

CHEMICAL HERITAGE FOUNDATION

KENNETH E. MANCHESTER

Transcript of an Interview  
Conducted by

Christophe Lécuyer

at

New Hope, Pennsylvania

on

13 October 2004

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION  
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## KENNETH E. MANCHESTER

1925 Born in Winona, Minnesota on 22 March

### Education

1949 A.B., chemistry, San Jose State College  
1950 M.S., chemistry, Stanford University  
1955 Ph.D., thermochemistry, Stanford University

### Professional Experience

1952-1955 Stanford University, Stanford, California  
Fellow in Chemistry

1955-1962 Shell Development Company, Emeryville, California  
Surface Chemist

1962-1963 Sprague Electric Company, North Adams, Massachusetts  
Section Head of Semiconductor Chemistry

1963-1969 Department Head of Semiconductor Chemistry

1969-1976 Director of Semiconductor Research and Development

1976-1979 Chief Scientist

1979-1985 Director of Semiconductor Quality Assurance and Reliability

1985-1989 Corporate Vice President, Research Development and Engineering

1989-1996 Allegro Microsystems, Incorporated, Worcester, Massachusetts  
Consultant

### Honors

1948 Scholarship award, top ten highest grade point averages at San Jose State College

1949 Member, Key Club of Phi Beta Kappa Faculty Club, San Jose State College

1961 Member, Optimist Club of Concord, California

1984 Sprague-Worcester Major Achievement Award, Sprague Electric Company

1984 Employee of the Year, Sprague Electric Company

1985 Commendation from Vice President of Integrated Circuits Operation, Sprague Electric Company

1985 Sprague Fellow, Sprague Electric Company

1988 Biographical record, *Electronics Buyers News*, September edition  
1993 Biographical record, *Who's Who in the World*

## ABSTRACT

Kenneth E. Manchester was drafted after high school to serve in World War II and later attended San Jose State University. Manchester then completed a Ph.D. in chemistry at Stanford University under the supervision of George S. Parks and worked as a postdoctoral fellow under Eric Hutchinson for three years. Manchester joined the surface chemistry group at Shell Development Company in 1955 and moved to Sprague Electric Company in 1962. At Sprague, he directed a research group that pioneered in the development of ion implantation, a key process in the manufacture of integrated circuits. Manchester later headed semiconductor research at Sprague before serving as director of quality assurance and reliability. He concludes the interview with thoughts on the need for chemists in semiconductor development.

## INTERVIEWER

Christophe Lécuyer is Research Historian at the Chemical Heritage Foundation. He holds a Ph.D. in History from Stanford University. He has published extensively on manufacturing districts, university-industry relations, and the history of electronics and scientific instrumentation. He was a fellow of the Dibner Institute for the History of Science and Technology and taught at MIT, Stanford University, and the University of Virginia.

## TABLE OF CONTENTS

1	Education and Military Service Childhood. Influence of early teachers. Military service. Marriage. College education at Santa Clara University. Graduate work at Stanford University. Eric Hutchinson. Innovation in equipment design.
12	Shell Development Company Surface chemistry group. Instrumentation.
14	Sprague Electric Company Semiconductor development. Ion implantation. Collaboration with Oak Ridge National Laboratory. Ion implantation system. Collaboration with MOSTEK Corporation. Operating through labor strike. Kurt Lehovec. Complementary metal oxide semiconductor. Hall sensor. Relationship between Sprague Electric Company and MOSTEK Corporation. Implanted resistor.
40	Sprague Electric Company at Worcester, Massachusetts Quality assurance. Relationship to IBM Corporation. Hall element patent.
42	Conclusion Importance of chemical education in semiconductor work.
47	Notes
48	Index

INTERVIEWEE: Kenneth E. Manchester  
INTERVIEWER: Christophe Lécuyer  
LOCATION: New Hope, Pennsylvania  
DATE: 13 October 2004

LÉCUYER: This is an interview with Ken Manchester, taking place in New Hope, Pennsylvania, on 13 October 2004. In general, it is a discussion of his life history and the history of ion implantation. Let's begin by discussing your background, your family and the place where you grew up.

MANCHESTER: I was born in Winona, Minnesota on 22 March 1925 to Lawrence Edwin and Daisy Idel, whose maiden name was Finley. I spent my childhood in Winona, and was drafted into the service very soon after I finished high school, which more or less ended the time that I lived in Minnesota.

My father and mother came from a farming community in Chatfield, Minnesota. My father didn't finish the sixth grade because it was necessary for him to work. My mother became a teacher in the country schools. She taught the first through twelfth grades, which were held in a one-room schoolhouse. After they married, they decided it was best to move out of the farming country and proceed with their lives in Winona. Therefore, they moved to Winona, and luckily my father got a job driving streetcars with the Mississippi Valley Public Service Company, the old electric streetcars. He remained with them through their conversion to buses until he retired.

My mother became a housewife after my parents moved to Winona. She was very active in the church, and was a diligent member of the WCTU [Women's Christian Temperance Union]. When they retired, my parents opened a resort in Bemidji, Minnesota, which they ran until several years before my father died. He passed away on 1 August 1974 and my mother passed away on 28 January 1992.

LÉCUYER: Did you have any siblings?

MANCHESTER: No. I am an only child, and I'm not sure if that's good or bad. After I got out of the service, married, and finished college, I enjoyed the time that I spent in Winona with my parents—a week or so at vacation time.



LÉCUYER: Could you tell us about Winona in the 1920s and 1930s?

MANCHESTER: Winona was a small farming community right on the Mississippi [River]. It had a population of about twenty-one thousand, which remained very constant. The only large businesses that supported the community were the [J. R.] Watkins Company, which was also headquartered there; Peerless Chain [Company], headquartered there; and Bay State Milling Company. Because the Watkins plant was there, the community had had a sense of importance. It was a huge operation, and they built a very nice building which now contains a museum. I think the company still exists, but is a very small operation.

LÉCUYER: What was made at Watkins?

MANCHESTER: They produced just about everything, though seasonings were their primary product. They distributed seasonings throughout the country. I can't tell you much more about it because I was a bit young at the time, but it served as an important point in Winona.

LÉCUYER: Did you attend elementary school and high school in Winona?

MANCHESTER: Yes. I attended first through sixth grade in the local school, which is still there. We had a junior high and a senior high at that time.

I was not a very good student in junior high, but one of my science teachers, Sandy Kerwin, really turned me around in my last year. He showed me the importance of doing things properly and guided my learning method for later life, and I owe him for that. Unfortunately, several years later, I found that he had drowned in the local pool. I'm not sure if it was accidental, but that was a loss.

LÉCUYER: Did he teach math or physics?

MANCHESTER: He taught what we called basic science, which covered all aspects of science: a little bit of physics, a little chemistry, et cetera. He was an excellent teacher, and had developed techniques to help his students better understand scientific concepts. I remained interested in science throughout high school because of his influence, though I had some other excellent teachers as well. One of them, Mr. [William G.] Zilliox, was an excellent teacher I had in senior high, who taught both physics and chemistry in the junior and senior years. He also encouraged my interest in science, and had a son with whom I went to school.

Unfortunately, we lost him early in the war [World War II], so that was a sore point. My graduating class had about three hundred students.

LÉCUYER: It seems to have been a very large high school.

MANCHESTER: Yes. It served Winona and rural areas up to 25 miles away, and had a pretty elaborate busing program. Looking back, it was an excellent school, as there seemed to have been a rapport between teacher and student. I really enjoyed lessons from Mr. [Harvey] Gordon, the shop teacher, who taught me much about the mechanics of life, living, and carpentry and metalworking. That has helped me a lot in what I have done.

LÉCUYER: Did you also take courses in English Composition and History?

MANCHESTER: Yes. Mr. Robert [G.] Pendleton, the coach for our swimming team, of which I was a five-year member starting as a freshman, was my English teacher. I took a number of classes from him. He developed for me the basics of the English language and an appreciation for literature, which has helped throughout my life.

I wasn't a good student of history, but Mr. Edward [M.] Davis took an interest in me and prepared for me the way to a scholarship to Wabash College, for which I am thankful. I never had a chance to use it, however, because I was drafted as soon as I got out of high school. I was drafted the day after my eighteenth birthday.

LÉCUYER: What subject did the college scholarship cover?

MANCHESTER: To pursue my interest in chemistry, physics, and math. I looked forward to going, but I didn't have that opportunity. But I did manage to graduate in the upper 10 percent of my class, which is somewhat of an accomplishment.

LÉCUYER: Was it clear in high school that you were bound for college? Did your family want you to go to college?

MANCHESTER: Yes. My father and my mother were determined that I get the necessary education, which they really never had. My mother was a teacher, and had a year-long preparatory course at a teachers college. My father never had a chance to continue, though I think that if he could have continued his education, he would have done much more. For me,

the force to get an education was always there, despite the detour of the war. I got out before I was twenty-one, and found that that experience is really what matured me.

LÉCUYER: When were you drafted?

MANCHESTER: I was drafted to a basic training session in Camp Abbot, Oregon in March, 1943. I had graded well on initial testing and was sent to the Army Specialized Training Program [ASTP] for more schooling. I spent one quarter at the University of Santa Clara in California before they disbanded the program. I did well in all my subjects except for history, which I failed. The brother who taught that course talked in a monotone and I found it easy to go to sleep. The program was a good experience. From there I was assigned, in preparation for deployment, to the 11<sup>th</sup> Armored Division in Lompoc, California.

LÉCUYER: Was the ASTP an officers' training program?

MANCHESTER: No. It was to provide an engineering education. Although it wasn't introduced to the individual that way, it eventually would have led to a commission in the army. I ended up in engineering anyway, and was assigned to the 56<sup>th</sup> Armored Engineer Company in the 11<sup>th</sup> Armored Division.

LÉCUYER: The idea was that you would go to [the University of] Santa Clara for two years or so, and then—

MANCHESTER: It was a four-year course. Yes, we would come out with a degree, and I'm not sure what would happen after that, because they disbanded the program.

LÉCUYER: Was it your first stay in California?

MANCHESTER: Yes.

LÉCUYER: Did you like it there?

MANCHESTER: Yes. I met my wife-to-be at a San Jose State College function while I was at Santa Clara University. I commuted back and forth several hundred miles on the weekends from the division at Santa Maria to San Jose. At that time I hitchhiked; there wasn't any other

connection that I could make between the two. I spent a lot of time on the road. Once she got a ride with friends and came down to see me and we spent a weekend together.

LÉCUYER: Was she from the Santa Clara Valley?

MANCHESTER: She was originally from Oklahoma, and after high school she worked in Oklahoma City at Tinker Air Force Base where one of her supervisors recommended that she try to get into college. Both parents were deceased, so she moved to be with her sister and spent a year at Bakersfield Junior College in Bakersfield, California. Because she had a real inclination towards the arts, one of her professors suggested that she try to get into San Jose State.

She met another young lady when she moved to San Jose and they got an apartment together. She started in the art department at San Jose State. She was in her freshman year when I met her, but after we met and really grew to enjoy one another, my division was shipped overseas. That was good in some respects. Well, that was the start of my understanding the ups and downs of what goes on in the world.

I was in the Battle of the Bulge and at the Remagen Bridge takeover, and I then traveled across Germany and arrived in Linz, Austria. The war had ended at that point. We had uncovered some of the atrocities that the Jews had suffered there in concentration camps, and I learned about man's inhumanity to man. [Begins to cry] You'll have to excuse me.

[BRIEF SILENCE]

LÉCUYER: Would you like to talk more about your war experiences, or would you like to move on to some other subject?

MANCHESTER: I'll move on. Serving in the armed forces matured me in a hurry, but I still have difficulty when I think about it.

LÉCUYER: Of course. Did you move back to San Jose in 1946?

MANCHESTER: Yes. After I was discharged in Fort McCoy, Wisconsin, I went home and stayed about three or four weeks, and then I went on to California. I married my wife in June of 1946, and started in San Jose State College. I said, "While I'm waiting for her to finish up there, I'll start, and then I can transfer if I need to."

I started in the fall of 1946. San Jose State [College] at that time was a small school, with a student body of about twelve hundred, and there was a close relationship between the professors and the students. It was far different than what I find now, in that we, the students, used to go out sometimes on weekends with some of the chemistry professors. We'd get together and sit around a fire, talking about just about anything. It eventually got around to chemical subjects. That kind of relationship between professor and student was a normal thing in the chemistry department. That's the better part of education. Enrolling at San Jose State was one of the best choices that I've made.

LÉCUYER: Did the G.I. Bill finance your studies?

MANCHESTER: Yes. I also worked in the construction industry to support myself. I tried to continue that because I didn't want to waste my G.I. Bill on the fees for San Jose State. My tuition was seven dollars a quarter. The cost of books was the major expense. I did apply the G. I. Bill, which allowed about five hundred dollars a year or something like that for school expenses.

LÉCUYER: Did you work at the same time?

MANCHESTER: I worked part-time in construction through my second year. After I had taken a physics course from Wilbur [H.] Moreland, whom I really enjoyed, I started working with him as a reader, correcting exams. Gradually, I got out of the construction business. We also got a small supplement from the government that allowed me to go at a more rapid pace than I normally would have.

LÉCUYER: How did you choose chemistry?

MANCHESTER: It was something that I wanted to do after I finished high school chemistry.

[END OF TAPE, SIDE 1]

MANCHESTER: After completing physical chemistry, I felt that my path was chosen. I think most of that was due to the fact that I had excellent professors, not only as teachers, but as friends. That was the turning point for my deciding to stay in that field. Dr. Benjamin Naylor and another professor named Dr. Albert Schmoldt also were the ones that steered me toward Stanford [University]. Dr. Schmoldt was a product of Stanford, as was Naylor. Dr. Schmoldt had a place out in Alum Rock where he grew and experimented with fruit trees. He had all

kinds of oranges coming out of one tree and such. He invited us to spend a day at his place, which was really interesting. Most of the individuals who had chemistry backgrounds from San Jose [State College] ended up going to Stanford. There seemed to be a tunnel going from one to the other. Of course, the trip to Stanford from San Jose was not that difficult to make. I had a close neighbor who taught in the Palo Alto area, and we would ride group together. He'd drive one week and I'd drive the next, and so that worked out very nicely.

LÉCUYER: Before we move on to Stanford, could we talk about the chemistry department at San Jose State? How big was it? Was it doing any research?

MANCHESTER: At that time there were six professors in chemistry, who covered organic, inorganic, analytical, and physical chemistry. They were all excellent. Our analytical professor was a woman, Dr. Gertrude Witherspoon. She generated an environment in which accuracy was paramount, and I enjoyed that. She left sometime after I had graduated, but the other gentlemen stayed with the college until they retired or, unfortunately, passed away.

As you're probably well aware, San Jose State is now a fairly large university. In fact, it upset me a bit that it's entirely different. The covered quad is gone. The only things that are left are the tower, the old chemistry building, and the old gym. Now, the chemistry building no longer is totally chemistry. It's dance and art, and it's entirely changed inside. The exterior looks the same, but the interior has been all changed and remodeled.

In the tower was where Tau Delta Phi, who was well known for its plank sticking out of the upper window of the tower, held the initiations. Everyone who was initiated thought that they had to walk the plank up there. It still looks like it did before, but the rest of the campus has changed.

In San Jose I lived in a small apartment with my wife on Ninth Street, which was a few blocks from the campus. Since then, the campus has really expanded. The college now extends past Ninth Street and all the houses that were there are gone. I would have liked to have sat in on one of their chemistry classes, but I didn't have the time to do that.

I wish the same sort of intimacy existed today as there was when I attended San Jose State. I looked into one of the large rooms which used to be one of the large chemistry rooms for the first-year chemistry, an amphitheater. There were three large TV sets for presenting lectures on the walls. I think things have changed a great deal.

LÉCUYER: There must be at least ten thousand students there, maybe even twenty thousand.

MANCHESTER: I thought it was somewhere around fifteen or eighteen thousand. It's a huge place.

LÉCUYER: How many students were in chemistry then when you were there?

MANCHESTER: On the order of twenty-five to thirty with a chemistry major.

LÉCUYER: Did you meet Gordon [E.] Moore at San Jose State? I think he studied chemistry around the same time.

MANCHESTER: I have never met Gordon Moore, although I've heard of him and I've seen pictures of him.

LÉCUYER: He studied chemistry at San Jose State for two years. That must have been just before you went there.

MANCHESTER: Yes. I was there from 1946 to 1949. He preceded me by a bit.

LÉCUYER: Were you involved in extracurricular activities when you were there?

MANCHESTER: I worked to support myself and my wife, so I wasn't involved in many extracurricular activities, but I belonged to a couple of fraternities. I was elected to Tau Delta Phi, the honorary scholastic fraternity, and Phi Epsilon Pi, the science service fraternity.

LÉCUYER: When did you move to Stanford?

MANCHESTER: I moved after I graduated in the spring of 1949. I enrolled the following fall in Stanford and took the qualifying exams in the chemistry group the second quarter. I passed three and was marginal on the fourth. The results said that I could go on for a master's, but that I couldn't go on for the doctorate, and I said, "Well, I'll just take them again," because I planned on going as far as possible. I took them the following quarter and passed all of them. I still have copies of them. They were interesting exams. I'm not sure if you had those in the history department or not.

LÉCUYER: Yes. I took them during my third year.

MANCHESTER: There were two experiences that are indelible in my mind. One was the qualifying exams, and the other was the oral exam that we had to take at the end of our studies, which you had to pass before you could graduate.

I got my master's and started in the Ph.D. program in 1950. I wrote my dissertation by 1952. I was asked to help with a program before then, so I was given a postdoc position, even though I hadn't got my Ph.D. officially. I stayed on with Dr. [Eric] Hutchinson, who was a key person in my life, for three years. I thought, "Well, I can't be a perpetual postdoc. I'd better take my exams and get out of here." So I did.

In the chemistry department, the oral exam was given by four professors, one of which was my thesis professor, and three that were picked at random. One of them was a physicist, and the other two were from the chemistry department. I had Richard [A.] Ogg [Jr.]. He was one of the geniuses around the campus and very eccentric. He took his life—drank cyanide, but he was brilliant. He was the last man in the university I wanted to have on my orals committee, and gave me the most trouble. He asked me some question about the PV law, and so I drew a curve. He said, "Is it curved like that up on top?" "No," I said, "That's my normal drawing. That's a straight line." "Well," he said, "Let's make it a straight line." We spent half an hour getting that line adjusted.

LÉCUYER: My gosh.

MANCHESTER: Talk about unnerving. After I walked out of that exam, I thought that I was going to have problems, that I wasn't going to make it. But, thank God, I made it. I got his vote, I guess. Those are the indelible moments. [laughter]

LÉCUYER: Who was your advisor?

MANCHESTER: Professor George [S.] Parks. He was very prominent in thermochemistry, and he was the one who interested me in the research program that I became involved in, which was measuring heats of solution. I studied under him for both my master's and Ph.D. For my master's, I worked on heats of solution of some sugars, using a piece of equipment that was generated by a previous student. I revised the design and enhanced its accuracy.

I worked on heats of combustion for my Ph.D. That was an interesting problem because Dr. Parks had said, "There is a series of compounds that we really need to know the heats of formation of to fit within a chain of the similar compounds with different carbon chains." "But," he continued, "nobody has been able to do the measurements," because it was "impossible to accurately know how much of the compound you will be using in your combustion measurements." They all hydrated rapidly or were very volatile. At that time the method of



doing heats of combustion was to fill a weighted platinum crucible with the material you're going to combust, and then weigh it again. Most of these solids had very, very low vapor pressures, so you couldn't get an accurate weight and know what the weight was at the time of ignition.

For my dissertation, I designed equipment which allowed me to handle the material in the absence of moisture and contain alcohols so that there was no loss due to vapor pressure. I devised a crucible capped with a combustible plug and the fuse that normally is used to ignite the samples. These samples could be removed from the equipment and weighted on a microbalance. That allowed me to do the five compounds with very good precision and accuracy. I guess my ability to design the crucible relates to the training that I got in high school with Mr. Gordon. He was very good at encouraging me to get to the root of a problem.

LÉCUYER: What did you do for your postdoc?

MANCHESTER: I worked with Dr. Hutchinson. He was trying to measure the heats of adsorption of water on some of the clay systems. The heats were extremely small, and we couldn't measure them accurately and precisely. I designed a semi micro-calorimeter which had very good accuracy and also a way of introducing the clays to the water. It was a small evacuated bulb system contained in the calorimeter. At the correct time, we could break the bulb to release the powder, and after the water adsorbed on the surfaces, we could measure the heat involved.

It took me three years to build the calorimeter. I designed, machined, and assembled the calorimeter in a machine shop, and it worked like a jewel. I'm very proud of that. I think one of the individuals that followed me used it for some of his thesis work, and redesigned some of the electrical measuring systems. It wasn't a linear bridge, but once it was calibrated it was a precise bridge. To my knowledge, it still may be at Stanford. My postdoc work was primarily measuring the heats of adsorption of some clays and helping some of his students who were working there also.

LÉCUYER: How was that project financed? Did Dr. Hutchinson have a contract?

MANCHESTER: Yes, he had a contract funded by Universal Oil [Products Company]. Universal Oil asked me to give them a seminar on the program at their lab near the La Brea Tar Pits. I think what they were interested in was the capture of oil by desorption of oil from clays.

LÉCUYER: How did you finance your master's and then your doctorate?

MANCHESTER: My master's and doctorate were primarily financed by my G. I. Bill. I supplemented that with income as a graduate assistant.

LÉCUYER: Could we talk a little more about the chemistry department in the mid-1950s?

MANCHESTER: The chemistry department was pretty good. Dr. [Harry S.] Mosher was there, as were Dr. Ogg, Dr. Parks, [Richard H.] Eastman and [Frederick O.] Koenig. I took a course from Dr. Koenig, a very intelligent man, who was deep into thermodynamics at the time. He was one of the people who gave you just enough [information] so you didn't have the final solution. You had to work to the final solution, which I think is a great approach.

Dr. [Harold S.] Johnston taught chemical kinetics. He introduced equations describing a reaction and integrated or differentiated internal terms as he arrived at a final solution. As a student taking notes, that was a little bit difficult to follow, but he was good. The department was very good.

LÉCUYER: It was a stimulating place.

MANCHESTER: I think it went downhill a bit after I left, but presently it's in good shape.

LÉCUYER: There was a big change in the department in 1960 when [Carl] Djerassi joined it.

MANCHESTER: I went back just after the earthquake destroyed a lot of the campus [1989].

[END OF TAPE, SIDE 2]

MANCHESTER: I saw Dr. Eastman and Dr. Mosher, but they are now gone. The old red stone chemistry building was still there. My place of work, or really, where I lived, was still there; the back building, down in the basement. It was an excellent place for thermochemical work. The temperature, because of the stone surroundings and the depth, didn't vary a half degree over a day.

When I first got there, I found wine bottles in the cabinets in the equipment room. I determined that whoever was there before me had set up a still and was distilling alcohol. [laughter] That was my introduction to my place down there. I had good equipment and the availability of the machine shop. If I couldn't buy the things I needed, I'd make them.

LÉCUYER: Could you speak about your peers, the students who were getting master's and Ph.D. degrees at the same time?

MANCHESTER: One individual who got his Ph.D. also worked in thermochemistry. In fact, he used much of the equipment that I had designed and built for handling volatile products. He worked with Dr. [Stuart W.] Grinnell, one of the professors who taught Electrical Aids to Chemistry. Leland [W.] Vaughn, the gentleman who was with me, worked with him. Unfortunately, he had some medical problems that really hindered his productivity.

One of the fellows who came from San Jose with me, Richard [L.] Perrine, worked with Dr. H. Johnston and took on a professorship at the University of Southern Cal [California]. A young lady who was with us was also an associate professor at the University of Cal after she graduated. I think the number of graduate students in the chemistry department at that time was probably on the order of ten to fifteen.

LÉCUYER: Very small.

MANCHESTER: The only time I interfaced with them was during coursework, but Leland worked close to me as well as Dick Perrine. Dick lived in Sunnyvale [California], and he and I rode pool together to Stanford. He was a student of Johnston.

LÉCUYER: Any memories of Stanford, the institution?

MANCHESTER: I always recall the chapel: from the first time I arrived to the time I left, it was the first thing I saw when I went through the main gate. Most of the chemical activities were in the stone building: Dick Eastman and Harry Mosher had offices in the temporary building out back, but the analytical group under Dr. Eastman was in the old stone building as were Parks and Koenig. I don't think Ogg had any students. He was a strange individual.

I am amazed at how things have changed. The old chemistry building is gone and a number of high-rises have been built. The department has expanded quite a bit and there are a lot more graduate students. I didn't become too familiar with the rest of the campus. The only time I was in that part was when I took my language exams. It was an interesting experience to get through the French exam, especially for one who had never taken French.

LÉCUYER: How did you pass it?

MANCHESTER: I read seven pages in Doyle's *Chemical French*. [laughter] I was asked to decipher a section from a French journal. I lucked out, because I got through all right.

LÉCUYER: You moved to Shell [Development Company] in 1955, right?

MANCHESTER: Dr. Fred [M.] Fowkes, who was with Shell, saw Dr. Hutchinson. They were both surface chemists and I think he was a pretty close friend to Hutchinson. Eric brought him down in the lab to meet me, and we talked for quite some time.

Three months later, I got an invitation to visit Shell to see if I was interested in joining the company. At the time Fred was a supervisor in the surface chemistry group, who reported to [J.] Norton Wilson. Ken [Kenneth] Detling was the assistant department head and Wilson was the department head. That group reported to Dr. Tamalee.

It looked like an interesting position—they wanted me to get involved in the grease field. I took the job. Web [Webster] Sawyer, Geze Ronet, Nate [Nathan C.] May, and I made up the surface chemistry group.

Initially, I spent a good part of my time cloning the calorimeter that I made at Stanford. I worked as an advisor with their instrument department, but I didn't do any of the hands-on work. It took the better part of a year and a half to build a model of the unit. They tried to automate as much of it as possible, with which there were costs and benefits, but we worked out the difficulties. It worked fairly well, except that their method of measuring the calibrating heat input had a fair amount of error because of switching times. I changed some of its electrical parts.

Most of the work that I did at Shell was on grease systems. They were trying to develop a clay-based grease and were concerned about a patent which said that in the Montmorillonite systems, certain amides bonded nicely to clay surfaces and could form a composite mass that had good lubricating properties. The patent said that there was a chemical bond between the amide system and the clays. At Shell, they were concerned about that patent, because they had found an amide clay system which gave them a very good grease.

They asked me to determine whether it was a chemical bond or a physical bond in the system that united the amine with the clay. I finally devised a piston-type sample container for the calorimeter from which I could inject part of the amide contained within it by depressing the piston with a rod. This allowed me to enter known increments of the amine, which was a liquid phase, into a given concentration of clay. The rod was attached to the calorimeter stirrer which passed through the top. I calibrated the displacement to measure exactly how much I was introducing. The system worked very nicely, because once you injected some of the amine, the system was self-sealing. There was not a continuous reaction.

I measured the heats involved, and showed that it was a good chemical system. I plotted the heats of absorption as a function of the amount of amide introduced, and proved that Shell would violate that patent. I wanted to publish that data. I thought it was a good piece of work, but [Shell] would not allow it because of the situation.

I didn't do a lot of work in the foam system. Web Sawyer, Geze Ronay, Nate May and I were involved in that. I designed a system to measure the rate of restoration of the foam after a depression, which was a critical property. I devised and constructed a system that allowed us to do that.

I left Shell for two reasons. The first reason was that I enjoy cooperating and working together with people. After I finished work on the grease system, they were going to transfer me to another department. I objected to that. I said, "If they've got a problem, they should talk to me about what the problem is, so that I know what's going on. If I can be of any help, I'll be glad to do that." I felt that I was now a small number in a big organization of numbers, and I was sure the individual on top didn't know beans about me and simply shifted me about. Being a young, maybe headstrong individual, I objected to that.

Secondly, during the problem with the clay based grease, there was a portion of that research for which I needed some other equipment. I went back to the university [Stanford University] to use the equipment that I had designed and built, commuting back and forth. I developed most of the information I needed, and then I was told, "We're not interested in that. You shouldn't have done that in the first place." That didn't fit very well.

Eric Hutchinson, whom I'd worked for and knew very well, consulted for the Sprague Electric Company. They were primarily a capacitor company and were trying to get into the semiconductor area. He suggested that they needed a good chemist, that he had someone to recommend for the position, and told them about me. John [L.] Sprague came to California, and we spent a couple of days together, talking.

He invited me back so that I could meet the people and talk about some of the problems. When I did, I was very enthused because there was a whole area that nobody was watching over. I thought, "Gee, what a great way to go." I talked to John, his coworkers, and Mr. Robert C. Sprague himself, who at that time was the chief executive officer. The people that worked in the area of capacitors all said that, yes, they really had the freedom to do what they wanted, which was what I and anyone else would have liked to hear. As a result, there was a lot of really good research going on.

I decided to join them; it wasn't difficult for me to make that decision after what had just happened at Shell. I went home and my wife and I decided that I would begin at Sprague. She always has supported what I wanted to do, which is good. We packed up, put our house on the market and moved back to Massachusetts.

LÉCUYER: Why was Eric Hutchinson consulting with Sprague? What was the connection between the two?

MANCHESTER: Eric was consulting with them through his connection with John Sprague who went to Stanford too. John took his Ph.D. there with [Claudio Alvarez] Tostado.

LÉCUYER: Would you like to talk about John Sprague's approach?

MANCHESTER: I think John and I have always got along pretty well. I sometimes disagreed with him, but we agreed to disagree, so it worked out. We're still friends after all these years and see each other on occasion. I have some property in Williamstown I have to check on, so I stop in at North Adams [Sprague Electric Company research and development facility, Massachusetts] to talk with John. He's an interesting individual and an intelligent young man despite the fact that I have had some problems with his administrative capacities. I first really got acquainted with him when he came out to California.

John had just joined the company, too. He found that he couldn't get a job in anyone's semiconductor company after graduation because his name was Sprague so he went back and joined Sprague Electric Company. At the time, his position was somewhat undefined, as he took over as director of research after Preston Robinson passed away. When I originally joined Sprague, I reported directly to John, and our communication lines were pretty good.

About a year later, John was elected vice president of research and development. He brought in Fred Fowkes from Shell Development Company as director of research and development and F. Lincoln Vogel was brought in as head of the semiconductor research department. In September 1963 Vogel was appointed associate director of research with responsibility for the semiconductor research because we needed to look very carefully at getting into the semiconductor field. Originally they had purchased the process for making germanium electrochemical transistors from Philco [Corporation], which was installed in the Concord, New Hampshire plant, so they were in that part of the business already.

[END OF TAPE, SIDE 3]

MANCHESTER: Sprague also wanted to get into silicon-based transistors, so when Linc [Lincoln Vogel] came, they established a group to produce integrated circuits while looking at the development of a planar process for transistors. The planar process was known at that time, but Sprague had no experience in it whatsoever. The germanium system didn't help them in that area.

First, they converted a part of the basement of the Research Center, which opened in 1962, into a semiconductor processing area. We built our own furnaces and bought vacuum systems that were commercially available. We started out trying to produce the old Texas Instruments 51 Series [integrated circuit], which had two big capacitors on it, a couple of transistors, and a resistor. That was late 1962 or early 1963.

LÉCUYER: Had Sprague made planar silicon devices before?

MANCHESTER: No, they made germanium transistors. The IC [integrated circuit] processing coincided very nicely with work on an individual transistor because the processes were very similar: we were developing transistors for an integrated circuit. At that time, the collector region was made by diffusion. Producing the deep region took something like seven days. There was no epi [epitaxy] at that time.

LÉCUYER: Oh, my goodness.

MANCHESTER: We started out with the bulk silicon at high resistivity, and diffused many days in order to form a deep PN junction and get the collector resistance down. Producing a circuit took several weeks, and lot sizes were small. I was involved because a lot of the problems were related to the surface conditions, which were determined on the ability to grow an oxide and prevent diffusion through it.

I spent a lot of time evaluating surface conditions and getting to the point where we could grow a reasonable oxide which wasn't full of holes. By the way, it took over a year before we got our first operating circuit. Not to specification, but operating. [laughter] There was a big celebration at that time.

LÉCUYER: If you go from scratch to a circuit in one year, it's very fast, right?

MANCHESTER: We didn't think so. The main problem was that we couldn't grow a good oxide because we didn't have good surfaces. There was also airborne contamination. It was not visible, but, boy, it was to our process.

I finally got through the oxide problem by using a double resist process. In other words, I oxidized and then put on a photoresist by partially etching the pattern, removing the resist, then cleaning and laying down a new resist layer. Any unwanted partially etched holes that developed in the first etch, because of problems with the resist adhering or because of contamination that developed in the oxide, were covered up with the new photoresist layer. The probability of another one appearing in the same place was pretty low.

LÉCUYER: Who was involved in that project?

MANCHESTER: There were six to seven people involved, because we were covering the whole gamut, from oxide growth to photoresist preparation masking and diffusion. In fact, we started growing our own crystals until the crystal growers outdid us.

LÉCUYER: Were the other members of the group chemists as well, or were they physicists?

MANCHESTER: Most of them were electrical engineers and solid state physicists. In the late part of 1962, I hired one chemist, Paul E. Roughan, who was involved in that area for most of the time. He got his doctorate from Iowa State [University].

At that time, epi was starting to make its mark, and a group of us went to Bell [Telephone Laboratories, Inc.]. We had a good relationship with Bell because of the capacitor contacts that we had with them. We also had good access to their semiconductor area. Mr. [R. C.] Sprague called them and said, "The stuff that the guys call epi, whatever it is, can you show us something about it?" We spent two or three days at Bell, learning a great deal about epitaxial growth, and came back and set up our own epi system. Controlling all of the variables in that process was horrendous at the time, and now you don't even think about it.

The epi immediately solved many of the problems in the planar transistor process. It also solved a great problem in the ICs, since we didn't have to do deep collector diffusion any longer. We installed the process and used that system to develop a process for NPN transistors. We also purchased commercial furnaces and put in a complete processing area in Concord [New Hampshire Sprague Electric Company manufacturing facility] that took care of the NPN transistors. They became pretty efficient at it, and developed a good product line.

Of course, then the request came in, "We need PNP's, too." That's a whole 'nother basket. We spent some time developing and controlling that process. The phosphorus diffusion for the bases was difficult, but we did get a process that produced saleable product though it didn't yield much.

LÉCUYER: Would that have been in 1964?

MANCHESTER: Yes. That was in 1964, 1965, when I started my work in implantation. The initial push [for implantation] was related to the problems they were having with phosphorus diffusion for the PNP transistors. It started with my desire to find a way to introduce dopants accurately enough and precisely enough to get reproducible diffusion across units.



About that time, a number of systems were devised for electron beam evaporation. National Research Corporation in Cambridge [Massachusetts] had developed a system which was not a magnetically controlled system for large beam but an electron beam, so that one could focus thus and get a very high power density. Initially, I contacted Cliff [Clifford] Sibley, who was working on that process. He showed me the system, and we bought one of them.

They finished it after a couple of months, and soon we had a focus beam and also the possibility of deflecting the beam. We thought about using a number of different materials and deflecting from pot to pot. I said, "Jeez, I wonder if it might be possible to deposit a film of boron-containing material on the surface of silicon, and then drive it in with the electron beam." It might have been more reproducible than what we did in diffusion, for instance.

I talked to Cliff [Sibley] about that idea. He said, "Maybe we can adjust this beam so that we can put deposits in different places on the surface. Let's check and see what's going on." Well, that little beam had a lot of power, but we couldn't, without losing the focus capabilities, reduce the power of the beam to a point where we didn't start melting the silicon. Cliff said, "Let's give that a try. Let's see if we can change the mechanics of the focus system to work at lower energies."

I think it was papers from the Isotope Conference that I happened to be looking through which talked about separation of ions and how far these ions would penetrate into substances. I said, "What if we changed those electrons in that system to ions? Perhaps we can do the same thing that these people with their accelerators are doing, only we're going to do it in silicon." It was not an original idea, because [William J.] King from Ion Physics [Corporation] was trying to do some implants with a high-voltage accelerator, but we thought about that independently.

Cliff knew Leon [O.] Love, department head of the separator group in Oak Ridge [National Laboratory, Tennessee]. We said, "That's a source of ions. Let's see if we can modify that equipment to handle ions very quickly. We've got to learn a lot before we can do that." [Cliff] said, "Let's see if it works as far as getting the ions into the semiconductors and check the sort of control that we have."

He called Leon, who invited us to Oak Ridge. Leon said, "That's a great idea." "Yes, I'll assign one of the accelerators, and one of the isotope accelerators to the program." I don't know if you've seen an accelerator—it's monstrous, but it can run amps of beam current.

Leon assigned Gerald [D.] Alton, a young fellow who was a graduate student at the time, to work with us. We talked to Gerald [Alton] and after checking the separators we thought, "Those currents are way too high for what we are doing." Gerald replied, "We can control that so we can get it down. What do you want?" "We want microamps." That kind of held him a bit, but he said, "We'll give it a try."

We had to design wafer holders to fit in the system so that we could try to implant in silicon. We sat down with Gerald, who was very good. He showed us a system and what

mechanics were available to operate sample holders, and came up with a carbon cylinder to which we could attach samples and rotate at constant speed.

We cut samples at North Adams. Our wafers at that time were quite small. We cut five-sixteenths-inch squares from these and then grooved the carbon crossways in the cylinder so that we had twelve rows going around. We then made carbon clamps that came down over the edge of the samples, so that we had a holder. Gerald put together a system to shadow that holder from the whole beam with ports in it, so that we could run the beam on any one of the ports. That reduced the current, which is total current divided by the area sampled.

We got up to forty microamps or less of ion current on the sample. Those were low voltage systems, and forty kilovolts was the maximum acceleration from the power supplies that they would use. We started going to doubly charged ion, which gave us twice the voltage. The initial work was all done with about forty-kilovolt beams.

The original experiment was to find out, number one, if we could put ions into the silicon without doing a tremendous amount of damage, which was what everybody was concerned about. Secondly, we wanted to check the suggestion by [M.] Scharff, [H. E.] Schiott, and [J.] Lindhard that you get deeper penetration through open channels of the crystal (1). Thirdly, and most importantly, we wanted to find out if the ions got in there; if we could make junctions with them by placing them in substitutional positions in the silicon crystal.

We set up the experiment with the three different orientations of silicon in five-sixteenths-inch squares and implanted them. We monitored the beam current and because we knew the area, we knew what the dose was. We implanted the three different orientations and took them back to North Adams for evaluation. The easiest way to find out if you have a junction is to angle lap the implanted surface and stain it, to see whether you have a conversion.

We did that, and lo and behold, we had junctions. However, when we measured them electrically, they were rotten junctions. Visibly they were there, so we thought that maybe the damage was causing the problem. In short, we looked at annealing processes to see if we could remove the damage, and we did. We found we got good junctions electrically, but we also found that the junctions had moved down, sometimes almost 50 percent more than the original depth.

LÉCUYER: Was that due to the temperature?

MANCHESTER: Even though we were using a low temperature, if we had damage, we might be getting some diffusion. We checked the remaining samples, and found that the junctions on the 1-1-0 plane were deep, and that the junctions on the 1-0-0 and the 1-1-1 plane were not quite so deep. That seemed to go along with the idea that Lindhard, Sharff, and Shiott had proposed.

I think at that point we had established that we could inject ions. We got good junctions, and the crystal properties had repaired themselves to the original with the thermal treatment, so maybe we had a process. That was the basis for the first paper. The first paper or the first talk I gave on that was at the 1965 American Physical Society meeting, and I'm not sure if it was intentional or not, but they put me on last in the program, and I sat through two days of that meeting and when finally it was up to me to give the paper I gave that paper to three people. Everybody had left. That wasn't very encouraging, but that was the first paper.

After refining and obtaining more data, I wrote the paper which I gave at the Electromagnetic Isotope Separator Conference. Very few of these people were semiconductor people. They all were involved with electromagnetic separation and ion sources, but I think we got pretty good reception. In fact, one gentleman, Philippe Glotin from Centre d'Etudes Nucléaires de Grenoble, came up to me afterwards and said he would very much like to have me talk to his people at the university. "Because," he said, "that sounds like a thrilling thing to do."

I rearranged our schedule and talked to the group in Grenoble for a few days. They were very enthused about it. A year or two later Glotin edited a book on applications of ion beams to semiconductors (2). He had collected a lot of publications on what had gone on and some of the work that we had done, and he asked if he could use some of it.

LÉCUYER: Was his lab a semiconductor lab?

MANCHESTER: Yes. There was a spark of interest there, so that was encouraging. The research progressed because there were still a lot of unknowns. What was happening? Are we using all of the ions we impinge on the surface or collecting them? Is the damage really repaired? Was there diffusion when we tried to thermally anneal these things? We also didn't have any equipment to do any of the implant. I talked to Leon Love at Oak Ridge, and said, "We've got this problem and we need to get more information."

[END OF TAPE, SIDE 4]

MANCHESTER: —if we can use tracers to really locate these ions. I said, "I think we can come up with a way of partitioning the substrate so that we can look at it in depth." Leon replied, "Well, let me talk this over with Gerald." Gerald came back and said, "Yes, I think we can mix  $P^{32}$  with  $P^{31}$ ," which was exactly what we wanted to do. There was a problem with radiation protection at Oak Ridge, and so there were limits as to what we could do with the radio tracers. They devised another holder in which we could put our five-sixteenths squares and monitor them, implanting with the  $P^{32}$  and measuring the dose by measuring the  $P^{31}$  impinging in the holder. The separation was such that the  $P^{32}$  would hit the sample, and the  $P^{31}$  would hit the collector. Using the isotope ratio that exists, we could measure the  $P^{32}$  dose on the samples by measuring the collector current.

That worked pretty nicely, yet with that setup we could do about twelve samples at a time. We couldn't do a whole cylinder of samples, but it worked well. We ran the three orientations of silicon again and put in a low dosage of the tracer element. It was somewhere around  $10^{11}$  ions per centimeter<sup>2</sup>, which corresponded to the same amount of ions on the sample. We then put in the remainder in P<sup>31</sup> to bring it up to  $5.0 \times 10^{14}$  ions per centimeter<sup>2</sup>, which gives us measurable numbers.

We added that in three ways. In the first series we added the tracer dose, followed by the P<sup>31</sup> to bring the total dose up to  $5.0 \times 10^{14}$  ions per centimeter<sup>2</sup>. For the next series, we put in about  $1.0 \times 10^{14}$  or  $2.0 \times 10^{14}$  ions of the P<sup>31</sup>, the tracer dose, and the remainder of the P<sup>31</sup>. Then we put in all of the P<sup>31</sup> first and then the tracer last. We did that to see if there was any influence on the penetration because of channeling.

We ran that series of experiments and it worked well. The accelerator was a great piece of equipment. We returned to the lab and devised a technique to remove 1,000 angstroms of silicon at a time. Because it was a chemical oxidation process, we oxidized the surface and then dissolved that oxide and measured the activity in it. We measured the activity of the rest of the sample as well. We had to be positive.

We did that step-by-step until we removed 10-12,000 angstroms. We corrected the data back to the time that they were implanted, because of the decay factor, and plotted the concentration as a function of the depth. We got the nicest curves, and we were really pleased that they matched almost exactly the depth and shape that Scharff, Schiott and Lindhard had calculated. They had made range calculations for a number of different materials, based on their isotope studies. These calculations weren't experimentally verified, but our results fit almost exactly.

We found, by varying the position of the tracer in the dose, that we got deeper penetration when we put the tracers in first rather than when we put them in the middle or last. That indicated that we were blocking the channels some way, either by damage or by knocking atoms out of the silicon and into a channel. In looking at the different orientations, we got different depths of penetration and they agreed with the projected area of the channel. The other two penetration depths were about the same, but less than the open channel.

The next thing we needed to do was to determine whether there was any diffusion during the anneal step. We saw that by delineation, you could see the junction move, and we had to determine whether that was due to diffusion or whether it was due to changing ions from interstitial to substitutional. We then annealed part of our samples. We had determined originally that there was an optimum time temperature anneal procedure. I think for phosphorus, it was about 600 degrees, for ten minutes, and for boron the temperature was higher. That brought us to bulk properties.

We took a sample that was annealed and the other unannealed sample, and did that procedure for delineating with depth. We found that the two tracer activity curves were exactly

the same. The ions weren't moving. We were reducing the crystal damage and a lot of the ions that were interstitial were now jumping into substitutional positions where they could donate carriers.

Having understood the anneal process, we were very confident that we had an effective process for making semiconductor devices. We were confident also that the technique would be useful as a diffusion source.

We wanted to go a bit further. We did some Hall measurements on these samples to determine whether the mobilities were what they should be for a bulk sample and whether the Hall constants were satisfactory. We pursued those studies, and they showed that effectively the mobilities were the same as bulk silicon, and everything was falling right in place. After we had done that, I did a series of implants, one of which was a boron base implant, followed by a phosphorus implant after masking. That created the first ion-implanted planar transistor.

LÉCUYER: Was that done at Oak Ridge as well?

MANCHESTER: I used the Oak Ridge machine for that, and I have a nice, big picture of that at home on my wall. Unfortunately, the properties were not very good. We had the same problem we had with diffusion: we had to be absolutely clean, because any little particle of dust on the surface could mask that beam, and there will be no penetration similar to diffusion. It's the same as creating a hole through the implanted layer. The emitter was shorted to the collector, through the pipe created in the base caused by the particle. Looking at the units with the electron beam, in the SEM [scanning electron microscope], we could see some of these pipes.

LÉCUYER: Was that in 1965?

MANCHESTER: No. That was in about mid-sixty-four.

LÉCUYER: It's around that time that you published an article with James [F.] Gibbons (3). What's the story behind the article?

MANCHESTER: I was never associated with that part of the university while I was in school; I met Jim [James Gibbons] at a conference. He had heard my paper, and was interested in the results that I was getting, and so we had a long discussion about them. That was my last contact with him.

LÉCUYER: I found an article published under both of your names.

MANCHESTER: Oh, that's interesting. I should have known that, because he said he wanted to know if he could use some of the data that I got, and I agreed.

LÉCUYER: I found it in the *Applied Physics Letters*. The paper is under his name, your name, and there are two other people there.

MANCHESTER: I'm going to have to look that up, because I don't recall seeing it. We never really worked together, we just talked together.

LÉCUYER: Did you present your results at other conferences?

MANCHESTER: Papers were presented at the Conference on Electromagnetic Separators, the Second International Electron and Ion Beam Conference, Electrochemical Society Meeting, International Conference on Electron and Ion Beam Science and Technology, IEEE [Institute of Electrical and Electronics Engineers] Conference, IEEE International Solid State Circuits Conference, and The National Electronics Conference. Some of the initial results were presented at an AIME Conference in San Francisco [California]. For that, I upgraded some of the data so it wasn't just a rehash of the same thing.

LÉCUYER: Was that the first time you saw Gibbons? Gibbons must have gone to the AIME conference.

MANCHESTER: I think that's when Gibbons and I talked, yes. He was very interested in implantation. I think he contributed to a book and published a number of papers. Some of his students were working in that area.

LÉCUYER: Were there were any semiconductor people at the AIME meeting in San Francisco?

MANCHESTER: Yes, there were. I got questions from the audience, which I'm sure came from semiconductor individuals. The universal position was that damage would be the problem. I don't think they fully accepted the data that we presented that indicated that we had removed the damage. That, I think, was the biggest impediment to implantation.

LÉCUYER: Was there any competition between Sprague and the people at Ion Physics?

MANCHESTER: No.

LÉCUYER: They were two independent projects?

MANCHESTER: Yes. All the work at Ion Physics was done with a high-energy beam, primarily 100 kilo-electron volts up to a mega-electron volt. I was concerned about the energy input to the system producing only crystal damage, which we showed that we could remove with the low energy beam, which had sufficient energy to even cause some preliminary melting of the surface. Late in 1964, after I got some of the first results, I'd proposed in my notebook that that might be a great way of channel doping for MOS [metal oxide semiconductor], yet with high accelerated voltage it couldn't happen. Also, getting the amount of dopant into the substrate for resistors or other structures would take a lot of time, because you have to work at extremely low beam current to compensate for the high accelerating voltage.

They were trying to do sources and drains for MOS, and I think they were attacking the energy problem by implanting through oxide thicknesses such that you could protect the surface. In other words, you could absorb a lot of energy, plus a lot of the ions, in the oxide. One of the patents that they were granted involved implanting through an oxide. MOSTEK [Corporation], who received financial and technical support from Sprague, was contacted about that patent by Ion Physics, because what we were doing infringed.

There was a patent on implanting through the oxide that IPC [Ion Physics Corporation] had, and we had a preliminary conference with the Ion Physics lawyers. They actually took depositions. I went back and really searched the literature for information that Ion Physics may have put out either in reports or government contracts, as they were doing a lot of government contract work. I found a government report that indicated they were implanting through oxides which was dated more than a year before they applied for the patent.

LÉCUYER: That killed it, right?

MANCHESTER: I said, "Jeez, we've got them by the short hairs." When we brought that up, it closed the deposition action very quickly. MOSTEK offered a small settlement to avoid court expenses, and I don't think they ever heard from Ion Physics after that. That patent would have been a great patent, had they not published before. In the use of implantation in integrated circuits or any semiconductors, most of the implants are done through an oxide.

LÉCUYER: Could we go back a little bit in the past? Why was the National Research Corporation interested in that type of problem? It's clear for Sprague, but why would NRC [National Research Corporation] be interested in that?

MANCHESTER: Because they were interested in the equipment side of it. They did much in the area of electron beams and source design and contributed quite a bit to the building of our ion beam system. Before we got to that point, I hired John [D.] Macdougall from McMaster's [McMaster University], who had done his graduate work in the source design and controlling of beam parameters. I met him at a conference in New York, and asked him if he might be interested in coming to Sprague Electric. He had not yet quite finished his Ph.D. program, but he invited me to his lab for further discussions.

LÉCUYER: Where was he?

MANCHESTER: In Hamilton, Ontario. I spent some time with him, and then made him an offer. After he finished his work, he decided to come, and he still is with Allegro [Microsystems, Inc.], which split off from Sprague before Sprague disappeared.

LÉCUYER: When did he join your group?

MANCHESTER: I hired him in 1966.

LÉCUYER: If you look at the history of ion implantation, there were two groups that made a major impact, namely, your group at Sprague, and Ion Physics. What strikes me is that these two corporations were both based in Massachusetts. Ion implantation was really developed in New England, and there was much less work in other places, on the West Coast or in Texas. What is the reason for that?

MANCHESTER: I don't think there's any real reason for that. Massachusetts is evidently a good place to work. Later on, one of the manufacturers of ion implantation equipment, Extrion [Inc.], was based there in Massachusetts, also. I'm not too familiar with the background of Ion Physics, but I thought that initially they were just high-voltage equipment people, the same as NRC was a low-voltage equipment company.

LÉCUYER: Many of the people who started Extrion were at Ion Physics before, so there's a move of techniques and knowledge between one and the other.



MANCHESTER: Yes. I never really gave that a thought, but I can't think of a basic reason, except that Massachusetts is a good place to work.

LÉCUYER: I wanted to ask you about the article that you published on the art of semiconductor doping by ion implantation in *Solid State Technology* in 1966 (4). What is the background of that article?

MANCHESTER: That came naturally, because it's always been my position that research is good if you can apply it. Research for its own sake might be self-satisfying to some people, but it doesn't help what we're doing. With that philosophy, the natural thought was, "How are we going to use this ion implantation process that we've developed?" We did the implantation to find out if we could put ions into silicon, remove the damage and make usable junctions. We now had to show how to adapt that to a useful combination process where one is a low-temperature process and one is a high-temperature process. We had to make them mesh. What we did is look at areas in which we could use that technique to replace a problem part of the process or add in a new step and then determine how we interface that technique with the remainder of the process, to make the device or integrated circuit.

One of the first areas relates to that Concord problem that I told you about. How could we now use the process to improve the precision and accuracy of diffusions, as we presently need them in our transistors and microcircuits, which are much deeper than what you can do with implantation, in order to reach the parameters that you need for the devices? Well, the normal thing is to use it just like we would a predep [predeposition] for a diffusion. In that case, we would control very accurately the concentration of material to put in that predep. Then when we did the diffusion, we would get the same parameters for that junction on the wafer and wafer-to-wafer. The question was: could we get sufficient material in the source for our diffusion? We proved that that was not a problem. In fact, one of the early outcomes of that program was the PNP process that I talked about earlier. They have implanted bases.

LÉCUYER: Was that in 1966?

MANCHESTER: That was about 1967, 1968. The other thing is that we had to convince our manufacturing people that what we were doing was going to be valid. We've always tried to maintain a good working relationship with our manufacturing group, which is key to making progress in the semiconductor industry. We did that and provided them with data that they could understand. We made a lot of PNPs in our lab, considering the parameters. We showed them the data, and they agreed to try the process. They were agreeable to their PNPs being ion-implanted in North [Adams], and they got beautiful yields. The parameters were always uniform and well-controlled.

LÉCUYER: Did you also make ion-implanted bipolar integrated circuits at that time?

[END OF TAPE, SIDE 5]

MANCHESTER: We didn't think that it would be advantageous to use it in a microcircuit for the transistor structures, but rather to use it in things that one cannot do easily with diffusion, like high-value resistors. Of course, in the MOS area it's an entirely different story. The primary process, I think, is implantation. Now, the channel doping is universal.

LÉCUYER: We could now go back to John and the group. You built a fairly large group by 1966. Did you have John Sprague's support for ion implantation? Where was the money coming from? Why did Sprague hire that many people to work on the project?

MANCHESTER: I think the prime reason we got as much support as we did was because Mr. Sprague really was interested in it. He would talk about "Technology X," the implantation program, and he really felt deeply that that process was going to rule semiconductors, and I think he was right.

LÉCUYER: Was this R.C. Sprague or John Sprague?

MANCHESTER: That was Mr. Sprague, the older Sprague. John Sprague was interested in it, but his dad was controlling. I have much respect for the old gentleman. He would come down to the lab and sit down with not just me, but any one of the people working around there, and ask about the projects they were working on. If you had a discussion with him, you very quickly learned that you didn't tell him something that you couldn't back up. He might have said, "Well, what do you think? Where is this going to be two weeks down the line?" You're much better off saying, "At this point I don't know," rather than, "Well, it's going to be here, here, or here," because two or three weeks down the line, Mr. Sprague was back sitting in your office, saying, "You told me that this was going to be here, here, and here. Now, how are you doing?" [laughter]

He was an amazing individual. He could keep track of many different projects. He always came down with enthusiasm for what you were doing, if you had the enthusiasm for what you were doing. If you didn't, he sat back and said, "What's your problem?" He was a great man. I truly enjoyed working for him.

LÉCUYER: How did you convince him that implantation was the process?

MANCHESTER: Through the limited results that I had when we first talked to him, and our enthusiasm for the process. I think he accepted that, and as time progressed, he became very aware that that judgment was good, because it was in production and it had solved a lot of problems. In fact, I guess today I would rate implantation right along with the epitaxial growth process. It's a prime process now.

LÉCUYER: All of the complex integrated circuits are made with ion implantation.

MANCHESTER: Everybody's using it, and not only in semiconductors.

LÉCUYER: Absolutely.

MANCHESTER: One of the fellows that worked in the lab went to Gillette [Company]. He was back not too long after, wanting to know if we were willing to talk about a program of implanting carbon in razor blades to maintain sharp edges.

LÉCUYER: When was that, in the seventies?

MANCHESTER: Yes, 1969, 1970. It was after MOSTEK was there. Now, whether they do that or not, I'm not sure, but I've been told on the side that, yes, they do.

LÉCUYER: Could you talk about John Macdougall and F. [Frank W.] Anderson and the people you hired?

MANCHESTER: Macdougall was a physicist. I also hired Frank Anderson, who was with me at Shell [Oil Company]. We were the research unit, Shell Development, and he was at Shell Oil, a production facility.

Fred Fowkes, who came from Shell Development and interested me in Shell, also went to Sprague. He came as director of research for not just semiconductor research but for capacitor research also. Fred came just about the time I did, and was there for a number of years. He wanted to go into teaching, so he went to Lehigh [University], and he stayed there until he passed away a couple of years ago.

LÉCUYER: As I understand it, the director of research and development followed you to Sprague. That was quite a move from Shell to Sprague.

MANCHESTER: Yes. Shell was having some moving pains about that time, and I think that whole function there in Emeryville moved to Houston.

LÉCUYER: There was strong incentive to look for something else.

MANCHESTER: Quite a few of the people that I'm aware of weren't very interested in moving to Houston. I'm not sure if that is why Fred came. I think Frank came because he thought it would be an interesting area. Shell Development had a good segment of people at Sprague.

LÉCUYER: Could we talk about the new system that your group built at Sprague? If I understand well, it was a joint project between Sprague and National Research Corporation.

MANCHESTER: It was a joint project, in that they were responsible for building the source and the sample chamber. John contributed a good deal to the design of that ion source. They also put in some information. It was one of the first systems that had an E-cross-H or Wein Velocity Filter analyzing system, which John [Macdougall] designed. I think most of the systems now have that kind of analyzing system. We built the rest of it in our machine shop and in our lab. In fact, we looked for a long time to find a magnet which would be acceptable for the H-field. [John] finally found one and then designed the rest of it around that magnet. In order to be sure we knew where we were, we had to re-magnetize that magnet, and for one that big, we finally ended up someplace in New York City. They magnetized our system again so that it was up to the field density that we needed. Then we installed it.

National Research [Corporation] built the chamber. The chamber that we used was about two feet wide, about nine inches high, and the top would lift up if we needed to get into it. We could put samples in through vacuum-sealed ports in the side. In it, we had an XY table which was driven by external motors which were connected through vacuum seals to position it. Also in that system was a Faraday cup for measuring the beam currents. On top of the chamber sat a glass section that provided drift space, and on top of that was the E-cross-H analyzer. We sealed all of the metal to insulator seals with epoxy, which by today's standards, is not the material to use, but at that time it worked very well.

Then on top of that there was a set of deflector plates, an aperture, the Einzel lens and extractor, which sat just below the source. John designed them and the spacing and the size needed to get the proper focusing, but National Research machined them. NRC built the source and the vacuum housing. The whole system sat on legs about three feet high, and the complete

system was about eight feet tall, so that the actual system minus the legs was about five feet. It was a nice compact system, and it worked very well.

The ion currents that we could get were in the low microamp range, which was all right. The separation was good. The dispersion was such that we could get one mass unit separation for the desired element, and we could run either a gas or a solid source that had sufficient vapor pressure. The whole system was pumped with a mechanical and a diffusion pump, and the main part of the system ran at about  $10^{-6}$  Torr. The source had its own diffusion pump, and because that's where we created the ions, we had to vary that pressure according to what we were using. It would nominally be about  $10^{-4}$  Torr.

LÉCUYER: Did the group make many units with that system?

MANCHESTER: When MOSTEK took over the process, we were the source of implant. That system made all of the MOSTEK wafers before we got their system in place.

LÉCUYER: This system was produced and given to the manufacturing people who made PNP transistors?

MANCHESTER: We did the first PNP transistor implants with that system, and then shortly after, we got in contact with Accelerators [Inc.]. They made isotope separators at the time, which was the initial part of the system that we wanted to run. We spent a couple of days convincing them that they needed to be in the business of making implantation systems. We showed our design to them, and they decided to gamble on it.

It was our design except for the accelerating lenses and the focusing lenses, and the source belonged to them. It was an RF [radio frequency] source, but it worked well with the boron and phosphorus materials we used. We designed a sample chamber and wafer handler, which was built in a small machine shop in Connecticut, so it was really a production-type system. It had capacity. We could run hundreds of wafers a day in that system.

LÉCUYER: That was the one that was designed by the Sprague group and produced by Accelerators.

MANCHESTER: Right.

LÉCUYER: Did Accelerators sell the system to other companies?

MANCHESTER: They did, with the exception of the sample chamber and wafer handler. That was one of the things that we had to agree with. If they did something for us, they're going to have to be able to use it. We didn't look at it as competition if somebody wanted to use the system. We looked at it as advancing the technology, and at that time that's really what it was.

LÉCUYER: The idea was really to outsource and then convince other people to do it, and specialize in that.

MANCHESTER: Yes.

LÉCUYER: Would you like to talk more about the systems that were designed at Sprague?

MANCHESTER: You mean the initial system that we—

LÉCUYER: Yes. The initial system and then the one that was designed jointly with Accelerators.

MANCHESTER: The initial system proved to be a good system for not only experimentation, but as an ion source for high-voltage work. One could tie the Sprague system or the Accelerators system onto the end of a Van de Graaf and work at high voltage. It produced a dispersion that allowed one mass unit selection, and we could run at up to seventy kilovolts if we wished. We didn't go any higher because of breakdown problems. When we started getting up into those voltages, we had to be concerned about spacings and material composition. Most of our work was done in the forty-kilovolt range, which has proved to be a good range. It was an excellent system, and I would have liked to have seen it go into a museum.

LÉCUYER: Do you think it's still at Allegro?

MANCHESTER: No, it's not at Allegro. We donated the first system to RPI [Rensselaer Polytechnic Institute]. Allegro has a number of systems now, but they're all commercial systems. That has become a big industry, too, just building and selling. As I recall, they have three systems that they use, and they have capabilities of 150 kilovolts and higher ion currents. Their wafer handling capabilities are far different than what we had. These present systems have input gates where you drop the wafer in and it goes through a gate and ends up in a position for implant and then drops out another gate into a bucket, so you can continually feed material.

Of course, the one that we initially put together for the Accelerators system had wafer holders which fit into a long tube, and the wafer holder had two-inch wafers all the way across on four sides. You could dope one side by running it through the beam at a controlled speed, and then flip it over and run it back again, up to four sides. Then you had to close the gate valve and open the system, take the wafer holder out, put another one in, pump it down, and open the gate valve. You're continually pumping down, and you just add wafers as you go. The initial system proved very good.

We did all of the Concord wafers and some of the MOSTEK production with that system. [MOSTEK] established a production area in the Worcester facility of Sprague. While they were doing that, we were doing their two-inch wafer, because Sprague was still running two-inch wafers and doing some of the processing for their circuits. They became interested in having an implant system of their own, and since they were going to three-inch, they wanted to be able to implant three-inch wafers. With the holder that we had on our system, it was not possible to run three-inch wafers without completely modifying the system.

They ordered a system from Accelerators which was essentially the same as ours, but it didn't have a sample chamber on it. We designed one that matched the one we had on our system in North Adams, only it was bigger so that it could take three-inch wafers. Looking forward a bit, it could be modified very easily to take four-inch wafers, if the industry ever got to four-inch wafers. We built and installed that system, and it ran nicely. Essentially it was a controlled-speed holder which ran the wafers past the rasterized beam.

The MOSTEK system was a six-sided holder. It ran the two-inch wafers at controlled speed, and the holder was such that you could check the beam current as you were implanting or at any time during the implant by sampling the beam with a Faraday cup. It had a capability of 150 kilovolts and currents up in the high microamp range, if you needed them. All of their implants were channel implants, so they really were using the same parameters that we did, as far as the current, the ion and the doses.

To get into perspective about MOSTEK, MOSTEK was a spin-off from Texas Instruments [Inc.]. It was formed by [Louay] Sharif, Bob [Robert B.] Palmer, and L. [Leon] J. Sevin. They were looking for some backing to start their own company. On paper, they formed a company called MOSTEK, but they needed some backing and someplace to manufacture. They approached Sprague. Mr. Sprague, I think, became very interested, because the MOS line was something that he envisioned tied in very closely to our bipolar research, and so he thought it was a good deal. Also, L. J. Sevin is a real talker. He could convince you of anything. He convinced [Sprague] to invest in the company. Part of that investment was related to technical exchange because Palmer came over to find out what we were doing.

They were interested in making their product different from that of Texas Instruments. They wanted to use tantalum oxide as a gate insulator to reduce their thresholds, and Sprague had experience in tantalum oxide since we made tantalum oxide capacitors.

Bob Palmer, a very energetic, likable guy, asked us whether we might be able to help [MOSTEK] in producing oxides on a wafer. We said, "We could probably work on that, but we think we've got something that might be more interesting for you." By that time we had a lot of data on threshold control and stability. Of course, Sprague was not an MOS company, so the technology didn't have anyplace to go. When Palmer walked in and we showed him all that data, his eyes lit up, his ears started to buzz, and he said, "Oh, boy, that's what we need."

[END OF TAPE, SIDE 6]

MANCHESTER: They said, "That's our new direction." To me, that's the way to get ahead. Look at the data, evaluate it, and then make the decision. They already had a small crew of designers in Texas that were part of the group. They designed the first circuits, and we processed the oxide growths and source and drain diffusions in Worcester [Sprague Electric Company manufacturing facility, Massachusetts]. Then the wafers came to us, and we did the implant for threshold adjust.

LÉCUYER: The circuits were designed in Texas. Was the process redeveloped in Massachusetts?

MANCHESTER: Yes. Most of their initial circuits were in ceramic packages. We may have done some of the packaging, but I think they may have contracted for the rest, because they weren't into plastic at that time. The first ones were a random-access memory and a read-only memory. They had quite a bit of success with those, because the low thresholds were compatible with TTL [transistor-transistor logic].

[MOSTEK] contracted for a portion of the space in the Worcester plant and put their own process line there, through which they ran their circuits. Initially we still did the implant. They contracted with Accelerators to build a system which matched ours so that they could install that there, too. In fact, it was interesting that at the time we were doing implants for MOSTEK, Sprague went through a disastrous strike that lasted almost a year. That was start of the demise of Sprague.

We did the processing. We would get the wafers from them and bring them to the Research Center. Although we were locked in by the strikers, we had to get supplies like liquid nitrogen. I used to run a truck from North Adams to Worcester to get tanks of liquid nitrogen into the center through the picket lines so we had nitrogen to operate the system. I also took wafers back and forth. We operated that system all during that strike and did quite a few implants. After we designed the sample holder for their system, they did all the processing, and they were free of that type of problem. We just worked with them as consultants. The reason they were successful, I think, is that they were so enthused about making that process go.



LÉCUYER: I have a few questions regarding the 1960s period before we move on to MOSTEK. One is that we didn't talk at all about Kurt Lehovec at all. What were your interactions with him and what did he do at Sprague?

MANCHESTER: Kurt didn't have much to do with ion implantation. He was more of a theorist and a very intelligent man. In fact, he had the original patent on integrated circuits and junction isolation, which most of us thought should have been pushed. That was one thing I kind of held against Mr. Sprague. He didn't want to push anybody on our patents for fear that there would be action that we couldn't handle. Maybe he was right. The amount of dollars involved in litigation bothers me.

Kurt was instrumental, with Joe [Joseph] Lindmeyer and Chuck [Charles] Wrigley, in beginning MOS design at Sprague. He provided most of the theory. I think a lot of his time originally was spent as a Sprague Fellow at one of the universities in Europe. He became a part of the research group when he came back, but he wasn't related in any way to processing. He left not too long after.

Kurt loved to play the stock market, and he did pretty well. In fact, he got to the point where he had enough to get out of the industry and go into teaching. He went to USC [University of Southern California] for some time, which was kind of unfortunate, I think, because not long after he quit, we noticed that the stock that brought him the great dividends had capsized. Kurt came back, oh, once a year or so. He didn't show any interest in the [ion implantation] program.

LÉCUYER: What was behind your work on transistors with variable thresholds? How did it develop, and what were the results?

MANCHESTER: Initially it was probably more fortuitous than anything else. I first wrote the idea in my notebook in 1964. I thought it was a great way of controlling thresholds in MOS. Although I didn't know a lot about MOS, I wrote that, and after we did the work at Oak Ridge and put up the new system, we said, "Maybe that's an area that we need to look into."

John [Macdougall], the head of that area, started a program to look at implant through oxides, and then implant through oxides into channels. The initial result looked pretty good. We could produce threshold shifts with a light channel implant and anneal. He also looked at the annealing process. The only problem that everybody looks at is damage, and so he went back and redid a lot of the studies that we originally had done on stabilizing the bipolar systems by baking, and found that he got pretty consistent results.

He said, "Let's see what kind of variation we can create," and measured thresholds at different doses. He found that there was a very linear relationship between the dose and the

shift (the delta), and he could very controllably make any voltage shift available in the transistor by controlling the doses. He really documented his work on that, and we looked again at stability. In fact, there's a paper published in the June 1970 issue of *Electronics* entitled "Ion Implantation Offers a Bagful of Benefits for MOS" (5).

We started with describing the shifts and advantages available, and completely documented the fact that that was a usable process. We had already done the depletion loads, and knew that it was the next active device that MOSTEK was going to design into their circuits. We thought, "Now maybe it's possible to look at what we can do with complementary MOS," with a PMOS and an NMOS transistor. The sources and drains we created by diffusion, as it had to be a low-temperature process. We created the pre-dep [pre-deposition] for the P-channel with an implanted dose so that we could control the resistivity of that pocket. That was critical in that process. We did create a process which made a very usable CMOS [complementary metal oxide semiconductor] structure. We wrote the article to generate interest for business purposes and it went over quite well. We still were of the position that anything we could do to move the process forward, why, that's good. In fact, Bob said that someone who had blacklisted implantation called him and said, "Maybe you have got something there." [laughter]

LÉCUYER: The special voltage work that you did was the key application for ion implantation. It proved that ion implantation made possible things that were not possible with other technologies. According to Jim Gibbons, that's really the time when ion implantation was accepted and adopted by many companies. That was a critical piece of work and a critical article, too. When was the CMOS work done? In 1969?

MANCHESTER: It had to be about 1969, because [MOSTEK] wanted to get Bulova involved. Bulova Corporation wanted to start a program to produce CMOS structures for watches. We visited them and put on a show for them. For some non-technical reason, they didn't accept our proposition. MOSTEK took over, and used the process in a number of circuits, including watch chips. They also produced the first calculator chip for HP [Hewlett-Packard Company], and I think we did some of the implants for that.

LÉCUYER: Could we talk about the Sprague product line in the late sixties?

MANCHESTER: They had a tantalum and aluminum capacitor line. In the late seventies they went into ceramics capacitors, for which they built the Texas facility in Wichita Falls. We were into semiconductors mostly for linear circuits, TV and radio applications. Concord supplied electrochemical transistors and some silicon units. Sprague developed a product line of military circuits, primarily linear circuits with the exception of some switches they made. They were the first high-current, high-voltage products on the market.

Sprague gradually moved out of the TV [television] and radio area. They made some FM circuits, but moved out of that area because it consumed too much of their test time, or our testing was not done correctly. We gradually moved on and got into high-voltage switching networks and, again, more military products. We were also into the Hall sensor business. In fact, still a large portion of Allegro's business is from Hall cell sensors.

LÉCUYER: What were the sales revenues that Sprague got from semiconductors in the late sixties?

MANCHESTER: I've got to be honest about this; Sprague didn't really know how to run the semiconductor business. They floated around six to eight million dollars a year, which was not good. Sprague semiconductor was kind of a loss leader in the company, and it was supported by the capacitor function of the company.

Sprague itself started going downhill. They were sold to Penn Central [Corporation], which pulled the plug, because Penn Central didn't know how to run a semiconductor group, or a capacitor group, either. This is strictly my take on it, but that's the primary reason for Sprague's demise. I was there when it started and I was there when it disappeared, so that hurts a bit.

We developed the Hall sensor. That is now a large part of the business. There's been a lot of work going on in that area, with which some of it I'm not familiar because it's been a while since I was associated with them. I did a lot of work in the area of packaging, primarily the chemistry of surfaces, to correct a lot of problems that they were having with stability in their devices. There is still, I think, a proprietary area, so I can leave that one out.

LÉCUYER: We could go back to the outline that I had here, and start again in 1969, because it was at that time that you became the head of research and development.

MANCHESTER: Yes. 1969. I think that came about since Lincoln Vogel had left. He went to the University of Pennsylvania to teach, so they put me in that spot. I was primarily involved with the semiconductor research. I wasn't involved with the capacitor research at that time. I think it was a change in name only, because I did the same things I was doing before, anyway.

LÉCUYER: Then you were orienting the research in semiconductors.

MANCHESTER: Yes, our prime goal yet at that time was still developing those aspects of implantation which we thought would be valuable in the present systems.

LÉCUYER: How big was the semiconductor group at that time?

MANCHESTER: After the strike we lost people, and I think at that time we had about twenty-five people. That's engineers and technicians, because we maintained a process area in the center. We could do all of our processing there.

LÉCUYER: We talked quite a bit about MOSTEK. Would you like to add things to what you've said before?

MANCHESTER: I think, by and large, MOSTEK was a good company, and I think its demise is also based on poor judgment by [United Technology Corporation]. United Technology took over after L. J. Sevin left, and at the time Bob [Robert] Palmer was president of MOSTEK, because L. J. had left to get involved in venture capital. I'd talk with Bob every once in a while, and he said, "Well, what they need just always got turned down when it got up to the board," and he said he just cannot run in that business unless he stayed abreast of what's going on, which is absolutely correct, and I think that's the reason it went down.

In my view, it should still be running, because I thought it was a great company. They weren't afraid to gamble. They would look at the data, assess it, and then make a decision, there was no uncertainty in it. If the decision happened to be wrong because the data was wrong, they would go down and come up fighting again. Good company.

LÉCUYER: How did the relations between Sprague and MOSTEK evolve over time? There was very close collaboration in 1969, 1970.

MANCHESTER: At least on the technical level, there was a good relationship. We worked together even after they put in their process section in the Worcester plant. At the same time they started building a facility in Carrollton, Texas. When they finished that, the move out of Worcester was very amicable. We hated to see them go, really. We parted on good terms so I think that relationship worked out well, because there was good interface between their people and our people.

LÉCUYER: That was due to your relations with Bob Palmer.

MANCHESTER: Yes. I haven't seen Bob since he left DEC [Digital Equipment Corporation]. I haven't seen him since he took over that presidency. We used to get together every couple of months or so and talk. Yes. Bob was a good guy. L. J. was really good, too. Sharif, I never

could get up to him. I don't know what my problem was, or his, but, he didn't stay with MOSTEK very long. I had many times thought of changing and going to work for MOSTEK, but I just couldn't do it. I'm one of the older people that are still loyal to a company. I don't see a lot of that today.

LÉCUYER: Could we move on to ion implantation at Sprague after 1971 or 1972? Did Sprague continue working on the process?

MANCHESTER: Yes. The initial work on resistor implants was done earlier. That was about 1969. We did a pretty thorough investigation of resistors. We did have a couple of linear circuits that we were making at that time, which had—

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MANCHESTER: —two 80K and two 50K resistors on them, which took up more than half of the circuit area with diffused resistors, because your diffused resistors are normally about 150 ohms per square. We took that as a vehicle to integrate the implantation process with bipolar processing so that the parameters of both were maintained. The normal bipolar processing is all high-temperature processing, and the implant process—we want to stay away from that to prevent diffusion and maintain the parameters we had there. We took that circuit and developed the process.

In order to get those resistors in and have them maintain the characteristic we're shooting for, we determined that that kind of implant had to be done after the emitter diffusion, because that's the last high-temperature step. Then the next one, which is the sintering of the contacts, is a 500-degree step, so that looked all right. We took their mask set and removed the four resistors, and then put in the geometrics for the four implanted resistors. There was a difference of day and night in the area that they took. We still had to use the same area in order to have that mask set. We put those in and determined what the parameters needed to be to meet their specs [specifications].

The other requirement that these two resistors had to meet was temperature tracking. The ratio of the two had to remain constant with temperature. They also had to track one of the diffused resistors in the circuit. That we found was easily attainable with the implants, since the actual concentration of the implant is of the same order or the same value as the diffused resistor, but the layer is very thin. For that reason, you get the high ohms per square.

The processes we looked at ranged anywhere from 1 kilo-ohm per square up to about 10 kilo-ohms per square. When you get up to 10 kilo-ohms per square, you've got some problems, as the stability with voltage is not that good because they're so thin that the substrate almost acts like a gate to a MOS transistor. If you squeeze that depletion layer back, the value of the

resistor changes. Most of the implants that we did were down around 3 or 4 kilo-ohms per square.

We had a bit of a problem getting implant into our production area. It was nowhere near as easy as it was at MOSTEK. I guess there was always the feeling that they're not going to be stable because of the damage, or just the fact that it's a change of technology. We had a lot of difficulty, and so I thought, "Let's try another circuit and see if it works." We took a TTL circuit and changed the resistor pattern to make it a low-voltage TTL with high resistance. We ran the same process and put the resistors in after the emitter, and they were annealed with the sinter process. It worked out really nicely. The fellow that was responsible for the circuit couldn't believe what was happening. He said he was going to make arrangements to get that into production. Then someone upstairs decided that we were going to get out of the TTL business, which we did.

We finally did get the implanted resistor into the process. Now anytime you need an implanted component in a circuit, you get it. They've got three implanters, and they're really tied into that process.

LÉCUYER: What was the time frame for the TTL circuit?

MANCHESTER: We were well along in that program around 1969, 1971. In fact, we had the accelerator's implanter already installed when we tried to get the resistor process into production. We offered them the opportunity to do it. It was a long haul, but we finally got over the hump.

LÉCUYER: The same thing happened in Silicon Valley. Many manufacturing people had no interest in ion implantation, and it took them five, six years to really get it and use it in production.

MANCHESTER: Yes.

LÉCUYER: Would you like to talk about the sale of Sprague to General Cable [Corporation] in 1971, the strike, and the downturn in the semiconductor business?

MANCHESTER: I don't think I can tell you too much about that. R. C. Sprague wanted to get out of the business. He was interested in just sitting back and feeling secure that Sprague Electric would still exist. Part of the deal would be that they would still maintain John and the system as it was, as John was president at that time.

As far as research and development, there weren't many changes. I was amazed. I was invited to go to their research and development department, and took Sid [Sidney D.] Ross, who was an organic chemist and one of the senior scientists at Sprague. He hadn't really been involved in semiconductors except for work in photoresists. We were ushered into the office of the director of research, a very plush office, and when we talked, we found out that the director of research at [General] Cable's [Corporation] main occupation is to make arrangements to take friends of the company out on a big plush boat. Never once did we hear anything about research projects. Sid and I thought that things were really going to go downhill for us if that was the condition.

Independent of that, General Cable did pretty good work in the area of silicon dioxide processing for fiberoptic cables for light transmission of electrical data. I'm not sure what happened to that. I did have a chance to talk to some of the people there in the lab, and they seemed pretty competent people. I also thought the project they were working on was viable. The fiberoptic work might have been something in which we could have been interested. We were investigating the formation of GaAs Diodes which could be used as transformers for electrical to light signals into the optics. We could have done something there. Strange lab. We didn't see much change in our direction, and we continued on with the programs we had.

LÉCUYER: What were the main orientations in semiconductor research at Sprague in the 1970s?

MANCHESTER: The early to mid-1970s was really a downslope for us, because that's when a decision was made that the semiconductor research should move to Worcester. That didn't fare too well for us, as some of the people in the research group didn't want to move out of the Williamstown area. What resulted was that four of us actually moved to Worcester: John Macdougall; Paul Roughan, a young fellow named Ole Tkal, and myself.

When we got to Worcester, we really didn't function anymore as a research group. We were "put out the fire" individuals for processes that were going on in the production areas. I did more chemistry in that time than I had really done for a while. Paul [Roughan] got involved in the packaging group. Sprague was doing its own packaging. In fact, we had all sorts of problems in that installation in Juarez, Mexico. [Paul] was given the assignment to straighten some of that out. John [Macdougall] got involved with the semiconductor process areas, which, in some cases, worked very well, because he got involved in really bringing the implantation process up to the level at which it should be.

I became involved in failure analysis. I firmly believed the root cause of most defects had to be related to a chemical reaction. I was instrumental in bringing the first scanning electron microscope into the section to look at and analyze failures, and I set up that group. I worked pretty closely with the quality control and reliability people, and also did some work in the packaging area with Paul. I continued on in that phase until I was asked to take over the

quality/reliability function, which I did. I saw a lot of deficiencies there, and I thought maybe I could bring some help.

That group had always been run by individuals who knew specifications and how to write them, but didn't understand what caused a failure. Since some of these people were good friends of mine, I don't say that to be nasty, I'm just saying that's basic. I think it's unfortunate that it should happen. When I took over one of the first things I did was close the plant down, because they were running a process which was introducing impurities. It was causing failures on the line. I just shut them down, and I got a lot of static for that, but we also got the process taken care of. We determined the source and eliminated it, and we put out some good product.

I did the same thing for IBM [International Business Machines Corporation]. I almost shut them down. They were a big customer of ours, and one of the products they bought from us was a switch called a 5712. Looking back at the records, we had a fairly high failure rate on that part. We received these failures and evaluated them, and they indicated, "You've got a corrosion problem." I got involved and evaluated some of them, and it was a corrosion problem.

Being me, I said, "I'm not here to provide a solution for the effects. I'm here to find out what's causing the effects." I did a pretty thorough search and determined, knowing a bit of chemistry about epoxy systems, since I'm an old Shell [Development Company] man, that the real problem was related to the die attach adhesive, which was an epoxy, a silver-loaded epoxy, which, as most chemists know, is loaded with chlorine or chloride. When we assembled the part and put it through moisture testing, this is specifically with plastic packages, which are permeable to moisture, we leached the chloride out, and it traveled around the frame and up onto the die, and it just loved that aluminum.

We did some experiments to prove that that was the problem and how it was happening. Then I called IBM to set up a meeting, and I said to them, "I'm not going to ship you any more 5712s." I said, "This is what we're going to do, and this is why we're going to do it."

I laid the whole thing out, the program that we'd run to determine what it was, and the solution that we had, and, boy, I never saw so many angry faces in all my life. IBM said, "You can't do that." They said, "You've got to do this." They've got a specific way of doing things, and independent of what the problem is, if it hurts them or not, they still want to do it that way. They said, "You've got to continue to ship and put up three or four lots of your new process, and we've got to fully evaluate that, and you've got to do such and such with it," and they're talking six months down the line to do that.

I said, "No, you're going to have to find yourself a new vendor." Well, we're the only ones that make that part. We left there with the agreement that we would ship them the new part, and they would evaluate them as quick as they could.



Boy, I guess it was a year or so later, I was talking to one of the guys in their quality department, and he said, “You sure had guts to tell us that you weren’t going to ship parts. That was the best decision that you ever made. You saved us millions.”

LÉCUYER: IBM remained a good customer.

MANCHESTER: I don’t think we make that kind of part, and I don’t think we have them as a customer anymore. I’ve got to be careful. I haven’t really been closely associated with that for a number of years. That was when it was still Sprague, before it was Allegro, and I don’t think Allegro ships them any parts. But that was kind of the way I ran the QC [quality control] department. Specifications are fine, but they’ve got to mean something. You just don’t write them to write them. It’s just the way I operate.

I stayed on there as the Director of Quality Assurance and Reliability until 1985. Then I was asked to take over the V.P. [vice presidency] of the capacitor end of Sprague research. I did that until I retired in 1989, and then I remained on as a consultant, primarily for Allegro, until 1996.

LÉCUYER: The semiconductor side, right?

MANCHESTER: Yes. Since that was the old semiconductor division, I stayed on with them until about 1997. After about eight years, I needed to take some time to really spend with my wife. She had been so good and really supported me all the way, so I decided that we would take some time off and do some traveling. We did a little bit, but unfortunately, I lost her.

LÉCUYER: Could we talk a little bit about chemistry and semiconductors?

MANCHESTER: The patent on the Hall element.

LÉCUYER: Yes.

MANCHESTER: I filed and received that patent (6). It was a patent for a device structure which allowed data transmission in a parallel method rather than series, which means that it had a number of Hall elements with built-in magnetic paths which lined up with multiple data sources to provide the magnetic fields for the Hall elements. I thought it was a good patent, but I don’t think they were using it.

There is an increasing number of chemists getting involved with the semiconductor industry, and that's a must. The semiconductor industry would have advanced faster than it has, had we had good chemists in place. There's a great deal of processing including that of surfaces, identification of contaminants in processing materials, different types of etching. To be honest, every step in the generation of a semiconductor relates to chemistry.

I would like to see more chemists in semiconductor production than we presently have. Semiconductor development is still a good area with lots of unaddressed chemical aspects. I would also like to see more of our technical people come out of school to the industry with a better approach to problem solving.

[END OF TAPE, SIDE 8]

MANCHESTER: The textbook approach doesn't allow for unanticipated variables. I'd like to see people come out with an idea of determining, then isolating a problem, and then developing the solution. The number of interacting parameters in semiconductor problems does not allow you to say, "This and this is going to mean this."

LÉCUYER: Do you think that chemists are especially well-positioned to approach things in that fashion?

MANCHESTER: I think so. If they received an initial education such as I did, they've got the tools. They will build on that foundation by applying them actively. In chemistry, you come closer to observing the real world. There is more hands-on education than with [electrical engineers]. My son, whom I'm here with today, is an electrical engineer. He presently is director of engineering for the design of power devices at Allegro. He does a great job, but his original experience was primarily bookwork, and he's now learning, sometimes the hard way, what really happens in the process of getting the function out without a problem. I truly feel that chemists, by the very nature of the discipline they're in, get some of that approach.

LÉCUYER: How do you explain the fact that semiconductor technology which was developed at first by chemists became dominated by electrical engineers?

MANCHESTER: That's a good question. I think that primarily relates to the fact that a process is a process, and it is of no value unless you can tie together that process and a device whose structure you create with it. That's why there are electrical engineers, because most of them originally were designers laying out functions and getting those functions into the semiconductor body.

LÉCUYER: In some ways the semiconductor industry is a chemical industry, but in other ways, it's not. That would be probably the lesson of this discussion. It is chemical because the processes are mostly chemical.

MANCHESTER: Yes.

LÉCUYER: But the product is not.

MANCHESTER: That's right, the product is not. But the function of that product can be very chemical.

LÉCUYER: Would you like to discuss non-chemists who made important chemical contributions to the chemical field? I noticed that in Silicon Valley many of the process engineers tend to be electrical engineers. They are trained as electrical engineers, but they do the chemistry.

MANCHESTER: That's the reason we need chemists there. When I came into the industry, the process engineers were saying, "Well, it says here in this book I make this solution by this, this, this," and they don't understand the basics. One of the things that really disturbed me initially was the fact that the designers would lay out a function with various transistors, resistors, capacitors, et cetera, and for each of those transistors there was a design rule that said, "You shall make it this way." You couldn't think, "Well, if we change that part and make it a bit like this, we will have a better parameter here." You cannot do that. That's not in the design rule. I think the reason for that is that they simply do not understand why that change is good or why the present design rules should always be used. Some of that has changed, but it's one of the first things I ran into in Worcester, in the background of the resistor implant that we talked about.

LÉCUYER: What was the impact of semiconductor technology on chemical knowledge and chemical techniques?

MANCHESTER: This technology is growing. It is a prime area to find what I'll call "problems to research." You can gauge the progress that we have made and say, "Yes, look where we've been. We put millions of transistors on a chip," but that's no different than saying, "I made a circuit that worked," forty years ago. To me there isn't a difference, because to get from there to here, a lot have changes have been made, and a lot of problems cropped up, and I think that there's always fresh area to work in.

LÉCUYER: If we tie together this discussion and the discussion on ion implantation, has the fact that you were trained as a chemist influenced or impacted your approach to ion implantation?

MANCHESTER: I think it did, because my chemical inquisitiveness led me to the first thoughts about ion implantation. Also, my background led me to the philosophy that one must always first define a problem, then isolate it, and then solve it. I couldn't confuse those things if I was to succeed. I've seen it over and over again that solutions devised to solve an effect lead to even a larger problem later on, because the source hasn't been addressed.

LÉCUYER: If I understand you well, the most important part of your chemical background was a way of thinking—a way of approaching problems rather than any particular technique or knowledge.

MANCHESTER: I think one's background and way of thinking lead to techniques that resolve the problem.

LÉCUYER: Would you like to make other comments on chemistry and semiconductor technology?

MANCHESTER: No, except that going back over fifty years has been an interesting process for me. I enjoyed it, and I hope it's been of some value.

[END OF TAPE, SIDE 9]

[END OF INTERVIEW]

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## INDEX

### A

Accelerators, Inc., 30, 32  
Allegro Microsystems, Inc., 25, 31, 36, 42-43  
Alton, Gerald D., 18-20  
Anderson, Frank W., 28-29  
Army Specialized Training Program [ASTP], 4

### B

Bakersfield, California, 5  
Bemidji, Minnesota, 1  
Bulova Corporation, 35

### C

Camp Abbot, Oregon, 4  
Chatfield, Minnesota, 1

### D

Detling, Kenneth, 13  
Djerassi, Carl, 11

### E

Eastman, Richard H., 11-12  
Extrion Inc., 25

### F

Fort McCoy, Wisconsin, 5  
Fowkes, Frederick M., 13, 15, 28

### G

General Cable Corporation, 39-40  
Gibbons, James F., 22-23, 35  
Gloton, Philippe, 20  
Grinnell, Stuart W., 12

### H

Hamilton, Ontario, 25  
Hewlett-Packard Company, 35  
Hutchinson, Eric, 9-10, 13-15

### I

Ion Physics Corporation, 18, 24-25  
Ion implantation theory, J. Lindhard, M. Sharff, and H. E. Shiott, 19, 21

### J

Johnson, Harold S., 11-12

### K

King, William J., 18  
Koenig, Frederick O., 11-12

## L

Lehovec, Kurt, 34  
Lompoc, California, 4  
Love, Leon O., 18, 20

## M

Macdougall, John D., 25, 28-29, 34, 40  
Manchester, Kenneth E., 1  
    Allegro Microsystems, Inc. 42  
        consultant for, 42  
    educational background, 2-3  
    father of, 1, 3  
    fraternity memberships, 8  
    G. I. Bill educational financing, 6, 11  
    influential teachers, 2-3, 6-7, 9-11  
    marriage, 5, 7  
    master's thesis, 9  
    military service, 4-5  
    mother of, 1, 3  
    patents, 42  
    Ph.D. Dissertation, 10  
    postdoctoral work, 10  
    recruited by Shell Development Company, 13  
    relationship with Accelerators, Inc., 31  
    relationship with Frederick M. Fowkes, 28  
    relationship with Robert B. Palmer, 37  
    relationship with John L. Sprague, 14-15  
    relationship with Robert C. Sprague, 27  
    research philosophy of, 26  
    San Jose State University, 5-8  
    Shell Development Company surface chemistry group, 13  
        leaves Shell Development Company, 14  
    son of, 43  
    Sprague Electric Company, 16, 18-19, 20-21  
        director of quality assurance and reliability at, 42  
        head of research and development at, 36  
        introduction of scanning electron microscope at, 40  
        ion implantation at, 28, 30  
        joins Sprague Electric Company, 14  
    Stanford University, 8-9, 11-12  
    transistors with variable thresholds, 34  
    wife of, 4-5, 14, 42  
May, Nathan C., 13-14  
Mississippi Valley Public Service Company, 1  
Moore, Gordon E., 8  
Mosher, Harry S., 11-12  
MOSTEK Corporation, 24, 28, 30, 32-35, 37-39  
    ion implantation system design, 32

## N

National Research Corporation, 18, 25, 29

## O

Oak Ridge National Laboratory, 18, 20, 22, 34  
Ogg, Richard A. Jr., 9, 11-12

## P

Palmer, Robert B., 32-33, 37  
Parks, George S., 9, 11-12  
Perrine, Richard L., 12  
Philco Corporation, 15

## R

Rensselaer Polytechnic Institute [RPI], 31  
Ross, Sidney, 40  
Roughan, Paul E., 40

## S

Sawyer, Webster, 13-14  
Sevin, Leon J., 32  
Sharif, Louay, 32, 37  
Shell Development Company, 13-15, 28-29  
    surface chemistry group, 13  
Shell Oil Company, 28  
Sibley, Clifford, 18  
Silicon Valley, 39, 44  
Sprague Electric Company, 14-16, 24-25, 27-40, 42  
    circuit production at, 16-17  
    effect of labor strike, 37  
    electronic beam evaporation, 18  
    facility in Carrollton, Texas, 37  
    facility in Juarez, Mexico, 40  
    facility in North Adams, Massachusetts, 19, 26, 32-33  
    facility in Wichita Falls, Texas, 35  
    facility in Worcester, Massachusetts, 32-33  
    ion implantation at, 17-20, 24, 27, 29-32, 34-35  
    ion implantation, design of system for, 17-19, 29-32  
    linear circuits, 38  
    packaging, 36  
    patents, 34  
    planar process for transistors, 15  
    PNP process, 17  
    product line in 1960s, 35  
    relationship with Accelerators Inc., 30-31  
    relationship with Bell Telephone Laboratories, Inc., 17  
    relationship with International Business Machines Corporation, 41  
    relationship with MOSTEK Corporation, 37  
    relationship with National Research Corporation, 29  
    resistor implants, 38  
    sale to Penn Central Corporation, 36  
    semiconductor processing area, 16  
    semiconductor research in the 1970s, 40  
    sensors, 36  
    silicon dioxide, growing, 16  
    staff, 15



Sprague, John L., 14-15, 27, 29, 32, 34, 39-40

Sprague, Robert C., 14, 17, 27, 39

Stanford University, 8

chemistry department, 9-12

facilities, 12

growth of, 12

## **T**

Texas Instruments Inc., 16, 32

Tostado, Claudio Alvarez, 15

## **U**

Universal Oil Products Company, 10

University of California, Santa Clara, 4-5

University of Southern California, 12

## **V**

Vogel, F. Lincoln "Linc", 15, 36

## **W**

Winona, Minnesota, 1-3

Wrigley, Chuck, 34