. I suspect that reassured him that I really could look at the budget and the money and decide how it should be spent and how it should be managed.

TRAYNHAM: You mentioned about innovations, particularly with computer access, that you made when you went to UOP. Do you recall any particular initiatives you made while you were in ACS governance?

GOOD: Yes, there was one in particular. Bryce Crawford and I, together, I think, actually caused *Chemical Abstracts* to move towards computerized publications—I mean, the whole issue of moving to computer databases and on-line delivery. We finally set a target for them that they should get 50 percent of their income from that sort of activity in a certain period of time. He and I had, from one time or another, both been chairman of the Publications Committee, where CAS [Chemical Abstracts Service], in those days, still reported. It was clear that if CAS did not move in that direction, they were not going to survive. My guess is that of all of the changes—I mean, the real initiatives and what I consider to be changes that made a difference—I think the modern CAS is the most important. I'm not saying that we did all of it, by any stretch of the imagination.

TRAYNHAM: Set it into motion.

GOOD: Exactly. Right, and the other one that I helped them do was their interaction in the international environment. We actually put together a joint agreement with the British, a joint

agreement with the Germans, and, ultimately, a joint agreement with the Japanese in publications and Chemical Abstracts Service. Those were initiatives that we put into place. Again, CAS would not be where it is today, had it not done those sorts of joint initiatives.

TRAYNHAM: Now it is, in effect, almost an autonomous operation, in terms of separation from reporting to the Publications Committee and the board there?

GOOD: Right. Well, actually, they've gone now even further than that. The new Outside Governance Board now includes publications as well as CAS. In other words, they put publications and CAS back together again, which I find fascinating. But they have essentially removed the control of them, certainly the business control, to this outside autonomous group. The board does not have that much clout in these areas today.

TRAYNHAM: Had that type of organization been in place when you were in governance structure, it would have been improbable that you and Bryce Crawford would have made the initiative and the goal setting that you did. Do you see that as an impairment to the future of CAS?

GOOD: That's true, except if you had had a board like that in place, they probably would have seen it, as well, because truly it was a business issue. My guess is that they would have seen the need long before the academicians that sat on the Publications Committee would have seen it. I think probably it would not have mattered. I think it would have gotten moved because of the business issues. Now, the real problem today is that CAS in some ways, in my view—and the publications program too—is at the same sort of crossroads they were back in the early 1980s. In those days, the whole issue was you needed to get the process computerized.

Today the issue that's on the table is the role that ACS and other scientific organizations have in on-line publications. I think there are two major issues, and organizations like the American Chemical Society should focus on them. One is that in this age of being able to put up anything you want on the Internet, people are putting up their pre-prints and all kinds of material. The question is, what does it mean to get a paper published in a journal? How long will you be able to get people to publish in journals? My guess is that they wouldn't publish there today, except for the fact that universities only recognize peer-reviewed journal articles in support of academic promotion. But at some time that will crack, as well. There will be no driver, actually, and so the question is, what will the scientific literature look like in the next ten years? The two big questions that affect the American Chemical Society, both from the journal side and CAS side are: number one, how do you access quality? How do you decide whether a paper is worthy of archiving or not? The second is, how do you archive it and who does it? I think those are the questions that nobody knows the answers to yet. No one has put appropriate

thought and effort into these issues yet.

TRAYNHAM: Also, there's an issue of what reliability to ascribe to the paper, if it has not been reviewed by experts in the field.

GOOD: That's exactly correct. For example, should you bother to archive it if it has not been reviewed? How do you decide whether this is a paper worthy of an archival spot or not? It is essential that you work that out because you sure don't want people re-inventing the wheel, either. But I think this is an area that's not even being addressed in what I consider to be a holistic way today. Within ten years, it will have to be addressed because the Internet is here. It's not going to go away and the rapidity at which you can disseminate information is so great, that to not have the ability to both do the quality control and decide on how to archive material is a major problem. Frankly, I don't see as much work being done here as we need.

TRAYNHAM: Peer review does not guarantee that the paper is reliable, but it's the only way we have.

GOOD: No. Well, between peer review and the editor's oversight, you certainly have a quality that you don't have if you just dump it on the Internet when you want. There's just nobody, and I think the ACS oversight board that has the oversight responsibility for ACS journals and for CAS is the right vehicle to begin to worry about these issues, because the ACS is one of the biggest scientific publishers. The other question is, without hard copies, how do we get paid for publication costs? The questions on the table with these two issues are: who's going to pay for that quality control? Who's going to pay to do the archives? That's not clear at the moment because even university folks are beginning to be somewhat upset by not being paid to review. You know, you keep hearing rumblings in the background about that. People are aggravated that the journals get all this free reviewing and then they charge more for the journal than they think they should pay. There's always a bit of rumbling out there. So I don't know how that's going to come out. I don't think people have really addressed it yet.

TRAYNHAM: Tell me something about your work on the National Science Board and your association with the National Science Foundation [NSF].

GOOD: Well, I was, as you know, appointed to the National Science Board in 1980 by President Jimmy [James E.] Carter. I suspect that a lot of the legwork to get me appointed was really done by Joe Reynolds of the LSU physics department. You know, Joe had been on the National Science Board and his term was essentially over. I'm sure that he and some of the

people at LSU ran all of the traps—political traps—to get that appointment made. So I came on the board in 1980 and it was an interesting time because that was the days of the Golden Fleece Award. It turned out that at one of the very first meetings that I attended—[Burton W.] Adkinson was the director at NSF—we were discussing this whole business about Golden Fleece Awards. Finally we tried to explain to him that what he needed to do was to clean up the titles of research projects so that they represented the science but did not provide "silly" project titles that could be distorted. So that was my introduction to the National Science Board. I very much enjoyed working with the National Science Board.

I think the National Science Foundation is certainly one of the best government agencies, even today, even though it's getting a little old, and has some of the problems of aged government institutions. But it still does a very good job. Its overhead is about 5 percent. In other words, for every dollar it gets, about ninety-five cents goes outside. That's not bad compared to other agencies that we could talk about—in fact, that's excellent. I think that over time, it has really done what it should have done. That's not to say that every program has been the right one or any of those things. It never is. They've always managed to come up with somebody to truly provide some oomph for it at the right time. Erich Bloch really sort of re-vitalized the agency. He really moved it forward and he got them to do some new things. He led the agency to create the engineering centers and the science centers, although the academic community, for the most part, wasn't in favor of these programs because they wanted only peer-reviewed, single-grantee awards. But what they didn't realize was that because of the politics, he could sell those centers and get more money for single applicant awards at the same time. In other words, he used the centers as levers to create excitement in Congress to get the entire budget of NSF improved. I think without them, you wouldn't have seen the increases that you've seen in the last few years.

TRAYNHAM: How long were you on the National Science Board?

GOOD: Until 1991. I was re-appointed in 1986 by President [Ronald Wilson] Reagan. That appointment was an interesting one. Having been appointed by Carter, you wouldn't expect to get re-appointed by Reagan. But some of my Republican friends weighed in and Erich, I think, made a big pitch, as well.

[END OF TAPE, SIDE 4]

GOOD: The NSF also has another thing going for it. It has always had a rather good staff. The policy of bringing in rotators has really served the Foundation very, very well. They don't do as many rotators today as they used to and I think that's a mistake. I really like the idea of rotators. I mean, you don't want everybody to be a rotator. I'm not suggesting that, but to have a fairly

decent number of rotators in all of the research programs that come and stay one or two years, or three years at most. I think that's really a very, very good mechanism because it brings new blood in and it brings new ideas in and so on.

TRAYNHAM: It's educational for the rotators.

GOOD: Is it ever! I mean, they take back as much information as they bring. The problem is that it's hard to get some of the best scientists to do it because they see it as a break in their career. They see it as time out of their creative juices, which is kind of a shame, in a way. You really would like to see some of the very best people participate. It's always been difficult to get some of the very best people to even take the assistant directorships or the division chairs. But on balance, I think the NSF has been able to get rather good people. Certainly the 1980s was an interesting time for research and development. Through that period, support for R&D was pretty bipartisan in nature. There wasn't much quarrelling. There was just a question of how much people thought they could afford. Then in 1991, President [George M.] Bush offered me a position on PCAST, which is the President's Council of Advisors for Science and Technology. Alan Bromley was the director of the Office of Science and Technology Policy and chair of PCAST. Remember, he was the science advisor to Bush. I accepted that appointment and I resigned from the Foundation board a year before my term was over.

PCAST was kind of fun, meeting in the old Executive Office Building with Alan Bromley, who is a very energetic guy anyway. We actually did get a few things done, I thought. For the first time ever, the President put out a document that actually said "technology policy" on it. They'd never been able to get a policy statement of any kind out before. That was the time, also, that a couple of new programs got started. Bush pushed the Advanced Technology Program and Bromley certainly did. It started, you remember, with only about ten million dollars, as a way to begin to connect the country's research base to the technology base. I still think those are extraordinarily good programs.

TRAYNHAM: All the while you were serving on the National Science Board, and now in this new office, you were still research director at AlliedSignal?

GOOD: That's right. [laughter]

TRAYNHAM: How much time out of your paid employment did these public positions take?

GOOD: They take more time than you have! [laughter] But you just do the best you can.

Those positions in the industry today, at that level, simply take however much time you have. I mean, there is no such thing as a normal workday. It just doesn't happen and so you learn how to manage your commitments. There are times when there are conflicts and you have to resolve it in favor of the company. It turned out that being chairman of the NSB [National Science Board] wasn't such a bad deal, and there were two reasons for it. One was that we had an extraordinarily good person who was the staff director for the board at NSF and if you've got good staff, you can manage these things. It really is that simple. If you've got poor staff, it's a disaster. You cannot do everything yourself. But we had good staff.

Secondly, the company was very gracious to provide me with support. In other words, I could use a couple of my administrative people to help. What the country doesn't understand is that we've gotten ourselves now into these conflict of interest positions where it's very hard for people who work in the industry to be in these positions today. That is a real shame because the input back and forth is excellent. Not only that, but AlliedSignal didn't get much out of me being chairman of the National Science Board. You know what I'm saying. On the other hand, the company viewed it as public service and were pleased with the visibility. That meant something to them, and they were perfectly willing to give me support, which turns out to be quite a contribution. So by having good staff and support, you can manage the demands from both constituents. On occasion, I would get NSF folks to come up to Morristown—it's not that far—rather than me making the trek down there. So you can make it work, but it's not easy to do. A lot of people really don't want to do it because it is a burden. I mean, the company, on the other hand, gives you little slack on your job. They think the outside activity is wonderful—fine, carry on, so long as it doesn't interfere with your responsibilities to the business.

TRAYNHAM: Just do it instead of sleeping. [laughter]

GOOD: Yes, right, so long as it doesn't interfere with what I need. [laughter] It's doable. But it's tough to do. I was fortunate because my husband is very supportive of these sorts of nutty ideas and that's a big help as well. If that were not the case, I don't think it would be possible—to be honest.

TRAYNHAM: Then you were persuaded to really shift into the public sector of the government.

GOOD: Yes. That is true. When I was asked to become the Under Secretary For Technology in [Department of] Commerce, I must admit that would <u>not</u> have been my first choice for government service.

TRAYNHAM: What would have been your first choice, besides being President? [laughter]

GOOD: You know, you're much better off being science advisor or something like that. Anyway, I must say Ron [Ronald H.] Brown, the Secretary of Commerce, was very persuasive. But the real reason you do it is none of that. There were two reasons. One was that for the first time, President [William Jefferson] Clinton and his transition team had put together a technology plan, a science and technology plan. If you remember, it's the first time ever that that's been done, to my knowledge. I mean, presidential candidates in the past have said little things, but in terms of really looking at the science and technology base as being a part of what makes the country great was a new idea for politicians. This plan was really quite well done. The transition team did a very good job with it. When you think about it, the ending of the Cold War says—and as it has now been borne out—that the Defense Department support of R&D is going to continue to diminish. Indeed, our biggest problem in research support, at the moment, is that we've lost the fundamental kind of work that was supported by Defense and no one has picked that up. That's why you have this disparity now between NIH, which continues to get funded because there's good rapport in the Congress for it, at the same time you have reduced support for engineering and the physical sciences. A big chunk of that used to come out of the Department of Defense. You remember the people even at LSU, who had support from the Air Force and the Navy. That's all drying up.

When you think about that, somehow you've got to elevate the concept of civilian technology, and civilian science that underpins it, in such a way that you can get the civilian work funded in the same way that you used to get it funded under the Defense rubric. We haven't done that yet.

So the opportunity to be in a position to articulate those needs was a compelling factor. The other issue, really, was that I had been extraordinarily fortunate in my career and the things that I had been able to do. It was an opportunity to try to give some of that back. I know that sounds trite, but it was a motivating factor.

TRAYNHAM: No, it sounds more inspirational than trite. I was just wondering, what was the particular persuasive argument that Ron Brown used?

GOOD: Two things. One was that he fully agreed with the President's science and technology plan. Not only that, he believed that the whole commercial enterprise, and issues associated with commerce, depended on it. He believed that if the country wants to grow, it has to have the technology base and he understood that. The second piece that he used to get me was that I could pick my own people and he meant that. I was never forced to take a single political appointee I didn't want. I got to interview them. They sent me people that they wanted me to interview, which was fine, but I did not take anybody that I didn't want or who didn't fit what I

wanted to do.

TRAYNHAM: Did anyone from AlliedSignal move with you?

GOOD: No. The person that I would have moved, if I could have, would have been my secretary, but she lived in Morristown and was pretty close to retirement, so it wasn't feasible to do. Since then, a couple of AlliedSignal people have come. One of the guys that used to report to me is now running the Partnership for a New Generation of Vehicles (PNGV). He was very, very knowledgeable about our whole automotive division. So we needed him. In fact, I hired two AlliedSignal people to do that. The first one that I hired for PNGV had been in the Washington office of AlliedSignal. I wanted him to manage that program, because he knew how to work with the government. But other than that, no, we didn't.

TRAYNHAM: In your role as Under Secretary for Technology in the Department of Commerce, you were called on to make a number of significant speeches or addresses.

GOOD: I'd thought you'd like to see that. [laughter]

TRAYNHAM: Whose titles are impressive. Can you tell me something about some of those?

GOOD: Well, it turns out that if you look at them, the content of the pieces are pretty much the same, in the sense that what we were trying to argue was that, first of all, the country needs a science and technology policy that it can articulate so that the public can understand it and be willing to make it a priority to pay for it. The second was to generate some understanding of what the federal support of R&D in the past has meant to the country. We put together two or three documents on that that I'm really quite proud of. One was *Technology in the National Interest,* (3) which really outlines what government support has meant. For example, things like the Internet wouldn't exist today without the support that came out of Defense and NSF. Semiconductors, all of that was started with government money. Things that we take for granted were really nurtured with government money early on and clearly the biotechnology revolution in the United States is totally dependent upon the NIH budget. It wouldn't be here had we not spent that kind of money.

What we tried to do in that office and the Office of Technology Policy was to get people to understand the fact that technology today, and by definition the science that supports it, really is the basis on which you're going to compete in the next century. Those countries that have it will compete. Those that don't won't. It's really not very complex. Secondly, that support for

R&D for civilian technology is as serious as the support for R&D for Defense technology was in 1960. Thirdly, that you've got to be willing to spread it across a fairly broad range of disciplines. I can't predict for you where the next major commercial breakthrough is likely to come. I need to have a portfolio of things. Now, I'm not opposed to making some plan that says these are the areas in which we really need to focus. But I don't want that focus down to three, which I think is very detrimental.

Our program was going very well. We were really making quite good progress. The Presidential Science Advisor, John Gibbons, set up a new National Science and Technology Council, which brought in all of the major players: me from Commerce, the Under Secretary from Defense, the Under Secretary from the Department of Energy, major players from EPA [Environmental Protection Agency] and NIH. The intent was to begin to have the government people talk to each other; understand the overall government base, take out the duplicates, try not to duplicate each other's programs, and try to collaborate when you can. The collaborative programs, like the Partnership for A New Generation of Vehicles—which was so difficult to get up and running—are classic examples of what you can do if you're willing to take the time and effort to get these groups to play together, rather than everybody playing his own game.

We were, I thought, making some real progress. The 104th Congress that was elected in 1994, however, took major wind out of all of our sails, because R&D was not on the agenda for that group, particularly in the House [of Representatives]. As much as anything, in my view, it's because they didn't have any clue as to why it should be. The other issue was that they came in with this anti-government mindset that said that any interaction of the government with a company really should be prohibited and that any development of commercial technology ought to be done by the private sector and the government should not intervene. This is a very simplistic view of the world today. It just is not sustainable because they did not understand the connection. They didn't know how federal R&D feeds the economic machine and that without it, you can't make it work. If you're going to get the energies and the speed out of the system, you've got to have pretty decent connections between them.

The other thing was that in his technology plan, Clinton and the transition team recognized that the Department of Commerce really ought to become the advocate and the driver for civilian technology in the same way that Defense had been the driver for Defense technology. Two things happened. One was that programs like the Advanced Technology Program (ATP) became "Clinton programs," and therefore targets of that 104th Congress. Without thinking about what was in them, they became highly political because Congress saw them as "Clinton programs" even though ATP was started back in the Bush Administration. The person who had my job in the Bush Administration had pushed very hard to get it done, had pushed the legislation, had pushed the organizational changes that made it possible to do it, but that didn't seem to make any difference to them. As far as they were concerned, it was a "Clinton program" and they were against it. So you ended up with at least a year, almost two years, of very partisan politics over R&D spending. I think we managed to just keep going. Our approach was, don't talk about the politics, just keep talking about why you need the programs.

Just keep making the speeches. Go to the hearings, make the same speech—why you have to have the programs, why it's so important, and why you can't exist without it.

Actually, I think we prevailed; at least to some extent. We salvaged ATP. It's still there. It's about two hundred million dollars, where it ought to be half a billion. That's okay. It's still there and it's now a regular program, hopefully. We have begun to get some movement in the Senate, they truly began to look at R&D to see if they couldn't move the agenda. [Senator William] Frist, from Tennessee, [Senator Jeff] Bingaman from New Mexico, [Senator Joseph] Lieberman from Connecticut, and [Senator John] Rockefeller from West Virginia, in particular. [Senator Pete V.] Domenici from New Mexico even came into the mix. The House is still confused, if you will. Then we had Mr. [Robert] Walker from Pennsylvania to contend with. Mr. Walker had been a colleague of Newt Gingrich back when they were in the minority. He was one of Newt's "bomb throwers," creating problems to try to get a political position. He was absolutely opposed to anything that the government might do to support technology. I went in and spent about an hour with him. He told me once that it was his opinion that if you cut the capital gains tax, that he would guarantee that within a year, the industry would be building research laboratories on the university campuses and hiring university people to do research. I said, "Congressman, I came from that community. I can assure you, that's not going to happen. I don't care what you do. That's not the way it's done. It's not going to occur." He stood by his position and that was the way it was. Which, of course, is just devastating to the whole concept of how you build a science and technology policy.

But when he retired from the Congress, everybody in the Administration was relieved, to say the least. Congressman [F. James] Sensenbrenner [Jr.] from Wisconsin, who succeeded him as chair of the Science Committee, is a much different person, very rational, fiscally conservative, but very knowledgeable about R&D and why it needs to be a federal priority. The last year or so has been a whole lot less political and so we were able to make some real progress. The question is whether or not it will result in any funding. I'm not at all sure yet, to be honest with you, whether it will or not. I'm very concerned about the 1999 federal R&D budget because the President put in some nice increases and all that, but many of them were dependent upon the cigarette tax bill and the politics of that, as you know, are pretty chaotic at the moment. How that's going to come out, I don't know. Congress seems <u>not</u> disposed to use the money for R&D. Then you have this absolute budget-breaking highway bill that is now there. The reason that's important is that there are caps on the discretionary spending and all of that excess in the highway bill has to be taken out of what could have gone to R&D under those caps. I think the caps will stay and so there's a real possibility that in 1999 you will get some increase for NIH, possibly a flat NSF budget and decreases everywhere else, which I think will be a disaster. But it's possible that will happen, because the priority level for R&D is too low.

We finally, I think, are getting the industry to begin to move to help and that's going to have to occur; otherwise, it's not going to work. The Council on Competitiveness is moving the agenda very well. I was up there for their summit meeting last week and the report that is coming out of that conference looks very good. You've got major CEOs in the industry now

who are willing to make the argument too. The issue was to try to reach as many groups as you could and to try to get the message as widely distributed as you could.

TRAYNHAM: Well, you had appointments in Washington, of one sort or another, by four presidents in sequence.

GOOD: Yes.

TRAYNHAM: Two Republicans book-ended by two Democrats.

GOOD: That's right.

TRAYNHAM: Would you care to comment on the prevailing style differences among these presidents or your experiences with them?

GOOD: The thing that I know most about is their approach to research, R&D and science, and technology policy. When Reagan first came in as president, we had a very difficult time because he brought some ideas with him that said that NSF, for example, should not fund social sciences. You know, part of the social conservatives' agenda. The problem that you've got today is that you're fighting, on the one side, fiscal conservatism, which affects both the Republicans and the Democrats. Then you've got the social conservatism that's coming in from the right side of the Republicans and you've also got some strange philosophy, frankly, from the left wing side of the Democrats about what you shouldn't do in things like the ethical issues of environmental policy.

You put all that together and it's been kind of a difficult time. When Reagan first came in, it was a very difficult time for NSF, because he just cut all of the budget for the social sciences. We were left with the problem of how to maintain the social-science databases. Because if I can't maintain the databases, I'm going to loose continuity and I can't restart social science research. So the NSF board took those databases and their support and moved them into odd places in the foundation. We managed to salvage them for a later time. Then, of course, after a year or so, we were able to go back and convince everybody that this was a dumb thing to do. You talk about computers today and the ubiquitousness of computer operations. You had nobody putting up any money to study the human-computer interface problems. We were able to restore much of that work. So you've got these discontinuities and hiccups that take place from time to time, depending upon the point of view of politicians in power.

The interaction with the Bush Administration was relatively positive, from my point of view. Clearly, Dr. Alan Bromley, the science advisor, really wanted to move the R&D agenda, and he moved it in very difficult times. I think he did very well under the circumstances. Then, of course, the Clinton Administration started off in the R&D area, you know, in really good shape and with a good understanding of what you needed to do. They've stuck to it pretty well. It's been tough to do that because it's been just a frontal attack since 1994. Now that has sort of ceased and everybody is now back to where yes, R&D is a good thing. That's fine, but now I need a priority for it and I don't have that yet. They all had their different styles and they all had different mindsets. In many ways, the people who were science advisors for them made a difference as to how these things went, as well. Because in no case did the president's people have strong science or engineering experience.

Oh, gosh, what's the guy's name who was president of the National Academy of Sciences, who was Carter's science advisor? Dr. Frank Press, he's a really hard-core R&D person—in particular, a research person with a long academic research career. Then Reagan brought in that guy from Los Alamos [National Laboratory], [George A.] Jay Keyworth [II], who was a very conservative Republican, and he was the one who really thought that social sciences should not be supported. Of course, Bromley, who was the science advisor for Bush, was a plus. I think the science advisors have had a lot of impact on how these things go with respect to R&D and the interaction with organizations like NSF and other R&D agencies.

TRAYNHAM: Now, a feature article in an Arkansas newspaper about you included what was identified as a self-portrait by you, in which, among other things, you identified your favorite U.S. President as [George] Washington.

GOOD: Yes.

TRAYNHAM: Then your fantasy dinner party included, among the four presidents that gave you appointments in Washington, only President Clinton? What was the reason for picking him for the dinner party?

GOOD: Oh, that's easy. He is one of the most interesting people and one of the most intellectually talented that I've ever met. He has an interest in a lot of things and can discuss issues at great depth. I mean, really philosophical kinds of issues at great depth, and he is very articulate. He is just fun to talk to. If you were to rank him, as time goes on, with respect to the quality of his presidency, who knows? But in terms of being able to engage in a challenging discussion on the kinds of topics you might be interested in, I think he's just almost unbeatable.

TRAYNHAM: Now you are out of government service, but have retired into a very active role with Venture Capital Investments. I would like for you to comment on what prompted you to retire from the government post, where you seemed to be exercising considerable impact. It surely was not because you were tired of working; you're here in a prominent office in Little Rock.

GOOD: Well, that's true, except that I probably was tired of working fifteen hours a day. But I had two reasons. One is that I promised my family I would stay one full term, and I did that. We had bought a retirement home here seven or eight years ago. You get to the point where it's time to do that. That essentially was what I thought, because another four years would have been pretty far down the road. We had done a lot of remodeling on the house here and so it was just time to do something else. That's hard to define exactly why not.

The other issue is that jobs that are as intense as those—and, well, I guess that's my own fault for doing them as intently as I do—I don't seem to have any capability of handling them any other way, which is probably a mistake. Four or five years is probably the optimum time, frankly.

[END OF TAPE, SIDE 5]

GOOD: I stayed until all of our budget issues had been handled by the appropriation committees for last year. That's a good time to bring in somebody new, because then they have time to get settled and you don't really have to start the next budget cycle and all that until the fall. Well, a new appointment hasn't materialized, unfortunately, and I've been kind of distressed by that, but the fellow who was my deputy, Gary [R.] Bachula, seems to be doing quite well.

However, it's different to have somebody there who really can exert the kind of influence you would like to see. I have been sort of disappointed that a new appointment has not been made. Actually, the real issue about your question, I think, is that four or five years of these intense jobs is enough—in fact, the job as the senior vice president for technology at AlliedSignal was a very intense time. We had a new CEO, [Lawrence A.] Bossidy, come from General Electric about the second or third year that I was there. He began to change the company and to really make demands on where we were going, and the upper management positions were really intense jobs. I think five years, six years, is probably adequate. You may use up your best ideas.

TRAYNHAM: What is the particular nature of your work here now?

GOOD: What we're in the process of doing here is to stimulate the creation of some technology-intensive companies, and I have no idea of how successful we're going to be. We're trying. But the reason I'm doing this is that I have an old friend, Bill [William] Bowen, who was the CEO of the First Commercial Bank, the bank building where this office is. He, during his tenure as the CEO of First Commercial, had worked with the state and the community to do a lot of community things. He's always been very active in trying to build the community and the State of Arkansas. He had organized the National Advisory Committee for the State of Arkansas, which the bank financed and nurtured. He asked me to be a member of that board back in the early 1980s and I've been a member ever since. It meets once or twice a year here in Little Rock. In fact, it began back in the days when Clinton was governor. What Bill has tried to do is to bring at least one really important topic for this group to discuss and to have a report and put that out for the state government to use every year. He has always wanted to start some venture capital activity here. There has been very little, and particularly not for start-ups and early-stage, technology-intensive companies.

The timing is pretty good right now. The University of Arkansas Medical School has about fifty or sixty million dollars in research money every year and they've had that for about seven or eight years. So they're beginning to build up a cadre of very good people. They have a few world-class activities, one in geriatrics and another at the Children's Hospital here, their pediatrics department. Arkansas' Children's Hospital is ranked quite high in the country. So they're beginning to throw off some research results that can be exploited. The University of Arkansas in Fayetteville has an incubator that's doing quite well; it has a number of companies in it and they're beginning to move some of them out as successful companies. We actually have a pretty aggressive software service industry here. Alltel is headquartered here. Acxiom is headquartered here. Several other related companies are here, so there's enough activity now to build on. Just within the last two years, part of the old Pine Bluff Arsenal has been designated as the National Center for Toxicological Research. The FDA [Food and Drug Administration] is moving all of its toxicological research base here. So you've now got a base on which to build.

Bill Bowen had always wanted to start a venture capital activity, so when it was clear that we were coming back last June, I said, "Look, I'll make you a deal. If you'll get the investors put together, I'll come and review the technologies and get the office up and running for you and we'll see what we can do." He managed to get about twenty-four investors and we're now looking for opportunities for things that we can do, start-ups and early stage companies that can be nurtured here in the state. It's an educational problem on both sides, because we don't really have the infrastructure that supports these little fledgling companies and we're trying to get the universities to help with that. We need the business school to help with business plans and the small business group here to work with technology intensive companies. We are trying to get that to work. So we are trying to get the infrastructure activity in place and at the same time we are trying to educate a group of investors whose own money has been made, for the most part, in banking, financial houses, and real estate development. We have a couple

of investors where that's not the case. I have the fellow who was very instrumental in starting Systematics, a software service company that was absorbed by Alltel. He really knows about technology and start-ups, and does some venture funding himself. Another member was a partner, and I guess still is a partner, in Goldman Sachs [Group, Inc.]. He has funded many ventures personally, and through Goldman Sachs has been involved in lots of start-ups in his career. But it's very hard to get people to understand that if you're going to do this sort of venture capital, you've got to have a portfolio of investments because there's no way to guarantee that any of your specific projects are going to work. The group has resources, but we must convince them that these technology intensive companies are good investments. We're working on it.

TRAYNHAM: Have you yet moved this particular agency into sponsorship of some new venture?

GOOD: We have two on the table at the moment, which for a first year is not bad. We are actively raising money for one of the potential companies, and I presently have about half of what I think is necessary to get it off the ground. I'm going to see if I can get some more from outside the group. The second is one about which I expect to send them a letter, perhaps today, outlining what I think they should do. Then we've got about two or three others that we're actively doing due diligence on. I think we're not doing badly, but, again, people who are not familiar with venture capital think that's very slow. But it's not really, because venture capitalists will look at a hundred deals and make maybe five.

TRAYNHAM: Is your role to make the pitch for the in-flow of capital?

GOOD: Well, no, really my job with this group—and I'm doing this on *pro bono* basis by the way, but anyway—is to look at the opportunities that I can either generate, or that come in over-the-counter, and also to look around to see what opportunities are out there. For example, talking to the universities to see if there's anything that they have and then deciding which of the opportunities look like they could be promising for us here in this state. The next step is working with the group to get a decent business plan together that my executive committee can look at. Then our executive committee decides whether or not we should pursue that particular opportunity or not. If they choose to do that, then we'll do a thorough due diligence on it, be sure we understand "where all the bodies are buried," what kind of outstanding loans they have, and whatever other outstanding issues may be important. If that all then comes together, and we can make a good enough deal with the people who own the technology or the company, then we will put together an offering for our members. Then the issue is to sell that offering to them.

TRAYNHAM: During the course of your long and varied career, your achievements have been recognized with several significant awards. Would you like to comment on that record of awards?

GOOD: Oh, I don't know. I really do appreciate them, and there are times when I don't understand why they give them to me, but certainly some of them are more significant than others. Clearly, the Boyd Professorship at LSU was a very significant one. It was early on, and had a lot of impact, which was, I guess, the reason why all of the nonsense came up over the politics in the university. Why that was so difficult a time for me was the fact that my colleagues challenged whether I should have gotten the distinguished title. One of the things that I've tried during my lifetime to do is to not take anything that I thought was offered just because I was a woman. The accusation that the chairmanship was offered to me because I would be the first woman was an underhanded attack, I thought. Anyway, certainly the Boyd Professorship was very appreciated by me.

My election to the National Academy of Engineering was really neat for a lot of reasons, primarily because of the Academy's leadership in technology issues in the industry. One of the others, which you'll find kind of strange, but I felt was wonderful, was that I was elected to the Swedish Academy of Engineering. I have found this organization just fascinating because I've been to a couple of their annual meetings and it was really fun. I guess the Priestley award from the ACS, I would say, is really one of those things that one remembers and appreciates because it is for chemists. That is a very prestigious award. I must admit that this award, the recent one, from the Chemical Heritage Foundation, was totally unexpected and very, very pleasant. In fact, I'm very pleased that they're doing that award and I hope that they keep that going.

TRAYNHAM: You're speaking of the Othmer Gold Medal.

GOOD: The Othmer Gold Medal.

TRAYNHAM: Which was awarded just last month to you.

GOOD: That's right.

TRAYNHAM: You have made reference two times during the course of our conversation to your husband, Bill, and his very supportive attitude of all of your activities. You have other family members?

GOOD: Yes, I sure do.

TRAYNHAM: Tell us something about your family.

GOOD: Well, I have two sons. One we talked about earlier, who was born when I was still a graduate student. The other one, whom I should have made reference to when we talked about moving to LSU in New Orleans, was born in September of 1958 when we moved to New Orleans. One of the things that happened because of his birth was that I missed the first two weeks of school and everybody had divided up the teaching loads. The best I can tell, they decided what they didn't want to teach and I ended up with it. [laughter] But anyway, that's neither here nor there. Both of them have turned out to be just wonderful people. I've been very pleased with them. The older one has a Ph.D. in marine biology on the botany side from LSU, and is presently, as you know, the director for the Wetlands Programs for Louisiana. He absolutely loves the job and hates the politics; which is not new or different. He has two sons, one of whom just graduated from high school last week, who is going to Rensselaer [Polytechnic Institute] in the fall. He is going to RPI to do aeronautical engineering. At least that's where he's going to start. My second grandson will be a junior at Catholic High in Baton Rouge next year.

My younger son is an architect who graduated from Tulane. In fact, it's kind of interesting, his wife is the daughter of Professor [William] Ward, who is one of the best-known geology professors at UNO. They spent two-and-a-half years in the Peace Corps in the Philippines after she graduated from Brown [University] and he graduated from Tulane. That served them very well. They had a great time and enjoyed it a great deal.

TRAYNHAM: Was he doing architecture-related work there?

GOOD: Yes, actually, they did everything. They were assigned to some little island down south of Cebu, called Siquijor, and they did everything. They built an icehouse. The fishermen had no way to keep their fish fresh to get to the mainland. He designed and helped them build an ice plant—all the way from the generators and stuff they bought from Germany, and the whole bit. She started a library and did all kinds of educational things for these people. They learned a lot, because in building this ice plant, he had to go to Manila to manipulate all of the legal and government issues involved with ordering the equipment and getting funding. So they really learned a lot in their Peace Corps experience. He and his wife have two sons. The oldest one is now thirteen. They live in Madison, Wisconsin. They moved to Madison when they got back from the Peace Corps because she wanted to go to graduate school is soil science. They were looking for a school that had that, and also a place where he could get a job as an architect, and

they found the combination in Madison. They like Madison a lot. It's a lovely community and it's been just great. The schools are good and the kids like it there. He spent about five or six years as an architect, four or five of those as the architect for the University of Wisconsin, where he learned everything. I mean, they build everything on those big campuses.

Then he and a friend put together a partnership. He had always wanted to do his own firm, which they did. They got started doing some disaster work after Hurricane Hugo. They were asked to go down to the Caribbean for FEMA [Federal Emergency Management Agency] and evaluate the buildings down there and give them some feedback on how they should rebuild. They did that, and that apparently went very well. Well, they began to get some contracts from disaster agencies and things like that. At this point in time, they essentially do no architecture, in the normal sense of the word, and they now run a major consulting firm that has about six or seven permanent employees and another five or six that they keep as consultants. They do disaster-relief work all over the world, including working on major refugee issues. They run workshops for non-governmental agencies and other government aid and disaster agencies. He, as a high school student in New Orleans, was in the academic high school and they taught Russian. He took four years of Russian, so he reads and writes fluent Russian. This gives them an edge on all of the disaster things that the State Department, USAID [United States Agency for International Development], and the United Nations do in the old Soviet Union, because the only Western language those people speak is Russian. He's kind of the State Department's expert on Outer Mongolia and does a lot of work in the "Stans," you know, Kazakhstan. In fact, one of the neatest contracts that they've gotten recently is that they have the contract from the International Red Cross to help those particular former Soviet Union Republics build a Red Cross structure, a National Red Cross Structure for each of them. In other words, Kazakhstan would have a National Kazakhstan Red Cross, which they've never had before. They have the contract to help them build and design the organizations for those countries, which is really fascinating. They do all sorts of things and they do go to some bad places.

TRAYNHAM: You have four grandsons, I believe?

GOOD: I do, yes.

TRAYNHAM: You referred, quite early on in your review of your career, to the absence of women scientist role models when you were a student, and Madame Curie was the one that lead you into radiochemistry. You are such a strong role model for women in science, it's kind of a pity that you don't have any granddaughters. [laughter]

GOOD: Well, that is true. I agree with that. My husband, I think, was even more disappointed about that than I was. [laughter] He would really have liked to have at least a granddaughter.

You know, you go with what you get. They're all healthy and quite intelligent and that's good enough. I'm real pleased with our sons. They've turned into really good people and they are good parents and that's about as good as you can do. God, in these days, that's better than most.

TRAYNHAM: They saw the role model and picked up on it.

GOOD: Who knows? But, yes, it is also true that my husband has always been very supportive, and, in fact, one of the reasons that the boys, perhaps, have done as well as they have is that he's always been very hands-on with them, as well. That's made a difference.

TRAYNHAM: Well, you've had this remarkably successful career in academia, industry, and government service. It is seldom that a chemist combines success in all those segments of the possibilities for chemists. As you look back on your career, is there a segment that you wish you could have had a longer stay in or did the division among the three segments about balance out the way you want it?

GOOD: Actually, I think it turns out pretty well, really. Like I say, I suspect the job that I enjoyed the most was being vice president of research at UOP. However, I was there for about eight years and you need to move on, frankly. I'm actually consulting for them these days. That's kind of fun.

TRAYNHAM: As you look back, when did you really make the transition from being an experimental chemist to being manager?

GOOD: Oh, gosh, you know, it's interesting about that. I think most chemists do that, whether they admit it or not. You do it slowly, certainly. By the time you end up with three or four research grants from three or four agencies, fifteen or sixteen people in your group, your days of doing hands-on stuff is pretty far gone. I think that's true of most senior faculty. They don't admit it to anybody, but I think if you go into their labs, you will find that's the case. They spend their time managing that portfolio. That's not to say that they don't put intellectual thought into the science, still, I don't mean that, but in terms of going into the laboratory, they don't do much of that. In fact, by the time they get to that level, their graduate students would be absolutely appalled if they went into the laboratory, frankly. [laughter] I think that's more common than not, actually, rather than the other way around. That's always been pretty much the case, except for the single inventors and folks like that. In some ways, that's appropriate, because if you learn enough and if you've done it well, you learn enough over a period of time

that your time is better spent in teaching young people how to do it and providing the input that your experience allows. When you really come down to it, your success in all this stuff depends on how good your graduate students are.

TRAYNHAM: Is there anything you think of that needs to be included now to make the story complete? You've been very generous with your time on this interview.

GOOD: Well, only a couple things, I guess, that didn't get mentioned. One is that I did have a role model, as you very well know, when I went to LSU. Virginia [Rice] Williams was there, which was a big help. People don't realize that, but Virginia was there, and she had been there all by herself for a long time and had a lot of good wisdom to share, which was very helpful. Secondly, the other thing that made it work was that we were able to hire help in those days. We actually hired a housekeeper when I first went to LSU because Billy was only nine months old at that time. The ability to have somebody who you trusted taking care of your children makes a huge difference. In fact, I'm convinced the biggest problem that professional women have today is quality childcare. It's hard to find.

On the other hand, and I say to some of the young professionals who moan about it, "Well, you know, it's just too expensive and all that." I say, "It's not too expensive." Because we paid our housekeeper more than Bill made. [laughter] You know, that's what you have to do. I don't think people should bitch about that. That's the choice you make. The people that I feel sorry for, with respect to childcare, are those who are at the bottom of the economic scale and really can't afford very much because they have to eat off of what they make. Two-career families, in my view, can afford quality childcare and ought to pay for it. The only way we're going to get good workers in the domestic field is to pay a decent salary to people. But that's true, I did have a housekeeper who worked for us for quite a long time—a wonderful woman who was very, very helpful. I think, as it turned out, it was helpful to her. I think it was a win/win situation all around.

I guess the other comment that I would make is that my siblings have all done extraordinarily well. My sister, who lives here in Little Rock, is the medical director of Children's Hospital, the hospital that I told you is now in the top five in the country. She came here when it was not the top five in the country by any stretch of the imagination. She has been president of the American Academy of Pediatrics and is probably one of the most widely respected pediatricians in the country.

My second sister has a Ph.D. in nutrition. Presently she is director of the Delta Project that has south Arkansas, Louisiana, and Mississippi looking at the nutritional issues in the Delta Region for the Department of Agriculture. My brother, who is the youngest, just retired as the senior supervisor for the Western Forest, the U.S. Forest Service. I think that our success is because of our parents. My mother, in particular, was one of those memorable people. She

worked all of her life and taught school all of her life. A very, very bright lady, brighter than any of us, actually. She really instilled in us the idea that you can do whatever you want to do. I never understood how valuable this concept was. It never occurred to me to even think about whether I could shift to chemistry or not. It was exciting, and it sounded like fun, so you just did it. When I got through and wanted to go to graduate school, it never occurred to me whether women did this or not. People don't believe me, but I'm absolutely serious. I never thought about it. I think that was an unusual gift. Talking to lots of women who have not had that experience and had some real tough times making it, I think that's perhaps more important in my success that people would believe.

TRAYNHAM: Thank you very much, Mary, for being so generous with your time. Your interview will certainly be valuable.

GOOD: My pleasure. It's kind of pleasant when it's an old friend, you know. That makes it easier. [laughter]

TRAYNHAM: Good.

[END OF TAPE, SIDE 6]

[END OF INTERVIEW]

NOTES

1. C. A. Clausen, III, R. A. Prados and M. L.Good, “A Mossbauer Study of the Bonding in Ruthenium(II) Compounds,” *Journal of the American Chemical Society* 92 (1970): 7482.

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3. Mary Good. *Technology in the National Interest*, published by the Office of Science and Technology Policy, the White House (1996) by the Government Printing Office.

INDEX

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