CHEMICAL HERITAGE FOUNDATION

PAUL M. COOK

Transcript of an Interview
Conducted by

James J. Bohning

at

San Carlos, California

on

2 April 1992

(With Subsequent Corrections and Additions)

Paul Cook

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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PAUL M. COOK

1924	Born in Ridgewood, New Jersey on 25 April

Education

1947	B.S., chemical engineering, Massachusetts Institute of Technology

Professional Experience

1947-1948	President, Warren Wire Company
	Stanford Research Institute
1949-1952	Chemical engineer
1952-1954	Head, Radiation Chemistry Laboratory
1953-1956	President, Sequoia Process Corporation
1957-1991	Founder, CEO, and Chairman of the Board, Raychem Corporation
	Cell Net Data Systems
1991-1995	CEO
1991-present	Chairman of the Board
1994-present	Chairman of the Board, SRI International
1995-present	Chairman of the Board, Sarnoff Corporation
1995-present	Founder, Chairman of the Board, and CEO, Diva Systems Corporation

Honors

1985	Member, National Academy of Engineering
1986	Winthrop Sears Medal, Chemical Industry Association
1988	National Medal of Technology
1989	Golden Omega Award, Electrical/Electronics Insulation Award
1990	Member, American Academy of Sciences

ABSTRACT

Paul Cook begins the interview with a discussion of his family background and childhood. When Cook was young, he took an interest in chemistry, developing a laboratory in the basement of his parents' house. After graduating from high school in 1941, he attended the Massachusetts Institute of Technology [MIT], where he studied chemical engineering with Warren K. Lewis. In 1943, after enlisting in the Army, he went to basic infantry training. Cook then enrolled in the Army Specialized Training Program [ASTP], through which he attended Stanford University for two terms, studying mechanical engineering. After a year, Cook was sent to the Hunter Liggett Military Reservation, and then to Fort Benning, where he became an MP. While at Fort Benning, he joined the Officer Candidate School, and shortly after completing the training, was sent to fight in Italy. In February 1946, Cook left the Army and worked for Submarine Signal in Boston. He then returned to MIT, where he completed his degree in 1947. After graduation, Cook started the Warren Wire Company with his older brother. A year later, Cook left the fledgling company to join the Stanford Research Institute as a chemical engineer. There he worked on a number of projects, including the growth of the algae *Chlorella* and the potential uses of waste fission products. In 1951, Cook founded the Sequoia Process Corporation. Five years later, he left Sequoia to found Raychem Corporation, which opened in 1957. Cook concludes the interview with a discussion of Raychem's international competition, the growth of the company, his thoughts on managing innovation, and the possibilities of radiation technology.

INTERVIEWER

James J. Bohning is currently Visiting Research Scientist at Lehigh University. He has served as Professor of Chemistry Emeritus at Wilkes University, where he was a faculty member from 1959 to 1990. He served there as chemistry department chair from 1970 to 1986 and environmental science department chair from 1987 to 1990. He was chair of the American Chemical Society's Division of the History of Chemistry in 1986, received the Division's outstanding paper award in 1989, and presented more than twenty-five papers before the Division at national meetings of the Society. He has written for the American Chemical Society News Service, and he has been on the advisory committee of the Society's National Historic Chemical Landmarks committee since its inception in 1992. He developed the oral history program of the Chemical Heritage Foundation beginning in 1985, and was the Foundation's Director of Oral History from 1990 to 1995.

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INTERVIEWEE: Paul M. Cook

INTERVIEWER: James J. Bohning

LOCATION: San Carlos, California

DATE: 2 April 1992

BOHNING: I know you were born on April 25, 1924 in Ridgewood, New Jersey.

COOK: That's correct.

BOHNING: Could you tell me something about your parents and your family background?

COOK: Ridgewood, New Jersey, is a bedroom community for New York City. It's perhaps 12 miles from the George Washington Bridge. I was born there, as you say, on the 25th of April, 1924. My father was a mechanical engineer and a businessman. He graduated from the Stevens Institute of Technology in Hoboken, New Jersey, which at that time was one of the best mechanical engineering schools in the country. He was an outstanding mechanical engineer. He started a business with three college friends of his. I believe the business was started in 1924, the year I was born. His father, in addition to being a mechanical engineer and practicing mechanical engineering in his early life, had been a patent agent in Washington, DC. My father became proficient in the world of patents by working for his father, along with his two brothers. He learned a great deal about the field of U.S. patent law, and for a year was a patent agent—not a patent lawyer, but a patent agent—and thereby learned a fair amount about patents, which played a very important part later in my career.

He started this business in Paterson, New Jersey, with three college-mates. The name of the company was the Cornish Wire Company. My father was president of the company, but he was also the chief mechanical engineer, the chief designer of all the machinery, and the chief builder of all the machinery. He was a practicing mechanical engineer, but with a strong business background. In my early career, I was exposed to technology, math, mechanical engineering, and such, and that obviously had a major impact on my life.

My mother was the opposite in terms of her interests. She was very interested in music, very interested in art, and had been a nurse. She was a bundle of energy, and took care of her four children, which she carefully spaced in terms of family planning to be at least three or four years apart in age so that she'd have a chance to spend more time with each one in order to allow the development of their interests and skills. My brother was seven years older than I. My older sister was four years older, and I'm the third. Then my younger sister was six years younger than I. In terms of ages, my family was well spread out. My mother was tireless in terms of supporting all of us and allowing us to develop whatever interests we had, although strongly encouraging us to be interested in music and art and that part of the world. Whereas my father's stimulation was in the world of technology, engineering, and science. It was an interesting combination of influences on me, which I believe had a very positive effect on my later life. I went to public school.

BOHNING: In Paterson or Ridgewood?

COOK: In Ridgewood, New Jersey. Paterson was five miles away, very close. I went through grammar school, went through what was called junior high school (seventh, eighth, and ninth grades), and then through the Ridgewood high school. During those early years I had a wide variety of interests. I studied astronomy in some detail. I built a six-inch reflecting telescope myself and ground the mirror. That was a major interest for a while. Photography was a major interest for a while.

I started a chemistry laboratory in the cellar of our house when I was about twelve years old, and developed it into a very wonderful chemistry laboratory facility. With the help of my mother, I was able to acquire things that would not normally be available to a twelve- or fourteen-year-old boy. She used to go to New York City with me and we'd go to the chemical supply house named Eimer & Amend. I'd give her a list of chemicals that she would have to buy that they wouldn't sell to an underage person. [laughter] If she ever knew what she was buying, it would have been a disaster. I remember acquiring such things as picric acid, which I used to make bombs and explosives and all sorts of things. She was cooperative and helpful, and I was pretty conservative in terms of being careful. I never had any serious accidents, although there were many fun accidents.

The main point is that my mother supported my interests strongly, whether they be in the world of science or in the world of arts, and I can't say how much of an impact that had on me in terms of allowing me to explore and develop skills, interests, and talents.

I graduated from the Ridgewood high school. I played football, and sports were important to me. I lettered in football. I played in the high-school band and in the high-school orchestra. I was head of the bowling league. I did all sort of things like that that were important. I was a good swimmer. I was even a lifeguard one summer. I worked, throughout all of my early days, on vacations and especially in the summertime. I worked as a draftsman and as a machinist. I pumped gas. I worked in an ice cream parlor. I did a wide variety of things, always earning money, and built up some capital so that I could even buy a car and do things like that.

BOHNING: You were very young when the Depression came, and I was wondering what effect that may have had on your family.

COOK: It meant that we had to be very careful about the money we spent, but my father's business survived through the Depression. Although it was very difficult, through the ingenuity he had as a mechanical engineer, the company was able to have relatively low costs in its processing, and succeeded in a modest way. It never became a very large business, but it was doing in those days two or three million dollars per year. It was very modestly profitable, with small losses during some of the bad Depression years. They were able to keep it together and it continued to succeed.

BOHNING: Did you have any teachers in high school or elementary school who influenced you? You certainly were getting enough influence from your parents, but I wondered about teachers who may have also influenced you.

COOK: There were several teachers in junior high and high school, and even in grammar school, who had major impacts on me. They helped stimulate my interests, particularly in the world of science and technology, which happened even as early as the grammar school years. The ones that I remember in particular were the mathematics teachers. There was one teacher who was so good and so strong that I worked for him personally so that I had a chance to get more exposure to him. I helped him remodel his house and things like that. The physics teacher in high school was particularly strong and helped develop my interests, allowing me to go much further than what high-school physics would allow by doing outside and extracurricular work. It was the same thing with the chemistry teacher in high school. The public high school was an outstanding public high school. For example, I got into MIT [Massachusetts Institute of Technology] even without examinations because the school's record had been so strong at MIT. They were very, very good teachers who had a very big impact on my life.

I had difficulty deciding what school I wanted to go to. My first selection really was Caltech [California Institute of Technology]. The problem with Caltech was that it was so far away. The expense of transportation would have been such that my family felt it would be prohibitive for me to go there. I decided to go to MIT as the second choice and was delighted, of course, with the result.

BOHNING: Did you have any help in selecting Caltech or MIT as potential schools, or did you do that on your own?

COOK: Not really. I did that pretty much on my own by studying all the schools. I had some pressure on me from relatives that I ought to go to an Ivy League school to get better "rounded." My interests were so strong in technology that I wanted to go to a technical school. Really, the only two that I seriously considered were Caltech and MIT. I got into MIT without any difficulty.

BOHNING: What year was that?

COOK: I graduated from high school in 1941, when I was just barely seventeen. (I skipped a grade in my grammar school period.) I entered MIT in the fall of 1941, in the class of 1945. I spent two years at MIT. Of course, World War II came along in December of 1941. I was there during the first years of the war. I enlisted in the Army in the second year of my college career, and they didn't take us until the end of the second year.

I went into the Army in June of 1943 and was sent to North Camp Hood in Texas for basic infantry training for thirteen weeks. I was then put in what was called the Army Specialized Training Program, the ASTP, where they put college kids. The ASTP in turn sent me to Stanford University for two terms, or six months, while I was in the Army.

Whereas at MIT I had elected to study chemical engineering, at Stanford they put me in the mechanical engineering course. I got two terms of schooling at Stanford during World War II in mechanical engineering and thoroughly enjoyed the experience. I got to know California, especially northern California, and resolved that someday I'd come back to northern California. It took only a few years to do so. My impression of Stanford at that time was that it was a lot easier and more fun than MIT. It didn't have the pressure that MIT had in terms of training the mind for the high pressure, rapid decision, prodigious output that you have to have while going through MIT, at least as I remember it during the first two years.

BOHNING: Why did you select chemical engineering?

COOK: I was influenced by some counselors in high school. As you know, chemistry was very, very interesting to me. I did a lot of work in chemistry. The combination of my interests in business and in engineering, combined with chemistry, led to chemical engineering as the thing that I wanted to study. Strangely enough, what I wanted to do at MIT was to pick a department that I felt had the most outstanding professors in it, because I felt that training of the mind was more important than any factual content that you would learn in school. I continue to believe that that's true—college is a technique for training the mind and developing the mental agility and skills and intellectual strengths as opposed to any particular factual matter, until you decide what kind of long-term pursuit you want to have. I wasn't sure what I wanted to do long-term. I had a conflict of whether I wanted to be a technologist or whether I wanted to be a businessman. I've never resolved that conflict. [laughter] I've enjoyed both to a very tremendous extent.

BOHNING: Could we talk a little bit about those first two years at MIT before we get to your Army experience?

COOK: The first two years were thrilling and exciting to me because all of a sudden I went from an easy high school, where I really didn't get pushed hard at all, to an arena where the competition was tough and where the number of hours that you had to put into homework, etc., was prodigious. It was a totally complete change in lifestyle, and my mental motor had to speed up several times compared to what it had been doing so far. It was a thrilling experience.

I realized that high school was not as profound as I had thought it was when I was in high school. One example. While in high school, I was a member of a mathematics team and we would compete against other schools in various competitions. Our school was rated number two in the state of New Jersey, so we did very well. We got involved in a competition in New York, which was held at NYU [New York University], and I think we were one of fifty high schools that competed. Although we were the second strongest in New Jersey, it was clear that our mathematics skills and training were sub-par compared to any of the New York schools that had specialties in mathematics—I think we placed something like thirty-seventh in the competition. It was a good lesson to learn, that you might think you're pretty good but there's always somebody that is a hell of a lot better.

In the first two years at MIT I was stimulated by several people. I had the chance to attend lectures that were not in the courses that I was taking. I was particularly interested in getting to know Warren K. [Kendall] Lewis, and I sat in on his lectures. There were some famous physicists there who would give lecture courses, and I'd sit in on those because they were so stimulating to me.

With the war coming along, my interest in academics in the second half of my second year tapered off some as I anticipated going into the Army, and I spent a fair amount of time in the world of music. I used to go over and hear the Boston Symphony rehearsals, which you could get into for practically nothing. I used to listen to [Serge Alexandrovich] Koussevitsky and the training of the Boston Symphony, which was a great thrill for me. In the first two years, I lettered in ice hockey and I lettered in lacrosse, so I played a fair amount of sports in college as well.

I went into the Army in June of 1943 and was sent to Texas for basic infantry training. Having been an athlete, I was in good shape, but even for me that was a difficult period because I went from the moderate climate of New England to the intensely hot climate of Texas early in July. When we went through infantry training at that time, our company (a company consists of two hundred and twenty-five people or so) had seven people die of heat exhaustion or heat prostration during the first two or three weeks of training in this brand new camp that had been opened, called North Camp Hood, adjacent to Fort Hood. That was difficult, but excellent training. The Army has a lot of hurry up and wait. My patience was tested a lot in not doing things, especially coming out of an academic environment where the output per minute is so important and so precious. That was difficult for me.

I adjusted to it, however, and was delighted to get to Stanford for two terms. Then they terminated the ASTP because they needed people in the Army because so many were being killed. I was sent to maneuvers in California with the 71st Light Infantry Division down in the Hunter Liggett [Military] Reservation. From there I was sent back to Fort Benning, Georgia, with my division. Everybody over six feet in our particular regiment was put into the MPs [Military Police] and given the assignment of being an MP in the Fort Benning, Georgia, area. Fort Benning is just across the river from a city in Alabama called Phenix City, which was Sin City, U.S.A. at that time. Every night when we would be on parole duty, somebody would be killed or shot, or there would be some kind of very difficult situation, and we would have to break up fights and do all sorts of things like that. That had an important impact on my later life, as well.

The only reason for pointing out some of these things is to show the wide diversity of human experience that I had in my early life, because I believe that led in many ways to a leadership capability that later became very important to me as I led the development of a company. I knew an awful lot about interrelationships of human beings. My experience in the Army was fundamental to my development in that area. I was not what I would call an outgoing person; I was more of a private technical person. I wouldn’t call myself a nerd, but I was a pretty private and introverted person until some of these experiences in the Army brought about some significant changes.

While at Fort Benning, I applied for and was admitted to the Officer Candidate School, OCS, and went through the seventeen-week Fort Benning OCS infantry training program. The result was that I came out as a Second Lieutenant and was sent to Camp Croft, South Carolina for a couple of months. I was then sent to Italy, where I joined the 10th Mountain Division and fought in Italy with the 10th Mountain Division. Again, this had a very major impact on my development and on my life. I became a Second Lieutenant in 1944, so I was twenty years old. I then went to Italy in late 1944 and joined the 10th Mountain Division and fought in late 1944 and early 1945 until the war was over. The war was over on May 2 in Italy, almost a week before it was over in Europe. I remember we crossed the Po River on my birthday, when I was twenty-one years old. That was a significant day in our history, and the war was over seven or eight days after that.

My combat experience, although relatively short, again had a big impact on my life, because as a Second Lieutenant I was a platoon head. We fought in the mountains and then broke out of the Apennines into the Po Valley, and then went very quickly up into the Alps. We were just entering the Alps when the Germans gave up in Italy. For several weeks after the second of May, I had a chance, since I had a jeep of my own, to wander around the northern part of Italy and up into Austria. I got to know the Dolomites quite well, which is an area that I have returned to many times over the last forty years.

The Army had a big impact on me. I became a platoon leader, and later a First Lieutenant, so I had a chance again to observe my skills in handling people and adjust them so that I could affect, through leadership, the performance of a series of people. That became particularly important to me at a later date. I believe today that my experience in the Army had more to do with my development as a leader and a manager than any other experience that I went through. It's kind of on-the-job training, you might say. After the war was over, I had a great experience in Italy, having the freedom to go to many different places for the three months before we were deployed back to the U.S. I ran the regiment's newspaper. I ran a school that we set up to allow the soldiers to get some education. I taught such varied things as mathematics, chemistry, and even applied psychology by reading a book ahead of time and staying ahead of everybody by a lesson or two. It was a chance to see education first-hand, and I got a tremendous response from the soldiers, who were just so eager for education and training.

BOHNING: When you made the decision to enlist, why did you pick the Army? Was drafting inevitable at that point?

COOK: Being drafted was pretty much inevitable. I wanted to serve. I felt it was the right thing to do. I could have gotten out of it. A lot of people went into the Navy V-12 program, where they were pretty well assured of staying at MIT. My conscience pulled at me quite a bit, and I felt it would be proper for me to go in and serve in the Army. I didn't anticipate the ASTP program would send me to a place like Stanford, of course. In retrospect, that was a wonderful thing.

Although I don't know that I would select it or advise it for any kid, the experience of having fought in the Army was a human experience that had a major impact on me. I was lucky that it was short enough that I didn't become as bitter as many did. Strangely enough, I'd say it did me a lot of good. It made a better human being of me by a lot.

[END OF TAPE, SIDE 1]

BOHNING: How much actual combat did you see then? You must have seen quite a bit.

COOK: Just a few months. I was head of what was called the anti-tank platoon of the 2nd Battalion of the 85th Regiment of the 10th Mountain Division. We had a fifty-caliber water-cooled machine gun, which was our anti-tank weapon. It was used for lots of purposes, but hardly any tank purposes because we never fought in the flat, so tanks could never go anyplace where we were fighting. But the fifty-caliber machine gun was a very key part of our mountain warfare and a very important weapon.

I served in Italy for three months at the end of the war. They moved us around. After the war ended, we were moved to the east border between Italy and Yugoslavia, the Isonzo River Valley. We did some patrolling there. There was continued fighting in Yugoslavia at that time, and we had to monitor and control that. In addition, a lot of us went into Austria and processed a lot of the German soldiers for demilitarizing their people.

The division returned to New York in August of 1945, and it was the plan of the Army to re-deploy us to Japan. The Japanese War was still going on, of course. We arrived and steamed up the Hudson River. I believe it was the second of August, which was unofficial V-J Day. We got off the boat and ran into New York City and celebrated the unofficial V-J Day with everybody else. I think the official V-J Day, when the Japanese themselves finally signed, was the second of September. Whatever the date was, the unofficial V-J Day was a very big day because at that time I was given thirty days leave, so I had a chance to resume what amounted to civilian life.

I was married on the second of September, 1945, to a girl who I had met at MIT. We were assigned to Camp Carson in Colorado. At that time, since the war was over and we knew that we weren't going to be re-deployed to Japan, we were given an extra fifteen days leave. In essence, we had a fabulous vacation in the Colorado Springs area of Colorado. I was then sent to Camp Swift, Texas, because I had not been in the Army long enough to get out that quickly. At Camp Swift, Texas, I had a wonderful experience where, again, I was put in change of running an Army education unit for the soldiers. My wife and I lived in Austin, Texas and I got to know quite a few professors at the University of Texas at that time. One in particular, an art historian who was a young professor that my wife got to know quite well, later played a very important part in my life in terms of our interest in art; I'll get back to that before the story is over.

At Camp Swift I was able to arrange with the University of Texas a chance for our students at Camp Swift to attend some of the lecture courses at the University of Texas, and took advantage of that. They were very kind to us and allowed that type of exchange. In so doing, I got back into the world of universities and had a chance to expose myself to some other disciplines. In particular, psychology was a subject that I was interested in at that time, along with science and engineering.

That didn't last long, and I got out of the Army in February of 1946, which was too late to get into the current term at MIT. So I went to work for Submarine Signal in Boston for a few months, waiting for the summer term, at which time I went back to MIT. I did the last two years of my undergraduate work in three terms by taking an extra load and by getting credit for some (but not all) of the courses that I took at Stanford. There were some mechanical engineering courses that were transferable to MIT for credit. As opposed to having two more full years, I was able to graduate from MIT in three more terms by taking a tough load and getting through pretty quickly. That was 1947.

BOHNING: What was the chemical engineering department like when you were there?

COOK: It was superb. The chemical engineering department during World War II had an awful lot to do with the synthetic rubber program. Warren K. Lewis and Ed [Edwin Richard] Gilliland were both very important stars in that whole wartime effort, and they became very, very important professors for me. Warren K. Lewis had a very strong impact on me. He was a riot.

A lot of us in the class were veterans. Having fought a war, we weren't particularly affected by people. Lewis would come into class and enjoyed the exchange with the veterans, especially the bright ones. He always came into class a couple minutes late. He came in with those glaring eyes that he had, these fiery eyes, and he'd look around the room. He always carried a whole bunch of books and he'd slam the books down on his desk. He would pick on me as often as anybody. He'd always save up a tough question that could be answered only if you'd done the proper preparation for the class. He'd slam the books on the desk and he'd just as likely say, "Cook, you horse's ass, what's the answer to this question?" Most of the people in the class would cringe and dissolve into a corner, but it was a fun experience for me because I'd stand up and shout right back at him and give him what answer I could. It was an interesting and fun exchange. Warren K. Lewis had a major impact on me. I spent a fair amount of time with him after I got to know him outside of the class.

Ed Gilliland had an equal impact on me. In many ways Gilliland was more important to me because his sensitivity and knowledge of physical chemistry was something that I was particularly interested in, and its application to chemical engineering was particularly important to me. I got a lot out of Gilliland, and I always stayed in touch with him long after I'd left MIT and even after I'd left the east coast.

BOHNING: What kind of laboratory experience did you have? Did you get much practical experience?

COOK: As an undergraduate, they did have an undergraduate thesis program, which is now MIT-wide. The UROP program [Undergraduate Research Opportunities Program] is a very well-known and popular program. This was the beginning of undergraduate theses. My thesis was on the high-pressure electrode dissolution of water into hydrogen and oxygen. I measured the efficiency at high pressures as compared to the electrolysis at atmospheric pressure. There's a surprising increase in efficiency when you electrolyze water under pressure. That gave me a chance to interface with Gilliland in some detail. I wrote a very modest undergraduate thesis. That was a chance to do some original laboratory work, albeit, the idea came from somebody other than me; but it was a fun thing to do.

I considered going on to graduate school. I also considered going to business school. Harvard Business School was something that I thought about, but I had an opportunity to start a business with my brother, supported by my father. I left school when I concluded my undergraduate work in June of 1947 and became the president and the operating head of this company, which was called the Warren Wire Company, that my older brother and my father had figured out was a good opportunity. I went to Williamstown, Massachusetts, where this business had started, which was adjacent to a plant that my father's company had there and which my brother John was running. He was operating this business as the head of this factory of the Cornish Wire Company in Williamstown, Massachusetts, one of the two factories that the company had. I ran this little start-up company to make enameled magnet wire. In essence, this meant processing bare copper wire through an enamel bath and then heating it and driving off the solvent and curing the enamel on the wire to make a very thin-walled magnet wire, which is used to wind transformers and motors.

BOHNING: What was your brother's background?

COOK: My brother, who is seven years older than I, also went to MIT and was a mechanical engineer. He was more of an extrovert than I and more of a socialite than I. He was interested in business and management more than technology and science, so we differed a little that way. He was very capable and had an interesting career. He was in the Army in World War II as a Captain in the Signal Corps and was involved in Signal Corps procurement activities.

My older sister was a secretary; she went to the Katherine Gibbs School in Boston. My younger sister, with my support and encouragement, went to Stanford University. That was the rest of my family. There was enough difference in age between my brother and I that I really didn't have a lot of interaction with him until we went into this business after World War II. I ran the company and developed and worked out the processes and that type of thing.

My brother and I did not get along. I didn't respect a lot of the things that he wanted to do. After a year and a half of running this business, I had a chance to join a friend, Ralph [Alvin] Krause, who I had met at the radiation laboratory at MIT in 1947. He was the associate director of the radiation lab. I should point out that the girl I married had been working at the radiation lab at MIT in World War II. When I got back from Europe and married her, she continued to work at the radiation lab while I was going through school. She and the government put me through the rest of MIT. Ralph Krause had a very big impact on my life. He and Jerrold [Reinach] Zacharias, a very well-known physics professor at MIT at that time, were important people who impacted my life.

Ralph Krause went out and became the director of research of what was then called the Stanford Research Institute [SRI], which was really just getting started in 1948. I think he came to California and joined SRI in the early part of 1948. He knew I was having conflicts with my brother. He urged me strongly to get some industrial research experience and offered me a job to join the Stanford Research Institute as a chemical engineer. I accepted that and left my brother, who arranged to leave my father's company and take over at the time. I left, came to the West Coast, and joined the Stanford Research Institute as the forty-eighth employee. Ralph Krause, who had urged me to come out and follow him, was the twenty-fifth employee, or something like that. Jesse [Edward] Hobson, who was the director of SRI, was another man who had an important impact on my life; he was somebody who supported me strongly.

I joined the Stanford Research Institute, arriving in California on election day of 1948, when the *Chicago Tribune* announced that [Thomas Edmund] Dewey had beaten [Harry S.]

Truman. It wasn't until around two or three o'clock in the morning that the final California results were tabulated, and therefore it was concluded that, in fact, Truman beat Dewey in one of the closest presidential elections this country has ever had. It was an interesting time.

BOHNING: What was your first assignment at SRI?

COOK: I had a series of assignments. One involved the fluidization of sawdust to drive out the water and, in essence, make charcoal of the sawdust in an oxygen-limited fluidized-bed arrangement.

I had another assignment, taking some scientific results from the Carnegie Institution of Washington laboratories facility at Stanford, which was supported by Carnegie, to see if the growth of the algae *Chlorella* could be made into an industrial process. Vannevar Bush was the head of Carnegie. Since scientists had been able to grow *Chlorella* so rapidly, and since it had such a high protein content, he wanted it studied to see if it could be made, in essence, into a forced continuous growth process for the production of food.

That was a fascinating project for me. I was later made head of the project because of what they felt was some very excellent work that I had done. We developed a continuous process for growing the *Chlorella*, measured the results, how much light conversion could take place in the photosynthetic process, controlling the variables of temperature, light, density, et cetera. Then we built a larger subpilot plant to grow massive quantities of *Chlorella*. Then the *Chlorella* in turn was used for feeding rats and a nutrition determination of the algae was made. I received a patent on the process (1). That was an interesting project, as well. It's hardly what you'd call industrial research, but it was something that I was put on, and which I thoroughly enjoyed. It took a year and a half or so of my time.

BOHNING: You have a paper in *Industrial and Engineering Chemistry* on the process (2).

COOK: Yes, and a paper to the American Phycological Society, as well. That's a pretty obscure part of science. I enjoyed that because of the interchange with the scientists, not only Drs. [Herman Augustus] Spoehr and [H. W.] Milner at Carnegie Institute at Stanford, but all of the people involved in that science. I enjoyed that thoroughly because of the scientific work they were doing. They in turn, I think, enjoyed their relationship with me because of the engineering and technology approach that I was using.

The important assignment that I then received at SRI was heading up a combined economics and technology study sponsored by the Atomic Energy Commission [AEC] to determine potential uses of waste fission products. What possible commercial applications could be found for the products of nuclear reactor operations, the creation of the very troublesome fission products? That assignment ultimately led to what has been my whole life's career. In doing the work for the Atomic Energy Commission, we had to understand what the waste fission products were, what separating them could do, doing the chemical separations into their elemental parts. This is one of the things that we looked at. We looked at using the waste fission products, concentrated but used together as a whole.

The main study was of what could be done using the radiation from the waste fission products and what the potential industrial applications for ionizing radiation were. The fission products being alpha emitters, beta emitters as well as gamma ray producers, led to the study of all of those. There were some alpha emitters in there that could be separated out. There were certainly beta emitters that could be separated out, and obviously it is a huge producer of gamma rays, cesium-137 being probably the most dominant. The study went on for quite some time. We became very proficient in the field. I had to learn a new science, which I had not known a lot about. It was the research into a new field that then had to be looked at from the standpoint of practical and commercial potential.

BOHNING: How many people were working in this group?

COOK: I think there were two people doing the economics part of it, and I was the only one doing the technical part of it. It involved extensive travel to all of the AEC installations throughout the United States, whether it be Oak Ridge [National Laboratory] or Brookhaven [National Laboratory] or Hanford [National Laboratory] or Idaho [National Engineering and Environmental Laboratory], for the reactor station there. It gave me exposure to a large number of people who had worked in the field of fission. The study concluded with an interesting recommendation. We concluded that there were indeed important potential commercial applications for ionizing radiation. It was at that time that high-voltage engineering had developed the Van de Graaf generator, and at the same time the General Electric Company had taken their x-ray machine and had made an electron beam generator out of it. The conclusion of the study was that there were indeed important applications for radiation, but the best sources of radiation were going to be machines, not waste fission products. The expense of concentrating the fission products and then packaging them was so great that a machine that could be turned on and off and powered by electricity was a very attractive way, indeed.

The result of that study encouraged me to begin to look for industrial applications for ionizing radiation. As a result of that, I promoted at Stanford Research Institute the establishment of a radiation laboratory to study potential industrial applications. I was made the head of what was called the Radiation Engineering Laboratory. We acquired a large cobalt-60 source and created a sizable swimming-pool type of shielding so that the experimental work could be done under water in a variety of experimental chambers to allow the impact of the gamma rays emitting from the cobalt-60 at quite high intensities. These were primarily chemical experiments, but we did the sterilization of foods. More importantly, we did chemical reactions.

At that same time, in the 1950-1951 time frame, while the work was going on in radiation, I decided, with some friends of mine at SRI, that I would start a company. I asked for and received permission from SRI to carry out a night-time and weekend business operation to make an electronic hook-up wire.

[END OF TAPE, SIDE 2]

COOK: In 1951, a company called Sequoia Process Corporation was founded. It was initially founded to do the production of some chemicals, but quickly changed because of my knowledge of the market for wire and cable through the experience with my father's company and the experience with my brother's company. We decided to acquire and buy an extruder, and to purchase tin-copper conductors and extrude polyvinyl chloride [PVC] insulation onto those with a nylon jacket, a very thin nylon outside coating for strength and abrasion resistance. We made that wire and sold it as electronic hook-up wire to the large aerospace companies and the electronics companies, primarily in the Los Angeles area.

I raised money for the capitalization of that company through a friend of mine who I had developed at SRI, a man named Bill Valentine. Bill Valentine had supported a development of a zinc reduction process that I worked on while at SRI, and we became pretty good friends. He was willing to be, in essence, an early venture capitalist by supporting the formation and later expansion of this Sequoia Process Corporation, which we later renamed Sequoia Wire Company. This was done in a little sixteen-hundred-square-foot Quonset hut in Redwood City, where we installed an extruder and accessory equipment and produced wire.

It became modestly successful and grew to the point where I left SRI just before my fifth anniversary there. That would have been in 1953. It was the early part of 1953 when I became a full-time CEO of the Sequoia Wire Company. That grew and expanded, and I made the classic mistake of letting Mr. Valentine own over 50 percent of the company as we received more money from him to continue our expansion. The company, at that time, was doing perhaps a couple of million dollars of business a year. It was, as I say, modestly profitable but growing rapidly. After Mr. Valentine got 51 percent of the company, as was his right, he asked me to do some things that I didn't want to do. To make a long story short, we had a serious conflict. I left the Sequoia Wire Company and started immediately thereafter Raychem Corporation, which was initially called Irradiated Products Incorporated. The name was changed in the second month to Raytherm. When we selected that name, Raytheon [Company] objected, saying that the name was too close to them, so we changed the name then to Raychem for radiation chemistry.

BOHNING: There are a couple things before we get to Raychem. You had several patents on radiating polyethylene (3). Did they come out of SRI?

COOK: No, they did not come out of SRI. That was later.

At SRI the only patents that I applied for and got were on this algae process, the *Chlorella* process.

BOHNING: Where does this interest in starting companies come from? Is that your father's influence?

COOK: Undoubtedly. My father's business interests and his strong technical background clearly were very influential. I was always excited to hear the conversations about business and always excited to see how technology, in fact, could be used for developing a business. I quickly learned about patents, and believed that a company doing innovative technical research could get a jump on everybody else and make a greater profit margin than others, particularly a greater gross profit margin, by being first and smarter. That's why I wanted to get more industrial research experience after I left the Warren Wire Company. I wanted to know more about how you developed industrial processes and industrial products. That's what led to my joining the Stanford Research Institute.

I believe that those several years of experience gave me exposure to that area, especially being able to see what the large companies of this country did to develop products. I felt that SRI was a great place to do it because it gave me exposure to those companies supporting work at SRI, as well as a chance to go around and try to sell ideas to the large companies and get exposure to them that way. In fact, that worked. It was a technique that was very successful indeed, and I learned a great deal about industrial research and how it was done and who did it well. I learned also who especially capitalized on it, leading to important innovative products that carried good price-to-cost relationships, which I felt was the secret to a rapidly growing and successful company. That turned out to be absolutely true. What I did learn at SRI was very, very productive, indeed. The fact that I'd had hands-on experience running the Warren Wire Company for a year and a half was very helpful. The experience I had developing the Sequoia Process Corporation was a very helpful experience, as well.

I'd had lots of learning experiences, all going in the same direction of trying to get myself ready for leading an effort to industrialize high technology. I didn't consider myself a particularly brilliant scientist, but I did have the capability to put technology together with the marketplace. I had enough drive and intensity to be sure that a process or a product would be developed quickly, efficiently, and successfully. I had lots of failures, and I happen to believe that you learn more from a failure than you do from a success because you know what not to do the next time. When you have a success, you're not sure whether it's luck or whether it came from the proper use of resources or whatever. I've had lots of failures in my life, and I've gained an awful lot, I hope, from having been through them and learning what not to do the next time.

Now, I've got you in the time frame of about 1953 when I left SRI, ran Sequoia, and built it up to a company doing several million dollars of business a year. I had the fight with Valentine late in the summer of 1956 and I resigned and left. I took a couple of months off to go to Europe and figure out what I wanted to do next, and decided that trying to do the industrial applications of high-energy ionizing radiation was what I wanted to do.

BOHNING: Were your disagreements with Valentine on the technical side or on the business side?

COOK: On the business side but revolving around technical things. I wanted to develop some products, and he didn't want me to spend any money developing those products. They were products centered around radiation. He felt that was a mistake and that the money shouldn't be spent on that, and that's why I left. It was just at that time that the General Electric Company had for the first time developed what I would call an industrial electron beam generator. As I mentioned, an electron beam generator had come out of GE [General Electric Company] and in high-voltage engineering several years before that, but both the machines didn't have enough beam current to make a process practical. It would allow experimental work, but not industrial processes.

It was just at that time that GE had developed a swept beam, so that the electron stream could come out of the vacuum and the vacuum tube into air, and would be swept in both directions over a horn at the end of this vacuum tube that was about twelve inches wide by about two and a half inches wide. The titanium window was there to maintain the vacuum in the tube, but was as thin as possible, as low Z as possible so that, first off, the least amount possible was converted to x-rays and the most energy was allowed to pass out into the air. GE had this swept beam, which was 1 MEV and 6 milliamps. Prior to that, the largest was 1 milliamp or, in other words, 1 kilowatt, 1 MEV at 1 milliamp. This new one was 6 milliamps at 1 MEV (1,000,000 volts), and that 6 kilowatts was enough to make practical some of the processes that I had envisioned.

I personally rented that machine, the first one that GE produced, to install as part of this new company that I was forming, the Raychem Corporation. I mortgaged my house and pulled together as much money as I could. For fifty thousand dollars of equity money plus the personal lease of this GE electron beam generator, a resonant transformer electron beam generator, I started this company in a little building in Redwood City. We had 5,000 square feet at 2821 Fair Oaks Avenue, a building that I revisited just a few years ago on our twenty-fifth anniversary. I opened the doors of this business on the first of January, 1957, New Year's Day. The business was incorporated on the fifteenth of January, 1957.

We proceeded to develop a series of products. We rented this GE machine, which was delivered in March 1957. We had built a housing and shield for this machine through a very clever, inexpensive innovation. We built walls around the machine to provide the shielding. We built them out of wood and made an annulus and then filled the annulus with sand, holding the wooden walls together with steel reinforcing rods. The sand was the cheapest form of shielding that I could think of, and it was very effective indeed. The only problem with it was that every time there was an earthquake the sand would be compacted some and we'd have to open up the walls and put some more sand in to make sure it was filled. [laughter] It was what is called a radiation shell, and we built it for something like seven thousand dollars. Today, to build a proper radiation shell is something like a million and a half dollars. It was an innovative and inexpensive way of forming a radiation capability.

During the first year of Raychem's history, 1957, we developed five products, all of which I invented. Three became successful, and two did not become successful. The three products that did become successful were a flame-retardant, cross-linked polyethylene hook-up wire, for the wire in the cable field; a high-density polyethylene foamed insulation for a coaxial cable for the military; and a heat-shrinkable, flame-retardant polyethylene tubing that, upon heating, would recover and return to an original smaller diameter shape. This was the predecessor of the whole field of heat shrinkable tubings and molded shapes and parts that are the basis of 80 percent of Raychem's one billion, three hundred million dollars per year of business, even today. That first year was very productive in terms of beginning an entirely new industry, an industry that today is probably five billion dollars a year worldwide. Products of the use of radiation had originated in this tiny little building in Redwood City.

BOHNING: Five products in a year is pretty good! How did you get your ideas? Where did they come from?

COOK: The cross-linking of polyethylene with ionizing radiation is, even today, in dispute in terms of who is the originator of that technology. A man in England named Arthur Charlesby was certainly one of the early workers. GE, in their laboratories in Schenectady, with somebody who'd be familiar to many people in this country, Art [Arthur M.] Bueche, together with Elliott [John] Lawton, did some pioneering work in using radiation to cross-link polyethylene and make a tape of it. The tape would shrink a little bit and bond to itself and form an insulation. GE thought this was a very important process.

The combination of those things led to my originating the tubing which would shrink. Only I made our tubing shrink about two to one, whereas their type would only shrink a small percentage. I came up with the idea of making a tubing that would be extruded in x diameter, then irradiated by exposing it to the high-energy radiation in a special process that we developed. Then that cross-linked polyethylene tubing would be heated up. Our original experiments were to put it inside of a piece of pipe and use an air compressor and just blow it up by increasing the internal pressure on the tubing so it would expand against the wall of the pipe. We made it in four or five foot sections, and sure enough, by heating it, it would return to its original diameter.

Then one of the other original people in the company, Dick [Richard W.] Muchmore, and I developed a method of doing this by an original continuous process, to expand the tubing in continuous long lengths into a shrinkable tubing. That was one of the three successful products the first year.

The company made its first profit in the month of August, eight months after it opened its doors. In September, the beam tube of this GE resonant beam transformer busted, and for eleven weeks we were without any radiation capacity. As a result of that, our customers—whom we had developed to use these exciting new products as the result of radiation chemistry—became disillusioned with the fact that they couldn't get delivery of the product. As a result, we had no income coming in. The company almost went bankrupt. We had to lay off most of the people in the company at that time, and there were seventeen survivors. Those seventeen survivors all had to take pay cuts. I took a cut of 33 percent, and then we scaled down the reductions of the pay of the others. The minimum anybody got was 15 percent. For eleven weeks we had a great problem.

I guess maybe it was October when this first happened, because we got a new tube from GE about two weeks later and installed it, and it didn't work. We received a second tube from them and it didn't work. I think it was the fifth tube that we finally got from GE, on Christmas Eve of 1957, that worked. We found one that worked, and we went back on the air. We worked throughout Christmas Day and New Year's Day, and got back into production and barely survived. We won back the friendship of the customers that we had developed and quickly ordered a second unit and also a couple of standby tubes, and we were in business for good.

BOHNING: Were you involved in the original selling?

COOK: Oh, yes. I was involved in everything. I was the chief salesman. I was the chief technologist. I was a little bit of everything. The big initial customers were Convair, part of General Dynamics in San Diego; the General Dynamics Company in Fort Worth, Texas; and Lockheed [Corporation] in Burbank. Those were the biggest original purchasers. Joining them not too much later was Lockheed's Missiles Division in Sunnyvale. We had done a lot of business with them regarding the Polaris missiles, as well as the satellite business later. It was highly military, highly defense-oriented. That's because the environmental needs of military equipment are more severe than in the commercial world. Those were easier sales for us because one customer would buy more product than customers from the commercial world. I think something like 70 percent of our business the first few years was U.S. government end-use type business. We became concerned about that. The U.S. government once canceled a major military program, the Skybolt missile, that impacted us very severely, and put us into a loss position for awhile. We resolved at that time that we were going to become much less dependent on military efforts, and we did.

BOHNING: Did you have any competition?

COOK: Raychem has competition today in many, many different areas. The Japanese, early on, picked radiation chemistry as one of the technologies that they felt was going to be important in the future and approached us for licenses. I visited Japan and saw the companies that wanted the licenses and decided not to license. That irritated them considerably. By that time, we had built a modest patent portfolio. One of the companies, a company named Nitto [Boseki Co., Ltd.], elected to go ahead and infringe our patents and started selling heat-shrinkable tubing in the United States. We sued Nitto and elected to pick the venue of Texas, since they had sold products in Texas infringing our patent. Not only did we sue them in Texas, but we also asked for a jury trial, anticipating that perhaps Texans wouldn't be too friendly towards an infringing Japanese company. To make a long story short, that court suit was settled by Nitto withdrawing the product. I became quite friendly with the CEO of Nitto. We developed a long, personal, and friendly relationship as the result of our suing Nitto fairly early on.

We've had lots of competition for years, but no one company's ever done a wide variety of things with radiation like Raychem. We had a big lead initially in the field, and I believe strongly that, rather than having a few products with huge markets, it would be much better to have many, many products with relatively small markets. It would give us protection through our having the collective efficiency of processing many different products through the common radiation process, and we could run ahead of the rest of the world by doing so. That theory proved to be very well-founded, because we had literally thousands of different products that we made by radiation processing, taking advantage of the huge capital investment and our huge knowledge lead in technology in terms of the radiation processing.

Today, I think Raychem still has well over 50 percent of the market of all of the radiation products that it makes. That's in spite of the fact that lots of Japanese companies have been trying to do it for well over twenty-five years. I think there's something like fifteen Japanese companies in the radiation processing field. Raychem was formed in 1957, so we've been in business now for thirty-five years, and it's given us a remarkable lead and opportunity.

We also felt that it was critical that we serve all of the available markets of the world, as opposed to just concentrating on the U.S. We immediately spread to Europe, and we went to the Far East, including Japan itself, so that there wouldn't be an open window through which a competitor could pass and enjoy a market unimpeded by competition. About 60 percent of our business is outside the U.S. and 40 percent inside. Of that 60 percent, we manufacture products where we sell it. We've got major facilities around the world with concentrations in Germany, France, the U.K., Belgium, Denmark, Japan, Brazil, and Argentina. We're well-placed and spread around the world, and we take advantage of the countries where they like to buy from local sources as opposed to imports. We've taken advantage of that.

[END OF TAPE, SIDE 3]

BOHNING: You said that when you first started, you were involved in almost all aspects of the company. How many employees did you have initially and how rapidly did that grow?

COOK: We got up to thirty-four people in August of 1957 and chopped it back to seventeen when we had the beam tube failure. In essence, it grew 25 percent per year for twenty-five years. That was our average growth rate for that period of time. We have something like eleven thousand employees around the world today, roughly 40 percent of them in the U.S. and 60 percent outside the U.S. In other words, well-balanced and well-hedged—where we manufacture products and where we sell products are equal.

BOHNING: There were a number of things that you said in an interview that interested me (4). You once used the term "patient capital." Willard [H.] Dow used that phrase just before World War II, when Dow was developing their styrene process. He talked about "patient money and a prayerful attitude."

COOK: The patience that Raychem has practiced is in the development of new technology. It takes patience and time to develop a new technology, but it has a major reward at the end of the patience. We didn't have any patience in the first year because we didn't have a lot of money and we had to get products out and get them sold and get a quick return. The patience was that we believed in and stuck to radiation chemistry as the technology that we were expert in, wanted to become expert in, and wanted to exploit through a myriad of different products. Albeit, all these products had relatively small markets because we were afraid that if we invaded a large market, the big chemical companies could easily come along and do the same thing that we had done.

Although we felt that patents were good protection, we didn't feel that you could count on patents to protect yourself against the large chemical companies that would have huge amounts of money to either get around the patent or to fight the patent. In those days, patents weren't as strong as they are today, and they didn't provide as much protection as they do today. It's been, in my opinion, a very happy change in the legal patent arena in the United States.

The patience required to stick with the technology and continue its development, even when you're not sure that the long-term is going to be productive, takes a fair amount of courage and you can make mistakes. Patience requires, I believe, an accompanying gross profit margin that is quite high to allow you the freedom to make mistakes. If you're in a commodity business where the gross profit is very low, you don't have the discretion to make investments that go over a long period of time. I think patience comes from high gross profit margins, just as you see in the world of semiconductors, where a company like Intel [Corporation] enjoys quite attractive high gross profit margins, which in turn allows them to put a fair amount of money into technology development.

I don't know how much we ought to go into some of the details of Raychem or whether you want this to have more of the technology bent or a management bent.

BOHNING: I'm interested in all those aspects. I was going to ask about how you hire people and what you look for in new employees.

COOK: The key to the success of any company, especially a high technology company, is in its people. Hiring people is probably the most important single criterion for a company's success, in my view. We spent and do spend a lot of time looking for, attracting, hiring, and then training employees. We have a very generous compensation program for results.

One of the things that we do, in terms of hiring people, is that first we process potential important employees through a large number of management interviews. In general, we will put prospective candidates through ten or twelve interviews, which is a big burden in terms of time. It takes a lot of time. We have found that, as opposed to putting somebody through three or four interviews, increasing the number of interviews to ten or twelve gave us enough additional information so that by the time of the fourth or sixth or eighth interview we'd have identified any weaknesses and we would have a chance to penetrate those weaknesses. This allows us to understand the individual that we were proposing to hire in great depth, so that we would have very excellent feedback. For that reason, we are able to create very high quality employees for the particular type of thing that we are looking for. That is, of course, innovative, strong technical people that can put together a technology with a market opportunity.

Your suggestion is a good one; that is a very key part of the success of a business with a high gross profit margin. It's not a commodity-type business where you're trying to do the same thing everybody else does cheaper and better. I always wanted to do things that were different, always wanted to have products that were original, always wanted to be first. I did not believe that being second is the way to win in business, and I continue to believe that today. Being first with a new technology, even though it takes extraordinary energy, time, and resources, is the better way to go.

It's an intellectual ball game today. The U.S. is not going to succeed in the future, in life and in the commodity business, just because there are always going to be people who can become educated that will do something that's been done before faster and cheaper because they're willing to accept a lower standard of living and accept lower pay. That's happening in Korea, and that's happening throughout the Pacific basin. Japan is beginning to have problems themselves, because Korea's making products cheaper than Japan does and they can land steel in Tokyo cheaper than Japan can make steel in Japan. That's inevitable.

It's an intellectual ball game in business, and if you don't continue to progress in technology and innovate, you're not going to be able to sustain success. This country's problem at the moment, in my view, is that they're letting the education system fall so badly that it is endangering our whole standard of living. That's a different subject that I'm sure we don't want to get into here, but it's one about which I feel pretty strongly.

BOHNING: Your key here in innovation is a large number of products in smaller markets and being first. How much freedom did your people have to be innovative? How did you encourage them?

COOK: A great deal. We've always had a central research facility where we tried to penetrate the basic technology and the basic science itself, or to do research in those areas that were common to all of our divisions. There was innovation in central research, which was more of the science and technology variety. The product innovation came out of the divisions. The divisions were responsible for developing their own products, and we did a lot of cross-fertilization by moving people back and forth between the division development areas and the central technology area. The information was shared and cross-fertilized everywhere.

We looked for innovative people, found innovative people, and as I said in the *Harvard Business Review* interview (4), we asked for innovation. I happen to believe that either people are basically innovative or they're not. It's something that is very difficult to teach. I think your body biology has got to be built that way. You've got to have a curiosity that makes you think about how things work and what you can do that's different and new. When you're trying to do something that's different and new, you've got to wake up in the middle of the night and think about how to solve the problem. You've got to think about it at odd times and you've got to have a relentless drive to answer the question. I don't think it strikes you like lightening; I think you have to really force it and push it and think about it.

For that reason, I think the management of innovation requires encouragement, interest, support, congratulations, and thanks for success, but the most important thing is interest. The person who is innovating new products, I think, gets more excitement from achievement and recognition of achievement than they do from financial compensation. I've said so many times. The interest of management in the <u>process</u> becomes fundamental to success. I claim that the manager who gets the best success is the one who continually asks his innovative people how they are doing, how their experiments are progressing. He asks questions about the magnitude of the success, the details of what they're doing. Nothing is more exciting to a developer than having somebody appreciate what they're doing, know it in considerable detail, and to follow the progress almost on a daily basis. If nobody's doing that, somebody can lose interest and excitement, and something that would be possible to do in a week could end up taking months. I believe that in a structured, large company environment, that's an easy thing to have take place. The intensity of small units, with a management structure that follows success and failure, is an important part of managing innovation.

BOHNING: Did you target a customer and then try to find a product, or did you find a product and then try to target a customer?

COOK: We were mostly a market-driven company. Obviously, we had the basis of radiation chemistry, which is the technology. Then the task is to go out and look for opportunities. The most difficult people to find were people who could put together a market opportunity with the technology and come up with an innovative answer. We looked hard and long for that type of person. I happen to be very good at that. I'm good at seeing a marketplace and understanding the economics of the marketplace; that is, what a product is worth to a customer, potentially, and then in turn, what can be done with the technology to produce such a product.

We would look at market opportunities in a wide variety of marketplaces. We divided the marketplace up into typical electronics versus electric energy versus telecommunications versus process industries. Then we'd organize those industry segments into market-oriented divisions, and use that as the structure through which we would develop products. In other words, the telecommunications industry would have a series of problems, such as environmental protection of telephone cables, and we'd attack those problems with the tools that we had at hand that we had developed over the years. That has been a very, very successful way of developing products. Those development efforts were always supported by the central technology of the company where basic discoveries were made, where basic skills were developed, where a variety of polymers would be cross-linked and formulated and fixed to answer the environmental needs.

BOHNING: In the *Harvard Business Review* interview (4), you also said that you basically developed all of the technology in those early years and you're still going back to that to develop products.

COOK: Yes, it's interesting. I think that in the first five years, we came up with at least the ideas for essentially everything that could be done with the technology. Obviously, we didn't have the money and the resources to do all of them or even a lot of them, but we thought about them all. I think when you invent a new technology, you can very quickly consider the applications in great detail that can be accomplished by that technology. I don't think that the semiconductor field has been doing anything that wasn't envisioned years ago.

The use of high-energy ionizing radiation to accomplish chemical change, of course, is the basis for Raychem and 80 percent of what we do. The physical phenomenon of what happens is relatively simple. Most of the radiation forms that we use are high speed electrons. They will range in general from .5 MEV up to perhaps 3 MEV, which is where most of our radiation is done. The only difference between whether it's .5 MEV or a 3 MEV is in the penetration distance that the radiation will make. In a plastic material whose specific gravity is generally in the range of one, from a polyethylene at .92 on up to a polyvinylidene fluoride at 1.7, it's still within a relative narrow range. The amount of radiation, in turn, will determine the amount of chemical change. The electrons penetrating a plastic will go in and be very indiscriminate in terms of how the plastic gives up its energy. It will sever chemical bonds and then, in essence, create free radicals.

The irradiation of polyethylene is the simplest. When you bombard a polyethylene with high-energy electron radiation, in essence you'll knock off hydrogen atoms, and you really do evolve hydrogen gas from the process. One molecule will join to an adjacent molecule where that hydrogen atom has been eliminated. That cross-linking is the foundation of everything that we do. The cross-linking of plastics in the solid state—that is, in the crystalline form if it's a crystalline plastic, or in an amorphous form if it's an amorphous plastic—is in essence what we do. You cannot do the same thing with another process very easily. You can't do it economically. If you have a plastic material and you heat it up, obviously it melts and changes its shape or loses its shape.

What we do, to give a typical example, is make a tubing of a crystalline plastic—polyethylene, polyvinylidene fluoride or any one of the many, many plastics that we use. We irradiate that in its tubing form. By irradiating it, you cross-link it. That is, you cause a three-dimensional network to be formed where one molecule will join together, or chemically bond, with an adjacent molecule. The more you irradiate it, the more cross-links or chemical bonds that you form joining together this three-dimensional network. With a thermoset plastic you'll have a cross-link every few carbon atoms. In the type of work that we do, you only have one cross-link for every one hundred to a thousand carbon atoms, depending on what type of property you're looking for. For example, when we join together adjacent molecules by cross-linking a polyethylene tubing, where we want the network to be strong enough to cause it to recover quickly and sharply, it'll require perhaps one cross-link for every one to two hundred carbon atoms. That, in turn, will determine how large a radiation dose we give it. We make a wide variety of products. In a wire and cable insulation, you want more cross-links so that you'll have more strength above the crystalline melting point.

I might just recite here how the heat-recoverable plastic works. You're talking about crystalline plastics. You cross-link them in the crystalline room-temperature form. You normally do not cross-link in the crystalline areas; you cross-link in the amorphous areas of the plastic, and those cross-links then form the three-dimensional network. When you heat up the plastic that has this network of cross-links and a three-dimensional structure, you melt the crystals and in essence make an elastic out of it, because it is an elastomeric material in its non-crystalline, heated, cross-linked form. When you apply a stress to that elastic material, just like an elastic band, it will stretch. As long as it is still heated and above the crystalline melting point, it will act exactly as an elastomer. You stress it and it will strain; you relieve the stress and the strain will recover.

As I mentioned before, you adjust the cross-link network based on how much strength you want to have in the elastomeric form. What we were able to accomplish, even in the earliest days, was to vary the cross-link density in order to accomplish the various product functions that we were after. As I mentioned, when you have a wire and cable insulation, you're looking for a material that will be functional at high temperatures in case there is a short circuit or an excursion to high temperatures for a short period of time. For wire and cable insulations, we would create a fairly high cross-linked network. For the heat-shrinkable tubing, it would be a lower cross-linked network.

Back to the example. If you make a tubing and cross-link it at room temperature and then heat it up, the elastic network takes over since the crystals have melted. You stretch the tubing. That is, you blow it up. You create a differential pressure so that the internal pressure is higher than the external pressure and that causes the tubing to expand, but only the dimension of the diameter. The circumference will increase, but the length will not change.

The heat-shrinkable tubing operates on the basis that first you extrude the tubing and cool it so that it's crystalline and then you irradiate it, which cross-links it. Then you heat it up again to melt the crystals and you have an elastomeric tubing. You put pressure on the inside of the tubing to make it expand in diameter but not change in length through a carefully monitored process. Then, while it's in the expanded form, you cool it, and that crystallizes the plastic in its expanded form by adjusting the elastomeric network, the cross-links, to be weaker than the crystals. The crystals will maintain that size, even though the cross-links would like to return it to its original size. When you subsequently heat up that tubing, the crystals melt, the cross-link network takes over, and it returns it to the form in which it was originally cross-linked. That's the process of a heat-recoverable polymeric structure. As I mentioned, the cross-linking is done with the exposure to high-energy ionizing radiation.

Cross-linking can be done with other techniques. You can use a peroxide to cross-link polyethylene. The difficulty with the peroxide process is that you've got to heat it up to activate the peroxide to bring about the cross-links, and that flies in the face of trying to keep a dimensional stable structure. Although it can be done, it's a cumbersome process. Radiation is an elegant way, and a very inexpensive way, of bringing about the cross-linking.

Additional things can be done, however, with radiation. For example, you can graft monomers onto a polymer backbone. Grafting is an elegant scientific accomplishment originally developed in France by [Michel] Magat and [Adolphe] Chapiro. Addie Chapiro is a close friend of mine and Raychem's and has worked with us for years. For example, grafting is a common process that uses radiation as a technique to change surface properties by grafting a monomer onto a polymer structure and creating an external surface of the polymerized monomer having different properties than the polymer backbone. One can provide strength; another can provide a surface adhesive quality that you desire. You can take a Teflon and graft a material on the outside of the Teflon to allow adhesion to take place.

[END OF TAPE, SIDE 4]

COOK: The process of exposing plastic materials and polymer elastomeric materials to radiation is a question of passing the material through a shield into the radiation field, which is at the end of a long vacuum tube. That process technology has developed over many years, and there are lots of trade secrets involved with it—ways of being efficient, ways of making sure that you use as many of the electrons as you possibly can.

The safety concerns that people had in the earliest days have never been a problem. We've never had an accident, never had a radiation over-exposure problem with any of our people. Obviously, everybody is monitored with film badges. There are interlocks on the doors, and the safety alarms, the shut-down monitors and such, have worked elegantly. There's never been a problem. I'm very proud of that. The concerns that many technical people had in the earliest days were unfounded, and we've had a totally safe and outstanding industry record.

The products that result from the technology have become very sophisticated. We started out with relatively simple, flame-retardant, antioxidant-containing polyethylene polymers, whether they be high density or low density, whether they be the ultra high-molecular-weight polyethylenes or even the simplest low-density polyethylenes. That's progressed now to areas where we have multiple layers of polymers, where each layer is irradiated at a different level to obtain a different property. For example, we have tubings with an inner melting layer, an intermediate heat-recoverable layer, and then an outside layer that will have a special adhesive property. It's become a very sophisticated field with very elegant developments. Again, nothing that could not have been anticipated in the first few years, and nothing that we did not anticipate in the first few years, in terms of brainstorming what could be done with this new technology.

Although we continued to develop lots of new products every year, it's not based on a technical capability, in general, that wasn't anticipated as much as fifteen, twenty, twenty-five years ago. How much more is there to go in this field? How much more can be discovered? How much more product innovation can be accomplished? A great deal. It's a field that will continue to grow at more modest rates than the 25 percent per year that our first twenty-five years resulted in, but even so, there's a good 10 or 15 percent per year of additional product capabilities in front of us.

BOHNING: Are there other applications to ionizing radiation that you use besides polymers?

COOK: Yes. None that we at Raychem are interested in. It is certainly true that ionizing radiation can sterilize foods or pharmaceuticals. For example, it is commonly used by Johnson & Johnson to sterilize sutures and things such as that. Food is only now really beginning to go because there's been so much concern about the by-products of the irradiation of food. A lot of people wondered whether or not the indiscriminate chemical reactions that result from the irradiation could produce by-products that could be carcinogens. It obviously would take quite a lot of time to prove that they are or are not. However, the FDA [Food and Drug Administration] has recently approved the irradiation of several different materials, and there are some irradiated foods now that have come to market.

The question is how successful that will be. The purposes of irradiating foods are multiple. They can be sterilized and put in sterile packages, but the most common thing is a relatively low dose to inactivate enzymes and prevent food spoilage for an extended period of time, as opposed to a totally sterile package that would last forever. The irradiation of wheat or other grains to prevent their infestation and spoilage is common. The elimination of salmonella through the irradiation of eggs or pork or whatever is another relatively low-dose procedure. There is a lot of concern, however, over whether the public will buy something that's irradiated. Anybody that provides an irradiated food product has to advertise that it is irradiated, and it may be that there will be enough resistance to irradiated foods. I don't know enough about the recent technology—that is, what's happened in the last twenty or twenty-five years in the irradiation of foods—to be very knowledgeable about it. So I don't even have an opinion on whether it's good, bad, or indifferent. I do know that there's a fair amount of work being done on it.

As I mentioned before, in addition to cross-linking, radiation could be used for grafting. The other big application for radiation in heat-shrinkable materials is what W. R. Grace [& Co.] does for their Cryovac heat-shrinkable food packaging and bubble packaging with heat-shrinkable, clear-sheet material—the big bags or sheets that shrink upon heating. They've made a big business out of that. Our business is something over a billion dollars a year in radiation, and their business is about the same size.

The original work of W. R. Grace was done at Raychem. I knew Brad [Bradley] Dewey [Jr.], the head of Cryovac, who later sold out to W. R. Grace, and his first lieutenant and technical lieutenant Bill [William G.] Baird [Jr.]. They used our first radiation unit to do their original work for making heat-shrinkable plastic packages. We had a close and good relationship for a number of years. Cryovac's business has been a very successful one. We've not been interested in that because that's an entirely different field than what we're in. Fortunately, they haven't been interested in the electrical insulation field, which is primarily what we're involved in. We've both gone in our own directions, although we've shared a common technology for these many, many years. I think they went into business with radiated materials two or three years after we did. It's been an interesting and very successful U.S.–centered technology development.

BOHNING: Do you build your own sources and units?

COOK: No, we do not. We maintain them and modify them and such, but we've always purchased our units from suppliers, the original one being G.E., as I mentioned. Radiation Dynamics is a company in Long Island that struggled and made what's called the Dynamatron. That company has, in the last few years, been bought by a Japanese company, and the Japanese continue to maintain them and we continue to buy our materials from RDI, as it's called—Radiation Dynamics Incorporated.

BOHNING: I can see how you irradiate something on a small scale, but how do you do this when you're involved in large-scale product formation?

COOK: It's not difficult. You just have a transportation system that will take large products in and out of the chamber, and you can do it through a maze. Obviously, radiation passes in a straight line and won't turn corners. All you have to do is create a maze for the product to go in and out of the chamber. It's not a complicated thing. Obviously, the radiation will cover larger areas the farther away from the source you are, so if you've got a really big product, you get a long way away from the source. There's nothing difficult or magical about that. Obviously, there are idiosyncrasies that go along with the field and lots of trade secrets that you develop as you bump into practical problems and solve them, but it's a straightforward, relatively easy practice. The irradiation of air produces ozone, and the ozone has to be properly evacuated. I guess with the concern about the ozone layer, we may have to produce ozone and this is a good way of doing it. [laughter] We've eliminated ozone through scrubbing the air. The ozone in concentrated amounts would be quite harmful.

BOHNING: You expanded overseas very early.

COOK: Yes. We moved to the U.K. and to Germany in about our fifth year in business. We had a major thrust of development in Europe and established ourselves there early on as the dominant company in our field. That's been very successful. For many years, we stayed away from Japan on purpose because of their interest in and dedication to radiation, but then decided that we'd better be there to see what they're doing, and to make sure that we participated in any rapid growth and especially profitable markets. That's worked well.

I think that's probably good enough for a session. What I've forgotten, I don't know, but we can take a look at what the product is and then figure out how we go from there.

BOHNING: I appreciate your taking your time this morning to talk with me.

[END OF TAPE, SIDE 5]

[END OF INTERVIEW]

NOTES

1. Paul M. Cook, “Apparatus and Process for the Production of Photosynthetic Microorganisms, Particularly Algae,” U.S. Patent 2,658,310, issued 10 November 1953 (application filed 22 December 1950).

2. Paul M. Cook, “Chemical Engineering Problems in Large Scale Culture of Algae,” *Industrial and Engineering Chemistry*, 43 (1951): 2385-2389.

3. Paul M. Cook, James B. Meikle, and Bruce Graham (to W. R. Grace & Co.), “Improved Polyethylene by Irradiation and Heat Treatment,” U.S. Patent 2,960,453, issued 15 November 1960.

   Paul M. Cook, James B. Meikle, and Bruce Graham (to W. R. Grace & Co.), “Irradiation of Linear Polyolefins,” U.S. Patent 3,006,829, issued 31 October 1961.

4. William Taylor, “The Business of Innovation: An Interview with Paul Cook,” *Harvard Business Review* (March-April 1990): 97-106.

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