CHEMICAL HERITAGE FOUNDATION

JAMES N. SHOOLERY

Transcript of an Interview
Conducted by

Arnold Thackray and David C. Brock

at

Palo Alto, California

on

18 January 2002

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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James N. Shoolery, interview by Arnold Thackray and David C. Brock at Palo Alto, California, 18 January 2002 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0230).

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

JAMES N. SHOOLERY

1925	Born in Worland, Wyoming on 25 June

Education

1948	B.S., chemistry, University of California at Berkeley
1952	Ph.D., chemistry, California Institute of Technology

Professional Experience

	Varian Associates, Inc.
1952-1962	Director, Applications Laboratory
1962-1969	Marketing Manager, Analytical Instrument Division
1972-1990	Senior Application Chemist
	Consulting
1969-1972	Independent Consultant
1990-	Independent Consultant

Honors

1964	Sargent Award
1965	Award in Chemical Instrumentation, American Chemical Society, Division of Analytical Chemistry
1982	Anachem Award, Federation of Analytical Chemistry and Spectroscopy Societies

ABSTRACT

James N. Shoolery begins the interview by discussing his family background and growing up during the Depression. His interest in chemistry began in his childhood and grew further during his undergraduate years at the University of California, Berkeley. His education was interrupted by World War II, during which he served in the U.S. Navy as a radar technician in the South China Sea. Upon his return to the United States, Shoolery toyed with the idea of pursuing electrical engineering because of his experiences in the Navy, but ultimately decided against it. Shoolery decided to pursue a Ph.D. in chemistry at the California Institute of Technology and worked under Don M. Yost on microwave spectroscopy. After visiting an electronics show in Los Angeles, California, and seeing their exhibit, Shoolery wrote to Varian Associates, Inc. about the possibility of his coming to work there on applications for nuclear magnetic resonance. He joined Varian Associates, Inc. in 1952 and spent nearly forty years working there. Shoolery shares his impressions of Varian Associates, Inc., its management, its products, and his pride in having been able to follow the development of NMR for such an extended period of time. Shoolery concludes the interview with a discussion of his life outside of Varian and shares some final thoughts about his career.

INTERVIEWERS

Arnold Thackray is President of the Chemical Heritage Foundation. He majored in the physical sciences before turning to the history of science, receiving a Ph.D. from Cambridge University in 1966. He has held appointments at Oxford, Cambridge, Harvard, the Institute for Advanced Study, the Center for Advanced Study in the Behavioral Sciences, and the Hebrew University of Jerusalem. In 1983 he received the Dexter Award from the American Chemical Society for outstanding contributions to the history of chemistry. He served on the faculty of the University of Pennsylvania for more than a quarter of a century. There, he was the founding chairman of the Department of History and Sociology of Science, where he is the Joseph Priestley Professor Emeritus.

David C. Brock is Program Manager for Educational and Historical Services at the Chemical Heritage Foundation in Philadelphia. He is currently a Ph.D. candidate in the History Department, Program in the History of Science at Princeton University. In 1995, Mr. Brock received his M.A. in the History of Science from Princeton University and in 1992, he earned a M.Sc. in the Sociology of Scientific Knowledge from the University of Edinburgh.

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INTERVIEWERS: Arnold Thackray and David C. Brock

LOCATION: Palo Alto, California

DATE: 18 January 2002

THACKRAY: Jim, please tell me about your family background.

SHOOLERY: My father was born around 1891, and raised in Alton, Illinois. He grew up there, but left that area when he was a young man and went to Denver to find his fortune. His mother was very dependent on him because his father had died, and his youngest sister had had an accident that ruptured her spleen. When his daughter died, my grandfather was so broken up over it that he lost his livelihood. He was a photographer who died at a fairly early age of pneumonia. That left my father responsible for my grandmother. My father was an only son, and after his father died, he had no relatives left on that side of the family. In addition, he had several uncles who were wastrels, so an awful lot was expected of him. He felt like the only way he could really keep going was to move to Denver and take his mother with him.

My mother was born in Colfax, Washington. She had a very pleasant childhood. Her mother was a schoolteacher, and apparently just a wonderful woman, but she died when my mother was just eighteen years old. In a way, my mother became responsible for trying to keep her father moving on with his life. She managed to attend two years of college at Washington State University, and eventually went on a trip to Denver to see her brother at the Colorado School of Mines. While she was there, she happened to meet my father, who proposed to her before she left Denver; they married very shortly thereafter. My parents lived in Denver for a while until a better opportunity arose in Worland, Wyoming.

My father—who had not actually completed high school, but had worked at various jobs and learned something about accounting—became the office manager and accountant at a sugar-beet mill in the wilds of Wyoming. My parents had lived in Wyoming for about two years when I was born in 1925. As a result of my birth, my mother developed an infection that could not be treated locally, so she was put on a train and sent to Portland, Oregon. She had developed a lung abscess, and in those days there were no antibiotics, so it took her five months to become stable. So there my parents were—they didn't have a lot of money, my mother was out on the coast, and my father was trying to earn money in Wyoming while taking care of me. Fortunately, my father still had his mother. She was essentially my other parent for the first five months of my life.

The survival of my mother was seemingly miraculous. When she finally left the hospital she weighed 95 pounds and had lost all of her hair. Before she had left, the doctors took her to a group of medical students and told them, “We just want you to see that she was a hopeless case who is now going home.” When she came home, she was very worried that I would not accept her as a mother, but she received an absolutely wonderful reception. When she came in the door, I raised my arms, held them wide, and smiled. [laughter] From that point on, our lives were much better. But having been that ill, she couldn’t handle the altitude any longer, so my parents decided to move back to the state of Washington where my mother had relatives.

My father worked in the city hall for a while, but eventually it began to seem that the opportunities were too limited in such a small town. My parents moved us back to Portland where my father worked in a larger company, again in the office. That company did fine until the stock market crash of 1929. Actually, he had been sent from Portland to a branch in Oakland, California, and then the Depression hit. The company had to cut back, and since he was one of the newest employees, he was let go. My father spent the Depression going from one period of unemployment to another, with a few in-between jobs. My mother also did somewhat similar things during that time period. It was a very tough time for them. Of course, being about five or six years old during that time period, I didn’t understand what was happening, and my parents did a marvelous job of preventing me from really seeing how bad things were. They always kept a positive attitude in my presence and kept life going in a pretty normal fashion.

During my childhood we couldn’t afford enough good food for the family. As a result, I had all the childhood diseases, in addition to being malnourished. When I was seven years old, I had to spend a year at a facility that helped children who were at risk of developing further illnesses. When I look back at that time period, it was very difficult to be away from my parents, even though they visited me every week. We were not that far apart, and they did have a car, so it could have been a lot worse.

THACKRAY: What type of facility was that? Was it state-owned?

SHOOLERY: I think it was a state run sanitarium for strengthening people who were at risk of developing diseases like tuberculosis. If one becomes too weak, that’s when disease can take hold. My doctor and my parents were afraid that might be happening to me. After that year of rejuvenating my body in the sanitarium, I went home and never had any more trouble. It was a very tough experience, but it helped me become more independent and self-reliant. Most things work out that way. If one looks at the good parts of one’s life, one will see that they are tangled up with some of the things that don’t seem so good.

When I was released, my parents moved from Oakland to Berkeley. By that time period, the Depression was coming to an end, my father had found work, and my mother had taken a job. I was pretty independent most of the time because both of my parents worked, and in those day’s childcare was not what it is today. I think it was a safer world. My parents never really worried about me. They raised me well and trusted that I would not get into trouble; and I didn’t. They were just wonderful parents in the way that they provided guidance and example, but they didn't sit on me and dictate my decisions. I made pretty much all of my decisions as to what I did every day, and I just got used to doing that sort of thing. I think that has had quite a lot to do with my attitude throughout most of my life; throughout the time I was at the University of California [at Berkeley] and then at Caltech [California Institute of Technology], and then finally when I entered industry at Varian [Associates, Inc.].

THACKRAY: Your father's name sounds like it is German in origin. What was your mother's maiden name?

SHOOLERY: My mother's maiden name was Cairns, which is a Scotch name, but she was only 50 percent Scotch. Her family came to America very early in its formation, and so had my father's family, but we were never able to trace his family back more than about three generations because there was a string of only sons that came down for a number of generations. The name Shoolery was corrupted from a Germanic name. The "c" was dropped, so it may have started out "Schooler," but it turned in to "Shooler." I don't know why they put the "y" on the end, but that's the way it worked out.

THACKRAY: You said your grandfather was a photographer, which has a connection to science, but your ancestors were primarily farmers, correct?

SHOOLERY: Yes. If you trace back another generation, they were probably farmers. I think that there was probably more farming in Indiana than in Illinois, but because of the series of only sons, we can't trace it.

THACKRAY: Was your family religious?

SHOOLERY: Yes, my parents were always church members, and they were rather ecumenical. They went to whatever church they thought would convey a message that was congenial for them. When I was a very small child in Portland, Oregon, we had attended a Presbyterian church, but when we moved to California, we tried several churches. We were members of a Methodist church, and then finally a community church, which was where they spent the longest time period. I was raised in the Protestant religion.

THACKRAY: Has religion continued to be a meaningful part of your life?

SHOOLERY: I consider myself a religious person even though I have attended church infrequently in the past. Although I am a student of science, I'm unable to mentally separate the essential truths of religion from the necessary facts of experimentation. I need something that I can synthesize; something workable; but I remain conscious of the fact that there's a lot more to human beings than logic and rationally. Interestingly, my wife is definitely a religious person. She was raised in the Lutheran church; and though she has attended church infrequently in the recent past, she had been very devoted while she was attending college. Recently, our church attendance has improved, and we are actually planning to become members of our local Methodist church. As I have matured, I came to realize that religion should always play an essential role in one's life and one's family.

THACKRAY: Describe your pre-college education and interests.

SHOOLERY: I really enjoyed school. I was a decent student, and I received good grades and plenty of honors all the way through. I tended to make pretty close friends with certain teachers, because if I really admired the way they taught, I would ask them a lot of questions. After having got to know me, they would invite me over to their houses; though I usually made a pest out of myself because I tended to show up at their houses without having been invited. I think the teachers simply put up with me rather than sending me away. As a result, some of my closest friends in high school were teachers.

For instance, I was close to the chemistry teacher that introduced me to photography, which I instantly fell-in-love with after I saw a photograph magically resolve itself when it had finished developing. I spent a lot of time at my chemistry teacher's house, where I used his dark room to learn about photography. I was also close with my physics professor and my mathematics teacher, who was a single woman. She was a very charming person, very cultured, and I got a lot out of my relationship with her. Our conversations taught me a lot about other aspects of life besides mathematics. She also introduced me to ice-skating, which I enjoyed doing for a number of years.

My experiences in school were very positive. I thought of school as a great place to be with good people who were really trying to teach me something. I found it relatively easy to meet the school standards and earn good grades, which encouraged me to work harder.

THACKRAY: Were you in Berkeley during that time period?

SHOOLERY: Actually, my family lived in Albany, which is right next to Berkeley, and about 5 miles away from the University of California.

From an educational point of view, my experience in high school was also very positive. I skipped a couple of grades, so I was small for my age and socially immature when compared with many of my older classmates. My inability to relate to the older students drove me to focus on academic activities rather than social activities. Still, I had a difficult time forming a confident self-image during my high school years.

Luckily, I always had one very close friend. Somehow, I would always meet someone, become instant buddies, and spend almost all my time with him or her. Usually, I found someone whose complementary personality would allow us to explore life together rather than argue all of the time. Ultimately, he or she would move away, and then I would have to find another friend. I don't tend to make a large circle of friends. It's not that I'm a misanthropist; I'm just not very gregarious! [laughter]

Focusing very hard on school was something that was rewarding for me; as was the approval, support, and encouragement from my parents. My parents and I always assumed that I would go to the University of California, which was a publicly-supported university, because we were lower-class. After I had graduated from high school in 1941, I began my freshman year at the University of California in 1942. During that time period, university fees were thirty-seven dollars and fifty cents a semester; or seventy-five dollars a year. Luckily, my house had been only five miles from the University, so I was able to live at home while I attended classes. Relieved of any housing expenses, my only pressing disbursement was the cost of books.

BROCK: Did you attend public school while your family was living in Oakland, California?

SHOOLERY: Yes. We moved to Berkeley while I was still in grade school, but my first few years of education were in Oakland. I had also spent one year in the sanitarium's school, where I actually skipped a grade. I finally finished grade school in Albany and went directly from sixth grade, which was the end of grade school, in to seventh grade in the high school. The high school contained grades seven through twelve, something that's not very common anymore.

In retrospect, the seventh through twelfth grade high school system is much better than the contemporary system. When a student in that system became a seventh grader, he would be at the bottom of the totem pole and then have to work his way up through five grades before becoming the oldest in the school. In that way, the student socially progressed in a linear manner that was much smoother than the contemporary system.

THACKRAY: When did you first get involved with science?

SHOOLERY: I have always been fascinated by science. When I was about eight years old, my parents took me to their friends' house for dinner. When we had finished eating, and the adults had gone into the living room to talk, their friends' son took me into his bedroom and showed me his chemistry set. As I remember it, he was very dramatic in his presentation. He closed all the windows and the door to make his room dark, and then he lit the Bunsen burner, which illuminated shadows that flickered against the walls. Of course, the experiments we did were elementary: changing the colors of substances by placing them in the flame; but I was fascinated.

I fell in love with chemistry. I remember thinking, “That is what I want to do for a living. I want to study chemistry and eventually become a chemist.” From that time on, if anybody asked me, “What are you going to do when you grow up, little boy?” I would say, “I’m going to be a chemist.” I never deviated from my desire to become a chemist, even when I got to high school and learned that there’s more to chemistry than just the fun experiments. By that time period, I knew that I had to learn all the aspects of chemistry to be able to run the most complicated experiments. Therefore, I had decided that I would continue studying chemistry until I became disenchanted with it, and if that ever happened, I would figure out my next step from there.

BROCK: After having played with that boy’s chemistry set, did you ask your parents for one of your own?

SHOOLERY: When I was young, I had a couple of chemistry sets, but I never really enjoyed experimenting with them. [laughter] After I was in high school and had learned more about chemistry, I started doing my own chemical experiments. I always went to the library to check out books on organic chemistry so that I could learn on my own.

[Joseph W.] Mellor was one of my favorite authors. He wrote, *Mellor’s Series of Chemistry* (1). Using his books, I would select elements and then read all about them in his text. Sometimes, I did the experiments in the books as well. For example, one day I decided to do the thermite experiment, and I mixed up aluminum powder and manganese oxide in a crucible to make a manganese oxide. After I finished mixing, I put a strip of magnesium ribbon in the crucible with the manganese oxide and ignited the concoction. The resulting flame was very impressive, and the entire mixture glowed for quite a while. From that point on, there was a little hole in the cement floor in our basement where the heat had actually fragmented out a piece of the concrete.

My parents allowed me to do those kinds of experiments in the basement, and they never questioned whether I was doing anything unsafe. Actually, they thought it was funny that I burned a hole in the cement floor. [laughter] They always trusted me, and that did a lot for my self-esteem. Of course, there’s always a risk in all of life’s experiments, and fortunately I never came to grief with any of mine; though I did come close a couple of times.

As time passed, I became fascinated with experimentation, and I spent much of my time thinking up new experiments. For instance, after making the manganese metal, I dissolved it in acid, and made various manganese precipitates. All things considered, I had a wonderful time exploring that stuff.

THACKRAY: Talk about the campus and your undergraduate experience at the University of California.

SHOOLERY: During that time period, Berkeley had one of the best chemistry programs in the country, unlike their contemporary program, which has been outshined by Stanford University's chemistry program. When I arrived at Berkeley, I was very excited to meet the professors in the chemistry department and to learn who I could be studying under.

[END OF TAPE, SIDE 1]

SHOOLERY: [Joel] Hildebrand lived to be one hundred years old and went to the lab every day. When I was there, he was in his seventies. He gave all of the freshman chemistry lectures in a huge, five hundred seat lecture hall. Even though he was old, he was a real showman. He'd put on impressive demonstrations. He was also a very exacting teacher, so his lectures were very, very complete. He would make sure that he covered all the material, and then he would test us on it.

From my college experiences, I learned that Nobel Laureates who teach have a much different style than regular professors. Their lectures seem to flow easier because they have such a mastery of the material. Further, Nobel Laureates are able to embellish the material with interesting tidbits of information to make the lecture interesting and emphasize the essential points of discussion. I wouldn't say that every Nobel Laureate is a good teacher, but most of the ones that I've had any contact with have been exceptional. To be exposed to those people and the way they think does have an effect on how one thinks about science and scientific methods. It was a great privilege to have gone to Berkeley where those people taught my courses.

THACKRAY: When you speak of Nobel Laureates at Berkeley, do you mean G. N. [Gilbert N.] Lewis specifically?

SHOOLERY: No. Unfortunately, I arrived at Berkeley just after his death, so I never knew G. N. Lewis personally. However, I owned his book, *Thermodynamics*, for a long time (2). In fact, there's a copy of G. N. Lewis's book at the bottom of the South China Sea because of an experience that I had during my military service. [laughter]

THACKRAY: So what are the names of some of the other lecturers you had at Berkeley?

SHOOLERY: I took a course on thermodynamics from Professor William [F.] Giauque, who was a low temperature chemist and a winner of the Nobel Prize. Not all of my professors had Nobel prizes, but some of them were absolutely outstanding in their fields. For instance, Melvin Calvin was a professor that I enjoyed very much. He hadn't yet received his Nobel Prize when I took a course from him. He was always organized and presented the material so beautifully that I was inspired. He was also very tough. His exams were really hard, but I learned a lot from his class. Also, I had an inorganic chemistry professor named [William C.] Bray. He was a very famous inorganic chemist who has written many books on the subject. What I remember most about him were his piercing blue eyes. When he was my professor, he was all ready quite old, but his eyes were a luminous blue. When he would fix them on me, I thought that he could see right through me, which really made me nervous when he would ask me difficult questions. No sloppy or evasive answers would get past him. If I tried, he would simply ask me another question.

THACKRAY: Was Glenn [T.] Seaborg a professor there?

SHOOLERY: Yes. I took one of his courses, but during that time period he was involved with atomic bomb research and was rarely available to give lectures. He turned the lectures over to one of his assistants who was a well known radio chemist.

THACKRAY: There was a spectacular array of people at Berkeley.

SHOOLERY: Yes. Berkeley was really a powerhouse. I really enjoyed my time there. I completed two years and was starting on my third when those of us who were in college on deferments were told there was a very high probability they would not be renewed. That was around 1944. The government was scraping the bottom of the barrel, and they told us that if we wanted to pursue something that would take advantage of our education, we should go to San Francisco [California] and take an examination called the "Eddy Test." They gave us information on how to do that and I went to San Francisco.

THACKRAY: What "Eddy" did that test refer to?

SHOOLERY: That was Commander [William C.] Eddy in the [United States] Navy, who had been trying to recruit people to be radar technicians. If a person passed the "Eddy Test," he was sent to boot camp, and then to a thirty-day first step that half of the people failed. Afterwards, that person was sent to a three-month training school where he began to learn the principles of electronics. During the training school, he worked on the construction of electronic circuitry and took courses in the mathematics of electrical engineering. From there, he went to a third school that lasted a further six months. There he finished learning about electronics and mathematics by working in troubleshooting labs at real radar sets. If he didn't fail those tests, that person was sent out to sea as a radar operator. The program had a very high failure rate because the instructors wanted to make sure that they had qualified people. Therefore, the instructors would constantly throw new problems at their students to see if they could weed out the students who couldn't meet their challenges.

After I took and passed the "Eddy Test," all that I needed to do was to get through the physical exam. The Navy ran the physical exam by putting everyone in a line and having them walk past a series of stations. At each station, a doctor would ask me a question or listen to my heart, and so forth. During my physical, I remember that the very last station was the eye exam station. The doctor there had me take off my glasses and then asked me to read the top line on the chart in front of me. Being nearly blind without my glasses, I said, "What chart?" [laughter] As you can imagine, I didn't pass the eye exam, but I did pass everything else.

After my physical, I was sent to the assigning station. At that station, a man sat at a desk with my papers and two stamps: one said [United States] Army, and the other said, Navy. He looked over my papers and said, "I see you don't meet the requirements for the Navy, so I guess you'll be in the Army." He picked up the stamp and was ready to bring it down on the paper, when I said, "Wait!" It was just one of those impulsive things. I didn't want to be in the Army. He stopped, and he looked at me, and I said, "Isn't there something we can do about this?" He said, "Why don't you go back and take the eye exam over again?"

So I walked back, and there was a nice doctor, probably in his fifties, waiting there, and I said, "I didn't pass the eye exam, and it's really important for me to get into the Navy because I want to be in the electronic radar training." He said, "All right, we'll do it again. Now this time, walk up toward the chart and we'll see how close you have to get." I walked up to the chart, squinted, and did everything under the sun to improve the situation. Apparently, I did well enough because he finally stamped my exam "Passed." I went back to the assignments desk and got the "Navy" stamp, thinking, "That was a close shave!" [laughter] If I would have accepted the "Army" stamp, I probably would have been unable to use my skills to the best of my ability. I might have been placed in a foxhole somewhere, and I felt that I'd rather be on a ship than in a foxhole.

BROCK: Were there a lot of technologically-oriented students trying to get in to the radar unit?

SHOOLERY: The only people who would have tried were people who had partially completed their science training because it was a tough test. Those who took the test probably had similar backgrounds as me, though there might have been other routes in to the unit. For instance, I suspect that career Navy men might have sent their kids to take the test and informed them of what was on it beforehand. I think most of the test takers were previously college students.

THACKRAY: Did anybody else from Berkeley join the radar unit with you?

SHOOLERY: Yes. William [Bill] H. Reynolds, one of my best friends at Berkeley, joined the unit with me. We were both freshmen in 1942 when we decided to join the chemistry fraternity, Alpha Chi Sigma. Once in the fraternity, we became good friends. Reynolds was basically an extension of me. Whether we were in classes, labs, or even at the fraternity, Reynolds and I were inseparable. I remember periods after lunch where, if we didn't have a heavy class schedule, we would go into the recreation room and play ping-pong for hours. He was a very good ping-pong player, and in trying to compete, I also became very good. We had some absolutely marvelous afternoons.

THACKRAY: Are you still in contact with him?

SHOOLERY: Yes. We've been friends since 1942. Sadly, sometime last year he had a heart attack. He had been having chest pains that he thought were indigestion. He had always been a very active person, so he never had worried about his heart. When his indigestion got worse, his family had tried to talk him in to going to the hospital, but he continued to insist that he was healthy. Finally, one morning at three o'clock, he had to call his son to take him to the emergency room. By then, his heart attack had gone on for too long, and his heart was really damaged. Since then, he has numerous heart problems, and doctors think that he is going through congestive heart failure. I try to call him regularly at his home in Houston, Texas, and he seems to be doing all right. Luckily, he's in one of the best heart care facilities in the country. Though both of us are in the second half of our seventh decade, I'm hoping that he'll hang on a little while longer. [*ed. note: William Reynolds passed away on 2 March 2003*]

THACKRAY: Did he have an industrial career?

SHOOLERY: Yes. Our careers paralleled in many ways because we both took the "Eddy Test." We both got into the program. We had gone to boot camp together, as well as to Wright Junior College in Chicago [Illinois] for the one-month screening period together. We had to ask where we wanted to go and then they drew it by lot. We were both selected to go to the Del Monte training school in Monterey, California for three months. Then, we both asked for Treasure Island in San Francisco, and we were drawn to go there. So we spent the entire year of training together in the same classes, and then were assigned to the same troop ship headed out to the Pacific. We both ended up in Okinawa [Japan] for reassignment, and only then did we get separated on to different ships.

When we finished our tours in the Navy, we both went back to Cal [University of California] and finished our courses together. I went on to graduate school and he was hired at the Shell Oil Company. He made a very good career for himself in the Shell Oil Company and became a much respected advisor to the top management in Houston. He retired about the same time I did. Though he stayed in Houston, we frequently visited each other over the years. Another similarity between us is that we both took up ice skating. We spent a great deal of time ice skating together, and that became a very important part of both our lives.

THACKRAY: Describe your experience in Okinawa. When did you first arrive there?

SHOOLERY: It took sixty-six days aboard a troop ship to get to Okinawa. It was very difficult to amuse ourselves for sixty-six days aboard that ship, be we managed. After I arrived in Okinawa, I was placed in a relocation camp for about two weeks until the Navy decided where I was needed most. Bill was assigned to a fairly large repair ship for electronic servicing. He was lucky because his assignment took advantage of the full range of skills that he had acquired during his year of Navy training. I was assigned as the radar technician to a small tanker. After receiving our assignments, Reynolds and I didn't see much of each other during the rest of our time overseas.

When I arrived in Okinawa in 1945, there were about one hundred other ships that were gathering in order to invade Japan in September or October. After one week of being docked, only one Japanese plane had flown over and attacked our fleet. It was a kamikaze plane, and it actually struck a cruiser that was about two miles away from my ship. Of course, every ship was at battle stations when the kamikaze arrived, and I was sitting in the communication center with the radios and the radar. It was a very brief experience because as quick as the plane appeared, it slammed in to the cruiser, killing a few sailors. That was the only action I saw during the War. The following week, the first atomic bomb was dropped on Hiroshima [Japan], and the Japanese surrendered a few days afterward.

In September of 1945, a gigantic typhoon built up in the South Pacific and went right over Okinawa. It was a major typhoon, maybe a force five. When the typhoon hit the island, there were numerous ships docked together in preparation for the upcoming invasion of Japan. Had the War not ended, that invasion would not have happened because those ships were dragging their anchors. They were all crashing into each other in the lagoon, and the damage that those collisions caused would have delayed the military action for a very long time period. The captain on my ship had had a lot of experience in stormy weather, and he decided that it would be safer to take the ship out of the harbor rather than risk colliding with the hundreds of other ships, many of which were a lot bigger than ours. In retrospect, I think he made the right decision because a lot of the ships that remained in the harbor were seriously damaged by the storm. In addition, the shore was not much safer than the ocean because pieces of corrugated metal from Quonset huts had flown through the air and destroyed everything.

In any event, he took the ship out through the mouth of the harbor. I couldn't even figure out how he found the mouth of the harbor with what was going on. As we came out into the open sea, I couldn't believe what I saw. The waves were higher than the mast, and the mast on this ship was at least a hundred feet high. The ship would climb one gigantic wave, barely top the crest, shoot down the opposite side, and then the prow would disappear under the next wave. Faced with an advancing wall of water, the ship would struggle to get its prow above the surface just in time to be lifted over the next wave. That process repeated over and over, and each time I wondered, “Will this ship hold up under this kind of stress? Is it built that well?” It was a tanker, and it had a lot of stability because it was carrying a full load of oil that forced it to sit low in the water. The oil also increased the weight of the ship dramatically. As we went over the tops of the waves, the center of the ship would float and the ends would jut out in the air, causing the ship to flex every time we went over the crests.

After about three or four hours, the flexing of the ship actually broke the hydraulic line that connected the steering wheel on the bridge to the steering engine room. The line was essential because it activated the steering engine that moved the rudder on the ship. Fortunately, the designers of the ship had thought that the hydraulic line might break in rough seas, so they put a second wheel in the steering engine room that could be manned by a sailor with earphones. When the hydraulic line broke, the captain was forced to send compass bearings to the sailor in the steering engine room, who then steered the ship and kept it headed into the waves.

After the flexing of the ship broke the hydraulic line, we were worried that it might also break the electrical connection to the steering engine room; in which case there would be no way to keep that ship headed into the waves and it would almost certainly sink. It was a very nerve-wracking situation, but there was really nothing we could do. The captain made me sit in front of the radar, which was pointless because during any large storm the only thing that a radar operator could see was “sea rerun,” or the reflections of the waves on the radar screen. In addition, I was terribly seasick, and since there were no seasick remedies during that time period, I was forced to hold a bucket between my legs while I sat at the radar station. There was really no reason for me to observe the radar, but the captain did things by-the-book, and Navy regulations stated that whenever the crew couldn’t see visibly, a person was required to observe from the radar station.

The ship pitched and rolled so much that everything in my small office was thrown about. Some of the textbooks that I had brought to study while at sea were ruined when they were thrown on the floor and a battery broke open on top of them. One book that was more or less converted into mush was *Thermodynamics* by Lewis and Randall (2). After the storm was over, I dropped the Lewis and Randall textbook over the edge of the boat. I even said a little prayer to it. It’s still out there, and because of my donation to the sea, there are probably some very educated fish over there! [laughter]

Anyway, that typhoon was really a monster and those waves were huge. The winds blew over two hundred miles per hour before they blew the anemometer off the top of the mast. There were several other ships, destroyers I think, that foundered and sunk in that storm, and we were pretty worried that we were going to join them if the ship kept flexing as bad as it was. Every time I went down below decks and tried to get some sleep, I had to wind my arm around the chain that held the row of bunks together to keep from being rolled out of my bed. I’d lie there and try to not pay any attention to the fact that the rivets were making popping sounds. I would think that every time we went under one of those waves, the whole thing might just come apart, and the whole prow of the ship might just fill with water. I thought, “Well, that’s a good quick way to go, and so there’s no point in thinking about it anymore.” [laughter] That was really the attitude that came into my mind. I thought, “I am absolutely powerless to do anything. I am at the mercy of whatever happens; therefore I may as well not do anything except focus on what I’m supposed to do.”

We went through the eye of the typhoon. It’s true that the eye of a typhoon can be ten, twenty, maybe even fifty miles in diameter of eerily calm water. While in the eye, the ship was sailing along very nice and level, but off in the distance I could see that we were surrounded by the big black walls of the storm. It was so odd to be surrounded by a dark and powerful storm and still have blue sky overhead. After our boat crossed the eye of the storm, the wind picked up from the other direction just as strong as it was before. Altogether, we were in very heavy seas for nearly three days. Then for another day or so we were in very choppy water, trying to work our way back to Buckner Bay. In those three days, I really grew to respect nature.

THACKRAY: When did you get out of the Navy?

SHOOLERY: Following that typhoon, everything in Okinawa went back to normal. The Navy began to send people home, starting with those sailors who had been in Okinawa the longest. I knew I’d be out there for quite a while. My little ship was assigned to follow around a group of minesweepers and provide them with fuel. It would take on a load of fuel and then go somewhere and sit in a bay while other boats went out and swept for mines.

[END OF TAPE, SIDE 2]

SHOOLERY: There was very little for me to do because the communications load was not severe and we rarely needed to use the radar. We didn’t travel much at night. We spent most of our time at a mooring in a bay where we could provide the fuel for the minesweepers. We left Okinawa pretty quickly, and from there we went to the island of Hainan in the South China Sea. During that time period, Hainan was a very remote place, with very little sign of human habitation. Since we weren’t trying to make any contact with the Chinese, we moored near a part of the island where there were no nearby towns or cities. From there we could go ashore to play volleyball and drink beer on the beach. That was about all we did for the better part of a month until we had used up our fuel. Then, after we had refueled, the Navy sent us to Hong Kong.

I actually got to see the city of Hong Kong right after the War, when it had a million refugees living on the streets. The city had not suffered a lot of damage. It appeared to have been bombed lightly, and there had been a lot of vandalism because so many property owners had evacuated Hong Kong, leaving their property unprotected. My visit to the Cantonese part of China was very interesting because I got to see the Chinese civilization in its seemingly preindustrial state, before the arrival of communism. It was my first trip to a foreign country, and it was about as foreign as I could get.

From there, our ship went to Shanghai [China] and spent quite a bit of time in Shanghai Harbor fueling the minesweepers. I was appointed mailman for the ship because I wasn't involved in duties that required seamanship. Every day I would have to get in to a little landing craft and cruise across the harbor to the mail ship. When I brought the mail back to the ship, I would have to distribute it to the crew, which kept me busy.

Our ship's crew was given frequent leaves while we were in the Orient. We might get to go ashore two or three times a week; sometimes for a whole day, and sometimes just for the evening. I was amazed that most sailors' idea of a good time was to find another group of sailors to fight with! [laughter] In all reality, the English were more aggressive at it than the Americans. I was not looking for any fights, and so I generally didn't go around with a group. I'd get one friend and we would go and have our own adventure.

It was a very educational trip. While I was on the ship, I became friends with a very interesting guy. He was a rather artistic college guy who was interested in architecture. His goal, which I believe he actually accomplished after his service was up, was to study architecture with Frank Lloyd Wright. While we were aboard the ship, he outranked me. He was a Boatswain and I was a second-class radar technician. We were very good friends though, and we did a lot of things together. He was actually much more interested in cultural and intellectual things than most guys in the Navy, so we were able to talk to each other and discuss philosophical ideas, politics, and so forth.

One of his most prominent characteristics was his very strong libido. Luckily for him, Hong Kong was full of refugees who were willing to do anything to make a living. It had a seemingly endless number of brothels. Whenever we went on shore leave, those were the first places he wanted to go. I wasn't interested in those activities myself, but I also didn't want to get separated from him, because I didn't know how easy it would be to meet up again. So I'd go to the brothels with him and wait for him to finish. [laughter] I would sit in the lobby or the waiting room. Meanwhile, women would be walking past with their bowls of hot water and everything else, in various states of dress or undress, but they never really pushed me. They would come and see if I was interested. I would be nice to them and say, "No, thank you," and continue to wait. When he was finished, we would go out for the rest of the trip. [laughter] But I got to see the inside of those Chinese brothels, and that experience in itself was awkwardly educational.

After our brief tour in Hong Kong, my ship was sent to Shanghai. Shanghai was much bigger than Hong Kong, but it had been blasted in to terrible shape after some intense attacks by the Japanese military. I was able to walk around the city, and I got a sense of what Shanghai had looked like before the War.

After only a brief stay in Shanghai, my ship was suddenly sent to the Subic Bay in the Philippines, which was fifteen hundred miles away from Shanghai. Apparently, the Navy had made a mistake in sending us to the Subic Bay, and after a week of straightening things out, our ship was reassigned to Qingdao, China, three hundred miles north of Shanghai. Qingdao was a beautiful city. Located on the Shantung Peninsula across from Korea, Qingdao was a German port city when trade in China had been divided between England, France, and Germany. As a result, Qingdao was and is famous for the beer they brew there, which is still served in restaurants all over China and is exported to other countries. I must admit, the beer in Qingdao was excellent, and it made our stay in that city all the better.

During my stay in Qingdao, the city was preparing for a seemingly unavoidable conflict with the Chinese communists. By that time period, the communists were very powerful in Northern China, and their eventual takeover of the South had been anticipated. While touring Qingdao, I could hear the gun-bursts from distant battles between the communists and the nationalists; so they weren't that far from the city. Upon our arrival, we learned that Navy officials had decided to decommission our ship and give it to the Chinese nationalists. I remember thinking, "We'll never see that ship again." To decommission the ship, we had to take inventory of everything, which took about two months. Afterwards, we handed the ship over to the nationalists.

While our ship was in Qingdao, the crew had been allowed to go on shore leave, where the usual recreational activities were available. In addition to the activities run by the Chinese, there was a golf course run by the U.S. Marines. That golf course was like nothing I'd ever seen before. It was a nine-hole course with greens that were equivalent to the roughs of an American course, and roughs equivalent to a complete jungle. [laughter] To play the course, I was allowed to choose only one golf club, and allowed to use only one ball. I always chose the five-iron because I thought it was similar to a cross between a driver and a putter. The course ball that I was allowed to use had cuts all over it from frequent use. Every time I drove the ball down the fairway, I would hear a humming sound as it curved off in one direction or another. [laughter] I shot one hundred and twenty after eighteen holes, which I thought was pretty good considering I wasn't a regular golfer.

BROCK: When you handed over the tanker to the Chinese nationalists, did you also give them the radar set?

SHOOLERY: Yes. By that time period, the radar set wasn't considered a classified instrument.

BROCK: After your crew was told to hand the ship over to the nationalists, did you ever see it again?

SHOOLERY: Surprisingly, yes. Many years later I read about it in the newspaper. It was sitting in a mothball fleet in Benicia [California], around the north arm of the bay. Before it was to be scrapped, sailors had brought it to the bay and moored it with a couple of other ships that were also headed in the same direction. I tried to visit my old ship, but I wasn't allowed in to the base. The fence was a mile away and locked, and there were no officials from whom I could request permission to get aboard my ship. It was pretty far out in the bay, but I was able to use binoculars to get a decent look at the hull. It may still be out in the bay, or it may have been scrapped by now.

THACKRAY: How did you get back to the United States?

SHOOLERY: As soon as we finished decommissioning our boat, we sailed back to the United States on a troop ship. It was a much bigger and more powerful ship than my previous ship, so it only took about eight days for us to cross the Pacific. It moved along at a good rate, and it didn't have to zigzag like it would have during the War, which spared us a sixty-six day trip like the one we originally took west. I arrived in the States in May of 1946.

THACKRAY: Were you immediately discharged when you arrived back in the United States?

SHOOLERY: Yes. I spent about two weeks in a military camp and then I was discharged.

THACKRAY: What did you do after you were discharged?

SHOOLERY: I spent that summer at home readapting to society. I went back to Berkeley in the fall of 1946. Having worked with electronics during the War, I had realized that I really enjoyed working in that field, so I decided to take electrical engineering courses at Berkeley. I all ready knew how to design circuits and work with logic circuits, so I felt that I could handle the electrical engineering courses. Soon after I began taking two electrical engineering courses there, I dropped out because I all ready knew everything they were trying to teach me. Actually, MIT [Massachusetts Institute of Technology] had designed the Navy's radar courses, and they were thorough. At Berkeley, I would have had to take a couple more years of electrical engineering before I would learn anything new. Luckily, Berkeley gave me college credits for the Navy courses that I had taken. Eventually, I realized that only a small portion of an electrical engineers job actually involved innovation and creativity. Most of the time, he would be busy with the tedious task of working the bugs out of one of his instruments. Therefore, I decided that I didn't want to enter that field.

I knew that I wanted to do something that involved research. Having realized that I knew quite a bit about electrical engineering all ready, I decided to continue studying chemistry, and then someday combine that knowledge with my electrical engineering experience. I remember thinking, "I've got a head start on this whole thing. I've all ready done a lot of work in chemistry. I want to finish my chemistry degree and then work on applying electronics to chemistry."

THACKRAY: Did you structure the rest of your coursework at Berkeley with that decision in mind?

SHOOLERY: No. I had to finish the undergraduate coursework for chemistry, which meant completing courses in organic and inorganic chemistry, and analytical organic chemistry. The analytic course was interesting because I learned how primitive the whole field was during that time period. The field badly needed electronics instruments to effectively analyze chemicals.

THACKRAY: I guess you weren't talking about IR [infrared spectroscopy] much.

SHOOLERY: Well, IR was there, but it was still in its infancy.

THACKRAY: Was there an undergraduate course in IR spectroscopy at Berkeley while you were studying there?

SHOOLERY: No. IR spectrophotometers were only being used in graduate work. There might have been an undergraduate instrumentation course, but I never took it.

THACKRAY: When did you decide to go to graduate school?

SHOOLERY: As far back as I can remember my parents and I were always on the same page. We always knew that I would go to graduate school. I was salutatorian of my graduating class at Berkeley, so I probably could have attended any graduate school that I wanted. I only received one B as an undergraduate, and that was for an ROTC [Reserve Officer Training Corps] course. [laughter]

THACKRAY: When you returned to the Berkeley campus in 1946, had it seemed different than it was in 1941?

SHOOLERY: No. The atmosphere didn't seem that different to me, but maybe that was because the people who were involved in the sciences tended to keep to themselves. Regardless, my courses didn't change, so I didn't notice a difference even if there was one. The [Montgomery] GI Bill did change my financial situation, for which I was grateful. The GI Bill helped me get on my feet while I was finishing my undergraduate work. It also helped me get through Caltech. I didn't need to ask for any help from my parents, which was nice because, although they were doing a lot better since the Depression, they still didn't have enough money to put me through college.

THACKRAY: Did you continue to live at home during your college years?

SHOOLERY: I lived at home to finish at UC. I bought a car when I came back from the Navy, so it was easier for me to commute to UC.

THACKRAY: What about your life outside the course work?

SHOOLERY: My life was very limited outside my course work. I was never very social in high school because I was so much younger than the other students, so instead of trying to re-invent myself in college, I basically decided to focus on my course work and deal with my social life later. I did go out on dates, but I tended to be pretty selective. When I got in to Berkeley and joined the fraternity, I had more opportunities to socialize and date; though I never exploited those opportunities aggressively.

During that time period, I became very serious about a couple of women, but my relationships with them weren't very successful. I learned that if I wanted something to happen, I had to be aggressive and make it happen. I realized that I simply couldn't wait around for good things to happen because doing that had all ready cost me a couple of relationships. So I learned from my mistakes and became more aggressive. I eventually became overly aggressive, which resulted in shortsighted and impulsive behavior on my part. But that's life; it's a learning process. In fact, I've always told myself, "Don't make the mistake of thinking that life is a theory course; it's a lab course!" [laughter]

THACKRAY: It's a course that seems to last a lifetime.

SHOOLERY: Yes. One never graduates, either. [laughter] During my college years, I didn't have much of a social life. I did reconnect with my high school math teacher, who was an older single woman. She was probably in her forties, and I enjoyed her company and she enjoyed mine. We talked about all kinds of things, and she introduced me to ice-skating. She was an avid figure skater who skated in dancing sessions, so I learned to ice dance; which encouraged me to socialize because it required the use of a dance card.

After I became a graduate student at Caltech, I immediately joined the Caltech skating club. The skating club was probably the only social activity I was involved in during my

Caltech years. During that time period, graduate students were considered "monks." That was a well know stereotype, and many people referred to the institute as "Millikan's Monastery," after [Robert A.] Millikan, the institute's president. I had my little "cell" in the basement of one of the chemistry buildings, from which I could barely see any outside light. But once a week I would drive four miles down the road for my ice dancing session, where I danced with many beautiful women. It was important for me to get away from my work once in a while; it kept me from becoming weird.

BROCK: When you first got down to Caltech, did you have to take an entrance exam for the chemistry college?

SHOOLERY: There was a placement exam for the chemistry college, but I was accepted on the strength of my academic record. Before I started at Caltech in the fall, I had completed six months of graduate work at UC Berkeley. During most of those six months, I worked as a research assistant to Professor W. D. [William D.] Gwinn. He was one of the first microwave spectroscopists on the west coast.

[END OF TAPE, SIDE 3]

THACKRAY: How did you become involved with spectroscopy?

SHOOLERY: I became involved with spectroscopy by going to spectroscopy meetings around the country. For instance, I had gone to a meeting for molecular spectroscopy in Columbus, Ohio, where I met researchers associated with the field. Afterwards, I met professor Gwinn and worked for him as his research assistant. He put me to work building some of the electronic units that his research group used in their spectrometer.

Actually, I made a few mistakes in the construction of those electronic units because I was unfamiliar with many of the essential engineering rules. One of the most important rules that I had neglected to incorporate in to my design was the proper separation and insulation of the wires in the unit. [laughter] The unit that I built had a tendency to arc over and malfunction, and it caused professor Gwinn's team many problems after I left. While I was studying at Caltech, I occasionally drove to Berkeley to visit Dr. Gwinn. He always would kid me about how much trouble his team had with the unit I built.

I learned a lot about microwave spectrometers from working with Dr. Gwinn's team. His team taught me various approaches to making a good microwave spectrometer and many techniques for using the instrument. Working with Dr. Gwinn's team convinced me to pursue microwave spectroscopy when I arrived at Caltech. Therefore, at Caltech, I found Professor Don M. Yost to take me under his wing.

THACKRAY: How long did it take you to figure out that Dr. Yost should be your supervising professor?

SHOOLERY: I only had talked to about two or three professors at Caltech before meeting Dr. Yost. I remember he said, "I have a lot of surplus microwave gear because I'm fascinated by that technology, so I would be happy to take you on as a graduate student." Ironically, he knew absolutely nothing about microwave spectroscopy. He listened to my problems and more or less encouraged me to figure out how to solve them by myself. [laughter] He was a wonderful guy with a very droll personality. He realized that being serious hadn't got him anywhere in life, and so he was very relaxed about most things, including my work. Dr. Yost probably wouldn't have noticed my progress had I never gone to his office.

THACKRAY: Didn't that worry you?

SHOOLERY: No. I was used to working that way, and I enjoyed being responsible for steering my course. Whenever I sought guidance from Dr. Yost, he would listen to everything I said and help me in any way that he could. But he had provided me with a laboratory and some financial support for buying lab materials, so it was easy for me to work on my own.

THACKRAY: How many graduate students did he have?

SHOOLERY: He had only a few. John [S.] Waugh was one; he and I came in together. Dr. Yost had also taken on a postdoctoral fellow, Bob [Robert G.] Shulman, who eventually made a huge name for himself in NMR [nuclear magnetic resonance]. Shulman had worked on microwave spectroscopy with Charles H. Townes before Shulman's fellowship with Dr. Yost. Further, Shulman was very helpful because he knew a lot more about microwave spectroscopy than I; he even helped me build some equipment.

Shulman is considered the father of NMR for biological applications. He and I were very different people while we had worked together at Caltech. Our personalities were very different, but that difference encouraged us to push each other. For example, I was from the Navy school of building equipment, where we designed everything to run at right angles and tied every wire down beautifully. Shulman had a different technique for building an electronic chassis, which I called the "teepee" method. He would take a bunch of wires that had a common connection, twist them all together, and put some solder on the bundle. [laughter] To me, his construction was really sloppy, but in reality, all of his designs worked perfectly. We had no problems with any of them, and yet the stuff that I had built so carefully at Berkeley fell apart right and left. [laughter] Learning his sloppy methods was a humbling experience; but he and I had a very good relationship.

THACKRAY: Are you saying you were into building equipment and defining a research project almost from the first day you were down there?

SHOOLERY: Yes. My first project was to build a microwave spectrometer. That required me to assemble all the equipment I could buy; and what I couldn't buy, I had to build. Building the spectrometer proved to be very difficult, but I learned many things about microwave spectrometry in the process. For example, the microwave spectra had to be run at a very low pressure to reduce the collisions between the atoms and maintain the integrity of the freely rotating molecules. I had to work at 10^{-6} or lower millimeters of mercury; almost in a vacuum, because once a molecule had more than about six atoms in it resulting from collisions, its vapor pressure could only be measure in micrograms. Further, as soon as a molecule was no longer symmetric; as soon as it became an asymmetric rotor; its spectrum became so complicated that was nearly impossible to decipher. I learned all those things in the process of building my spectrometer. As a result, it took me a couple of years to get the spectrometer to accurately produce microwave spectra of molecules.

For my graduate thesis, I analyzed the spectrum of isocyanic acid [HNCO]. Cyanic acid is HOCN; but in HNCO the hydrogen atom is fixed to the nitrogen atom. During my analysis, I discovered that HNCO is nearly a linear molecule except for a hydrogen atom that is cocked off to a slight angle. HNCO is also a symmetric rotor, and its spectrum is not as complex as an asymmetric molecule. I was able to measure enough frequencies of various spectral lines that I calculated all the moments of inertia and determined the structure of the molecule. Though my thesis was technically on the analysis of isocyanic acid, the entire project was an exercise in building equipment, finding a problem, solving the problem, and defending the solution.

THACKRAY: Did that project last four years?

SHOOLERY: No. I actually completed my work in three years, but near the end of my research I contracted mononucleosis. I was sick for about a month, and had to stay in the sick bay at Caltech for a while. Afterward, I went home to recuperate for a couple of months because it had been clear I wasn't going to get my thesis done before it was due. As a result, I didn't graduate on time, even though I had completed all my work. I had passed my final orals also, so it was only a matter of submitting my thesis.

After I graduated, Don Yost took me on as a postdoctoral fellow. I left in the middle of the project, which is generally a terrible thing to do, but Don Yost was the kind of guy that I could do that with because he was very laid back. He was disappointed, but he also saw that I had a great opportunity, so he didn't oppose my leaving.

Before I informed Dr. Yost of my early departure, he and I had been working on an entirely new method for carbon-14 [C-14] dating wood artifacts. During that time period, scientists carbon dated wood artifacts through a very complicated and inaccurate process. First, they burned the carbon in the artifact to transform it in to carbon dioxide [CO_2]. Then they purified the resulting CO_2 gas. Next, they converted the purified CO_2 gas in to a stable compound containing the carbon from the artifact. Finally, they measured the radioactivity of the modified sample with a Geiger counter.

Dr. Yost's idea was to make carbon dating more accurate. The process, which on paper had seemed simple, actually turned out to be quite difficult. First, we took that carbon dioxide that we had burned out of the sample and put it in a steel tank under about ten atmospheres of pressure. The chamber had a window on one side, and an electrometer inside the middle of it. The electrometer consisted of an insulated post with a flap of gold leaf on it that would take a charge and stand out at an angle. After the radioactivity had ionized the gas in the chamber, the electrometer discharged a measurable amount of electricity, which we would examine by looking through the chamber window. As a result, we could establish an accurate carbon date by using the measurement from the electrometer in the chamber.

Dr. Yost's instrument had many flaws even though it was much more sensitive than a Geiger counter. One that was fixed early on had permitted the emission of alpha particles from the steel chamber's radioactive interior to ionize the carbon-gas sample and distort the measurement of the electrometer. To stop the alpha particles from reaching the sample, we installed a grid of wires around the inside of the steel chamber that absorbed the alpha particles before they could reach the sample. The wires were able to intercept the stray particles because the particles had very short ranges and were very susceptible to ionization with the grid. After we had installed the wire grid, the only radioactive particle decay that occurred inside the grid was caused by either the decay of carbon-14 or the contamination of the grid and the gold leaf by alpha particles.

The instrument was also very sensitive to cosmic radiation. Cosmic rays generated a lot of background and distorted the clarity of the spectra. To eliminate that cosmic interference, we moved the instrument underground. As a result, I drove, twice a week, 30 miles east of Pasadena to a dam located in a canyon. Then, I went 200 feet under the dam, in to a lead house that doubled as our lab. The lead house was built with lead bricks to prevent the natural gamma ray activity in the cement from penetrating the steel chamber.

To conduct our experiments, I would charge the instrument and put it in the little house, and then come back three or four days later to take a reading. From that, I could calculate the amount of radioactivity in that particular number of grams of C-14 and work back to the age of the sample.

Dr. Yost's instrument seemed sensitive enough to accurately measure fifty thousand year old carbon-based artifacts; whereas a Geiger counter could barely measure artifacts ten to fifteen thousand years old. Despite our successes, a team of scientists eventually figured out how to correct for gamma and background cosmic radiation by surrounding the sample and subtracting the coincidence counting. They used a whole bunch of counters and took coincidences, which allowed them to obtain comparable results using a much easier experiment. As a result, our method became inferior; but since I quit in the middle of the project, our design was never fully developed anyway.

THACKRAY: You must have really learned something about the practical problems of implementing good theoretical ideas.

SHOOLERY: Yes. Later, I discovered that there were many practical problems in incorporating the useful NMR techniques in to an instrument's design. Having experienced such practical problems with Dr. Yost, I remained undaunted when I experienced similar problems later with NMR instruments.

THACKRAY: Did you take many courses while you were at Caltech?

SHOOLERY: Yes. I took a statistical mechanics course from professor [John G.] Kirkwood; arguably one of the leading statistical mechanics in the country during that time period. He was a terrific guy, but he was not very good at explaining what he taught. He would get in front of the class and write statistical theorems on the board without explaining the material. To make matters worse, his exams were merely reproductions of what he had written on the board. I'm not a very good mathematician, so that course was difficult for me. I certainly knew I didn't want to do that for a living.

One of my roommates and best friends at Caltech was Bernie [J.] Alder. Bernie and I were exact opposites. He absolutely loved statistical mechanics and eventually made it the focus of his career. After Bernie graduated from Caltech, he spent many years working on the theory of liquids at UC Berkeley. After that, he worked for [Lawrence] Livermore Laboratories on fluid problems associated with the motions of particles in nuclear bombs.

I also took some physics courses. I tried to take one course from Dr. [Richard P.] Feynman. Rather than asking his class questions, Dr. Feynmann would assume that his class could follow what he was teaching. He was a very interesting man, and I absolutely enjoyed meeting him and seeing how he functioned, but his ideas were way beyond what I was capable of understanding. As a result, I didn't finish his course.

THACKRAY: Did you ever enroll in any of the courses taught by Linus [C.] Pauling?

SHOOLERY: No. However, I did sit in on some of his undergraduate lectures because I had heard that he was an excellent lecturer. He was a very stimulating speaker and I really enjoyed listening to his lectures.

THACKRAY: While you were studying at Caltech, did you interact much with other graduate students?

SHOOLERY: The graduate students at Caltech tended to band together. Caltech had a very monastic atmosphere, as I've said, and most of us found occasional research breaks essential. The strenuous academic environment helped forge bonds between first year graduate students, and it was never very difficult to find someone willing to socialize. As far as women were concerned, there weren't any at Caltech while I was a student, and the closest place to meet them was either at Occidental College or Claremont College. I had lacked the self-confidence to venture over to either of those schools, and it wasn't until my second or third year at Caltech that I actually approached women at the dances or the skating rink.

I did some dating eventually, and I almost lured one young woman away from her boyfriend. I was aware that she had a boyfriend at the time, but I really enjoyed her company so I started to come on to her pretty strong. As time passed, my advances seemed to distress her, and when I realized that I was hurting her, I backed off. I didn't want to break up a relationship that was important to her. But I was attracted to her, and despite my obviously flawed pursuit, I learned a lot from our brief relationship.

THACKRAY: Where were you living during that time period?

SHOOLERY: I lived in several places while studying at Caltech. During my first year, I rented a room in a private home. That was a very difficult situation, but I made it work. During my second year, a room became available in Caltech's graduate-student housing, so I moved there. My roommate was Allan [R.] Sandage, an astronomer who's future pursuits included working to determine the age of the universe. He is currently a Carnegie Institute astronomer, and is considered one of the foremost experts on cosmology. During my third year at Caltech, Bernie Alder and I shared an apartment on campus.

Allen Sandage had very low self-confidence while he was a student at Caltech. Once he became famous, and proved to himself that he was better cosmologist than most others, his confidence grew. But while he was a student, he was usually unable to sleep at night because he constantly feared that his work wasn't good enough. It's ironic how some people can be enormously talented and productive, yet still feel like they're on the verge of failure.

THACKRAY: Had you planned out what you were going to do after you graduated from Caltech? [laughter]

SHOOLERY: No. I was an "I'll cross that bridge when I come to it" person. Even close to my graduation I focused mainly on getting my degree. I had to pass my final orals to graduate from Caltech, and that kept me focused to the very end. I didn't know if I would be interviewed by someone who wanted to give me a hard time, so I had to be prepared for anything.

THACKRAY: Did you ever take any extended breaks during graduate school?

SHOOLERY: Yes. During the summer of 1950, I took off from Caltech to intern at Schenectady Laboratories [General Electric Company, GE]. I learned of an engineer who was using microwave spectroscopy, and I wanted to see how effective that technology was in an industrial laboratory.

[END OF TAPE, SIDE 4]

BROCK: Did you and your team have regular meetings with Yost?

SHOOLERY: No, all of my meetings with Dr. Yost were one on one. During my three years at Caltech, I only had about six meetings with him; about two meetings a year. He occasionally would walk into my lab and ask, "How are things going? What's happening here?" He generally left me to my work.

BROCK: Describe you relationship with John Waugh.

SHOOLERY: John Waugh first got me interested in NMR. While I was at Caltech, he was working with a very primitive magnet that wasn't homogenous. It was a very large magnet, powered by a whole block of submarine batteries that constantly needed recharging. While the magnet was running, it would drain the batteries, which changed the voltage and caused the magnetic field to drift. The instability of that entire system forced John to do all of his experiments with NMR very quickly. Further, it limited him to the examination of NMR in solids, where a small drift couldn't appreciably distort the data because the lines were very broad. I became familiar with the fundamentals of NMR experimentation simply by talking to John and watching him work.

One day, I went to an electronics show in Los Angeles because I needed new klystron tubes for the microwave spectrometer. I noticed that Varian's Los Angeles representative had setup an exhibit at the show displaying their new model of klystron tube. Their klystrons were wonderful, with great specifications, but they had cost a couple thousand dollars a-piece, so they were way outside of my budget. I was about to leave the show when I noticed a little stack of papers on the Varian display table. On the papers was a description of an NMR electromagnet with a twelve-inch diameter that had been developed at Stanford University and was being sold by Varian. With those papers was a reprint from the *Journal of Chemical Physics* that described an experiment by Martin [E.] Packard and an Indian chemist named [Shirnivas S.] Dharmatti. Packard and Dharmatti had placed a small sample in the Varian magnet and stabilized the energy current to scan slowly enough to see the three peaks for ethyl alcohol (3). When I saw those three peaks of ethyl alcohol from a condensed phase liquid, I thought, "This is the kind of thing that could actually be an analytical instrument. It just needs to be improved."

I was also intrigued by Varian. The Packard and Dharmatti article had been printed only two months before the show, and Varian was all ready trying to make a profit on the magnet. I thought, "I want to be associated with such an aggressive company." By that time period, I had already decided I didn't want an academic job. I had seen many things in academia that I didn't think were admirable or fun: how hard professors had to work to raise money, how they became overwhelmed by their own importance, how their scholarly seminars were usually only appreciated by a few people, and so forth. I thought I would be a lot happier in a job with a set of goals and the resources to accomplish them. I wanted a job where I could be given an adequate budget simply by justifying my projects. In fact, if I hadn't gone to Varian, I probably would have gone to GE, because I had a good time there during my internship in the summer of 1950.

THACKRAY: What did you do after you saw the Varian display at the Los Angeles show?

SHOOLERY: I decided to write a letter to Varian (addendum I). In the letter, I asked the Varian executives to hire me to develop applications for their new instrument. I emphasized the importance of showing their customers the relevance of the new Varian instrument. Further, I asserted that I would be able show Varian's customers how they could solve their particular problems with the Varian instrument.

THACKRAY: So that led directly to the job offer and employment?

SHOOLERY: Yes. I was sent a letter inviting me to an interview. At the interview, I told my interviewers that I was currently doing a postdoctoral fellowship with Dr. Yost, but I could take off if it was necessary.

THACKRAY: Would you describe the interview in more detail? Who did you meet with and what were the results?

SHOOLERY: I had written the letter to [H.] Myrl Stearns, the general manager of the company. He passed the letter on to Ralph [W.] Kane, who had been running the special projects division at Varian. The special projects division eventually became the instrument division, but when I first arrived at Varian, they didn't yet have an instrument. I talked to Ralph Kane, and explained why I wanted to work for Varian, what I wanted to do at Varian, and why I thought it was important. I received a job offer later in the mail. As a result, I became the five hundred and twelfth employee hired at Varian.

BROCK: How large was the special products division?

SHOOLERY: When I first started at Varian, there were about fifty people employed in that division.

BROCK: What did the other four hundred and fifty people do?

SHOOLERY: They were making vacuum tubes. Of course, there were some corporate executives and administrative employees as well. There was a research department and an engineering group; each supported by technicians and machinists. There was also a production facility that housed sales people and assembly-line employees.

BROCK: Besides the magnet component, what other products was Varian selling when you were employed?

SHOOLERY: One product they were selling was a flux meter. It used NMR to measure the strength of a magnetic field. The instrument consisted of a radio-frequency unit connected by a cable to a probe. The probe was a small tube, several inches long, with some tuned circuits in it, surrounded by a coil of wire. A sample of distilled water was inside the tube. To operate the flux meter, one energized the coil surrounding the probe to establish the resonant frequency of the protons in the distilled-water sample. Then, placing the probe next to a magnetic object, such as a magnet, one de-energized the probe, causing the protons of the distilled water to precess in their attempts to realign their axes with the external magnetic field. Those precessing protons induced a small current in to the coil, which was proportional to the strength of the external magnetic field. Of course, if the magnet being examined had an inhomogeneous magnetic field, the instrument's results would be so blurred that the operator would be unable to read them.

Most people don't know that Varian Associates was actually started to exploit the potential of NMR. It was only out of economic necessity that Varian Associates eventually sought contracts from the government and returned to building vacuum tubes. Those government contracts were available only because the Russians had launched the first Sputnik satellite and the Americans needed vacuum tubes to compete in the space race. Hence, it was merely a coincidence that Varian was able to stay afloat.

Another interesting story is how Varian came to control the rights to NMR technology. Originally, Felix Bloch and William W. Hansen patented NMR, but they didn't actually patent it by themselves. After a new idea has been published, the creator of that idea has exactly one year to patent it before it becomes public domain. Therefore, in 1947, nearly one year after the discovery on NMR, Russell Varian showed up at Felix Bloch's office with a patent for Bloch's idea [NMR] and a license for Varian associates. Varian, whom had put his name on the patent, also had Bloch and Hansen sign both of the documents, which gave Varian Associates exclusive right to the development of an NMR instrument. Those documents virtually prevented any competitors from marketing any product that was based on the principle of nuclear magnetic resonance for seventeen years.

Other companies were sold licenses to NMR technologies, and Varian started to draw royalties on improvement patents as well. Eventually, Varian grew to be worth millions of dollars, and Felix Bloch became rich from the royalties he was receiving. As Felix Bloch's wealth increased, it became very difficult for Varian executives to negotiate with him. Tension came to a head when Varian wanted to increase funding for NMR research and development while maintaining a decent level of profit. They found that difficult to do because they had to give 5 percent of their profit to Bloch as royalties. On the other hand, Bloch was frustrated because he had to split his royalties with both Russell Varian and Hansen.

Eventually, Hansen died and his half of the patent, which was part of his estate, was willed to his brother. Hansen's brother was the graphic artist who came up with the Smokey the Bear campaign in California. He knew nothing about NMR, and yet received royalties equal to the size of Bloch's royalties, which I'm sure drove Bloch nuts! [laughter] But Felix received a lot of money from his discovery of NMR, and I'm sure that he was satisfied. He became a consultant for Varian, and the relations were generally amicable; although, when it came time to discuss the royalties, the situation always got quite tense.

BROCK: Did Varian and Bloch exchange a lot of ideas?

SHOOLERY: Yes. There was a very good exchange of ideas. For several years, we went to seminars that Felix Bloch had held at Stanford. In addition, all the students that worked under Felix Bloch came to Varian after Varian had acquired the rights to NMR. Most of Bloch's students were more interested in the instrument than in the application anyway, because by that time period, there were no applications. There really wasn't any other place to get a job in NMR during that time period.

Whenever we went to one of Felix's seminars, I kept my mouth shut because I knew that Bloch was a top-of-the-line physicist who didn't take ignorance lightly. Luckily, whenever I talked about the applications we were doing, he and his group listened politely and carefully. Whenever Felix came over to Varian and consulted with us, he was always very respectful and very careful not to talk too much about chemistry; he didn't want to get caught up in being wrong either! [laughter]

BROCK: When you first started at Varian, did Ralph Kane have a job description in mind for you, or did he create a new position for you based off the description in your letter?

SHOOLERY: I essentially wrote my own job description. When I first arrived, I had to setup my laboratory, which also meant building an NMR instrument. The guy who ran the special projects division had all ready sold an NMR instrument to Humble Oil Company before one had been built. It was a good thing I had experience building instruments because he had all ready set a deadline for the delivery of that instrument.

BROCK: Describe how you built the NMR instrument.

SHOOLERY: The NMR instrument had to be designed to fit in a cabinet. The first thing I built was the radio frequency unit, which ran at 30 MHz. I chose the 30 MHz frequency because it worked at about 7,000 gauss; which, with a reasonable amount of current, was about the highest field I could get out of the magnet. Thirty MHz amplifiers were widely available because they had been used in radar equipment during World War II.

An engineer had all ready designed the receiver and transmitter for the NMR instrument, and my job was to build those using his designs. For some reason, the engineer had designed the receiver and transmitter on a single chassis. When I received his drawings, I didn't recognize his design flaw until after I had built the instrument. As a result, we had a lot of trouble filtering out the direct pick-up from the transmitter.

After we had acceptably reduced the transmitter-interference problems, we began testing the completed unit. The specifications of the system were very loose, and it did not take us very long to meet them. All I wanted the instrument to do was produce a readout that showed the three separate peaks of ethyl alcohol. Once the instrument passed that test, we put it on the market, where it remained our primary NMR instrument for about five year.

BROCK: The only specification was an ethyl alcohol test?

SHOOLERY: Yes. All we had to do was to put a sample of ethyl alcohol into the magnet, find a good spot, and show three separate peaks; Humble Oil took the research from there.

BROCK: Did you complete the order on time?

SHOOLERY: Yes. I had to go to Texas to test and install the instrument, because I was the most experienced operator. None of our engineers or assemblers knew anything about the instrument. We hired and trained a service engineer later who could fix the instrument without my help.

Four or five months after I had arrived at Varian, we built a second instrument and put it in my applications laboratory. Subsequently, I was able to begin researching the possible applications of NMR instrumentation to problems of chemical interest. The lab instrument was also useful for dealing with customers interested in using the NMR technology for their businesses. By researching customers' specific problems, I could quickly and confidently tell them whether our instrument could benefit their businesses.

BROCK: Did you research your customers' problems for free?

SHOOLERY: Absolutely, because we were benefiting as much as they were. We were building a body of knowledge. In fact, fairly soon we started a technical information bulletin. We put examples of spectra into the technical information bulletin along with a basic description of NMR technology. Then, we sent that bulletin to people who had shown an interest in our instrument at trade shows and exhibitions. After about a year, we began running a monthly series of advertisements in the *Journal of the American Chemical Society*.

When we first inquired about placing an advertisement in that magazine, we were told that the only ad space left in the magazine was on the back cover. Though everyone had wanted to have an ad on the inside of the magazine, we thought that the back cover was the ideal location for our ad. We figured that there was a 50 percent chance that every time someone laid the magazine down, the back cover would be facing up. That meant people would see our ad without even having to buy the magazine.

We purchased and kept the advertising space on the back cover for five years. We made sixty ads: one a month. I remember the name of the ad series was, "This is NMR at Work." The ad pictured a sample spectrum with an interpretation of the results. After a while, we began emphasizing that the interpreted spectrum actually solved a problem, like determining a chemical structure.

BROCK: Who was the end user of the NMR instrument at Humble Oil?

SHOOLERY: Rolly Williams was the end user at Humble Oil. He had been trying to apply analytical techniques to the oil industries' problems when he realized that NMR might be useful for analyzing petroleum mixtures. Williams' assumption was correct, and his NMR instrument became quite useful. The use of NMR instrumentation has resulted in the discovery of numerous polymer structures that are very important in the oil business. In fact, almost every contemporary oil company relies heavily on NMR, along with the more traditional mass spectrometric methods.

[END OF TAPE, SIDE 5]

BROCK: It must have been an expensive device.

SHOOLERY: In the early 1950's, the first NMR spectrometer had cost about thirty thousand dollars. That's nearly equivalent to the current price when adjusted for inflation, but a modern instrument is probably a hundred thousand times more sensitive than an original. Its field strength is at least ten, or possibly even twenty, times higher than an original instrument, and it has a greatly improved dispersion. Further, the line widths have narrowed down by a factor of ten. When I think about the imprecision of the original instrument, I wonder how I ever solved any problems. [laughter]

BROCK: Did NMR instrument sales increase after you built the second instrument for lab application?

SHOOLERY: Yes. After the construction of the second instrument, we sold instruments to Shell Development [Company] in Emeryville [California]. Shell used the instruments the same way that Humble Oil had been using its own. Eventually, Exxon [Exxon-Mobil Corporation] acquired Humble Oil, and both Shell and Exxon were doing NMR research. We also built and sold low-resolution spectrometers because Varian was willing to make anything that might expand its market reach. Those instruments proved useful for distinguishing oil and water in core borings for oil wells. Later, that market also proved very profitable for Varian sales of NMR instrumentation.

BROCK: Discuss your relationship with the Varian brothers in the early days of your employment.

SHOOLERY: The Varian brothers were very unique people. Russell Varian was a visionary. In pictures from those days, he looks like a visionary. He gave the impression that he lived in a different world. But he had a very keen, intuitive, and creative mind that could see all kinds of possibilities; he invented the klystron tube. Sigurd Varian was practical. He ran the machine shops and made sure that all the mechanical work was done right. He was a very down-to-earth person. He had had a career in aviation, flying Ford Tri-Motors to South America, but he ultimately joined his brother to found Varian. They both put money in to it, along with six or eight other people. The whole story is told in a book by Russell's wife, Dorothy Varian.

Those people started the company, and Russell made it clear that their real aim was to make NMR instruments. After a couple of months of operation, they were running out of money and they still had nothing to sell. There was no income coming in, and it would have been a very short-lived company, were it not for the launch of Sputnik. The brothers realized that klystron tubes were about to become a huge commodity because the government would require them for high-frequency transmissions. Therefore, the Varian brothers went to Washington and won some tube development contracts. Those contracts paid for the companies first five, or maybe even ten, years of existence.

Varian's overwhelming reliance on klystron tubes led many of the company's employees to believe that the special projects division was a waste of money. Despite that, Russell remained adamant that the company's main objective was to make scientific instruments. He felt that the success of the klystron tube division was only the foundation from which a successful instrument division would be built. To reinforce that belief, Varian soon changed the name of the special projects division to the instrument division. Though both of those divisions had been parts of the same company, they constantly competed with each other for research funds and argued about which division was actually bringing in the money. Many years later, Varian actually divested itself of the tube division because it lost the ability to make significant profits.

Varian's executives eventually became disenchanted with the company's multi-divisional organization. The individual divisions had become very self-sufficient, and they were not synergistic at all. They didn't share technology, or even the same markets. The only things that all the divisions used were the same circuit boards. Therefore, instead of three divisions in Varian Associates under a corporate umbrella, executives split Varian Associates into three individual companies. Varian, Inc. became the instrument company, Varian Medical Systems became the medical instrumentation company, and Varian Semiconductor [Equipment Associates] became an ion implantation company.

Over a number of years, the medical division transformed in to a company that sold linear accelerators for cancer treatment. The original Varian Associates Corporation had gone into vacuum work, which then led to the production of silicon chips for microprocessors' integrated circuits. They built the machines that make and implant chips with ions. Those chips were then turned in to microprocessors. Hence, there were three separate businesses with almost no overlap, and it didn't make sense to have the extra cost and unwieldiness of a big corporation linking all of those divisions together. Therefore, in 2000, the corporation was dissolved, and the three divisions became three independent corporations.

BROCK: Who headed up the instrument division when it was first formed?

SHOOLERY: It was headed by an engineer named Ralph Kane. For several years, I reported directly to Martin Packard, who was the research director. I was considered an employee of the research department, where I ran the application laboratory. After a few years, Emery Rogers, who had designed the power supply for the twelve-inch electromagnet, was appointed sales manager. Emery envisioned expanding the sales force and doing all the things that one does to market products; by that time period we really did have a line of products. He wanted to transfer the application laboratory to the marketing department. Martin accepted that change because there had been plenty to do in research. After that switch, I began reporting to marketing.

Emery and I had an instant affinity for each other. We always understood each other perfectly; we worked well together, and traveled together a lot to meet with potential customers. On the way back from one of our trips, we stopped in New Mexico because one of our sales reps informed us that he had assembled an audience to listen to our sales pitch. When we arrived to give the lecture, we discovered that most of his contracts were with IEEE [Institute of Electrical and Electronics Engineers]. Only two people came to the lecture hosted by two lecturers. [laughter] We gave them our talk anyway, and they listened and they enjoyed it, but they were not really potential customers.

For a while, Emery and I traveled around the country giving lectures on NMR. Sometimes we had very good audiences, and as time passed, we were invited to give lectures without having to request an audience. We also became known through the trade shows we were attending, and our sales skyrocketed. Emery Rogers was the main force behind the ever increasing demand for our products. He ran a very aggressive marketing operation. He worked very closely with me because I provided all of the scientific information that supported our claims and emboldened our literature. Emery and I discovered that the best way to market our instrument was to show our customers how our instrument could solve problems that they encountered in their particular lines of work.

BROCK: When did you switch from working in the research department to working in the marketing department?

SHOOLERY: I switched around 1956. We started developing instruments in 1953, and I switched three years later. Actually, the department that I switched to wasn't named "the marketing department" until four or five years after I had been in it.

BROCK: Did you find it odd that Varian had a research lab located in its marketing department?

SHOOLERY: Yes. Most instrument companies were using techniques and designs that were decades old. Many companies were not taking advantage of technological advances that had been made during the War. Typically, instruments had maintained their unwieldy shapes and complex designs even though the technology existed to eliminate both of those deficiencies.

Similarly, instrument companies didn't have laboratories that tackled new problems for customers. The concept of an applications laboratory that did innovative research to explore a products potential was relatively new. As a result, Varian's market dramatically expanded when they began publishing the results of their research so that the customers could see the potential of the NMR instrument. I think Varian was the first company that did research on their own instrument to expand their consumer base.

BROCK: Do you know why Russell Varian was committed to developing NMR as a tool?

SHOOLERY: He realized it was an excellent tool the first time he saw it used. That was even before he saw the three peaks from the ethyl alcohol spectrum: when he saw that each nucleus had a magnetic moment, he thought that one could use that characteristic to analyze things. I don't think he had a particular development plan, but after I came to Varian and we'd set up the applications lab, he often came around and looked over my shoulder; or I would see him in the cafeteria at lunch time and we'd talk about what we were doing. He kept abreast of what was going on. He never came in to my lab and told me what I should be doing; rather, he seemed to think that everything was going the way it was supposed to.

BROCK: How long was Russell Varian the president of the company?

SHOOLERY: In 1960, after he had been head of Varian for twelve years, Russell had a heart attack on an Alaskan vacation. He was active right until he had his heart attack, but he wasn't always in good health. It was clear that he was in a precarious state in the last few years, but he was very much involved in all of the technical affairs and he had a sense of the business.

Russell was a typical entrepreneur. He was motivated by profit, and 10 percent of that profit always went to research. He believed that research investments helped to keep his product better than his competitor's, which then allowed him to charge more for his product. Varian has always gone after market share and has been aggressive at developing new products. That had been the philosophy of Russell Varian, and many who had worked with him also believed in his philosophy, so it lived on after his death.

BROCK: Russell Varian believed in the profitability of NMR while he had been president of Varian. After his death in 1960, did the company change directions, or was the applications research of NMR still considered the primary vehicle for profitability?

SHOOLERY: I think that company executives still believed in the profitability of NMR applications after Russell's death. They continued to allocate money for research and development with the expectation that it would turn a profit. I'm not sure when the division first became profitable, but it was probably in 1957 or 1958. It might have been a relatively low profit business until we came out with the Model A60 in 1961. The A60 was a huge success, and after its release our division continued making profits well in to the future.

BROCK: How did the applications laboratory grow and change up until the introduction of the A60?

SHOOLERY: The lab began with just me, and I worked alone for about a year. Then, Virginia Royden was hired to work in the applications lab. She was the wife of Stanford mathematician, Hal Royden. She had been trained as a chemist, and could interpret the spectra. In the lab, I provided most of the planning and set the direction of what we would do. She helped me with the sample preparation and the operation of the A60. The instrument initially required two people to operate, and I've got a great picture of Virginia and me operating it. One person would adjust the magnet, and other one would watch all the other dials, and so forth. It was a nightmare to run the early models of the instrument, and Virginia was very helpful in making it possible to run everything. In addition to working in the lab, Virginia also accompanied me to the trade shows, where she participated in discussions with potential customers.

Eventually she retired, for reasons unknown to me, and I was forced to find someone else to work with me; one guy really couldn't do the whole job. I had to travel quite often, and I needed somebody to run the lab while I was away. Further, I was having more and more success at solving problems, so there were lots more samples coming in, and I needed to keep up with the extra work.

I got in touch with a graduate student at the University of Washington who was working towards his Ph.D in chemistry. He was in a financial hole and he really couldn't afford to keep going to school. Of all the people that I interviewed, he seemed to me to be the most qualified person. His name was LeRoy [F.] Johnson. Ultimately, Roy Johnson turned out to be quite a strong figure in the NMR instrument business. He worked for me until I left the company briefly in 1969, for a period of a few years. During that time period, Roy Johnson took over the application laboratory and ran it quite successfully for about three or four years. Then, he left Varian with several other people to start a small NMR specialty company. He had a long career in NMR and became well known and respected in the NMR field.

People tend to get stuck in a niche: even though they want to explore other areas of their fields, they're forced to work on what the company deems profitable. Incidentally, Varian had a number of spin-offs from its operations, some of which turned out to be quite successful. One example is Spectra-Physics [Inc.], which was the first commercial laser company. That company made huge fortunes for the people who first established it.

Roy Johnson left with a few other people and formed a company called SpectraSpin. It was acquired eventually by Nicolet [Thermo Nicolet, division of Thermo Electron Corporation] in Wisconsin, and they began producing NMR spectrometers. That operation got into trouble, and was absorbed by General Electric. As a result, Roy Johnson put in the last part of his career with General Electric as head of their applications laboratory. He did an excellent job there, and was highly regarded in the NMR field. At meetings he gave excellent talks and was a great speaker. He was very good at conceiving and describing experiments, as well as developing new NMR experiments.

He had a good career, but he started with me around 1956 or 1957, and was there until 1972. He spent a long time there and became my right-hand man. We actually published some articles together. He was a good contributor to our internal bulletins, and wrote articles about various accessories and applications.

While Roy was working at Varian, we hired a few other chemists to work under him in the application lab, though never more than three or four. Later, the application laboratory branched in to a wider range of instrumentation. Varian began to put less emphasis on liquid samples and more on solid state samples, which weren't high resolution; but it still made sense to use them when working with group applications.

Anywhere from six to ten people are working in the current high resolution application laboratory, which has changed significantly over the years. For instance, the applications lab has imaging laboratories, high resolution NMR for studying solids, very high field high resolution instruments, state-of-the-art biochemical instruments, and average instruments used by organic chemists. The applications lab itself has never been a very big operation, though it has grown with the market.

[END OF TAPE, SIDE 6]

SHOOLERY: An infrared spectrometer was a lot cheaper and more convenient than our NMR instrument, and the IR spectroscope could fit on a table instead of taking up a huge room. When we first developed our NMR instrument, we knew that it had to analyze samples that couldn't be analyzed by infrared spectroscopy to make up for the NMR instrument's shortcomings. It took us two or three years to find such a sample, which was introduced to us by a young chemist named E. J. [Elias J.] Corey.

E. J. Corey had determined that the sample molecule was either a heptagonal ring of carbon atoms or a hexagonal ring with a bond across it; making the molecular shape like a triangle ring bonded to a hexagonal ring. The vibrational spectra of the two shapes were very similar, and Corey was unable to tell the difference between the two shapes with IR spectroscopy. The NMR, however, showed very significant spectral differences. The NMR results clearly supported the heptagonal ring hypothesis. That conclusion was then published by Corey, in collaboration with the members of the Varian applications lab.

That accomplishment convinced us that our instrument could analyze chemistries that couldn't be analyzed using infrared spectroscopy. Sometimes, we could solve them more elegantly and easily. Further, we could work with a sample in a liquid solution that was easy to prepare; as opposed to the preparation of samples for infrared, which could be kind of tricky. We realized that our NMR instrument had some real advantages over IR spectroscopy and after our consumer base gained some interpretational experience, they would also realize its advantages.

Before our instrument could become a big seller, we knew that we had to redesign it. First, we needed to make the instrument more stable so that it didn't require two operators to run a spectrum. Second, we wanted the instrument to produce the same spectrum every time it ran the same sample; it had to be consistent. It took us about five years to develop the proper stabilization techniques that enabled the instrument to produce accurate spectra consistently. Once we had succeeded, we knew we could produce an instrument that organic chemists would accept.

However, if our instrument was going to compete with the Perkin-Elmer [Inc.] Model 13 [infrared spectrophotometer], it had to be made smaller and cheaper, without sacrificing the stability of the instrument. More importantly, we wanted the instrument to be pre-calibrated so that the chemist didn't need to significantly modify the settings of the instrument to get it to work. During that time period, an operator had to go through a calibration procedure to determine where everything was each time he ran a spectrum; hence, the resulting spectra never looked the same. What we needed was an absolute reference in the NMR instrument instead of a spectral peak that drifted along.

We created the absolute reference on the instrument through the addition of the Nuclear Side Band Oscillator. It consisted of a sample of distilled water placed in the side of the probe, close to the coil. The distilled water would be excited by the radio frequency in the coil, which was then closed through an electronic-loop pathway, causing regenerative feedback around the loop. It would only oscillate at the frequency that the nuclei would precess, permitting an operator to use the sample of water to determine the oscillation frequency of the system. If the magnetic field ever changed, the frequency would shift with it, which created an absolute reference.

The first test of the instrument was quite successful. I put the sample in it and we got a spectrum. Then I thought, "The real test of this is to run another spectrum." So I told the engineers, "Slide the pen back to the beginning, put it down and start the scan again. If the instrument works properly, the pen should come out in virtually the same position." I started the instrument again, and there was just one line on the paper when it was finished. I said, "Didn't the darn thing work?" The engineer replied, "It ran. I think it worked." Again, I reset the pen and ran the instrument. There was still one line. I ran five more traces and they all came out exactly the same. At that point, we knew that we had an instrument that was going to appeal to organic chemists. We were right. One hundred and twenty-five instruments sold in the first year.

BROCK: Which instrument was that?

SHOOLERY: That was the A60. It was announced in 1961, which was only eight years after the first primitive instrument in 1953. It wouldn't have been possible to develop the A60 without the step-by-step development of all of the elements that contributed to it. We learned how to make magnets that were more efficient. We knew also how to form the pole surfaces in such a way that they didn't distort the field, but took maximum advantage of the geometry. Then, we learned how to correct little remaining imperfections in the field using electric current in the coils, which could be controlled by knobs. The instrument could be fairly easily adjusted by someone who wasn't an expert in instrumentation, and it could be run easily because the scan fit a known scale. In addition, it was locked internally so that the reference couldn't drift. That instrument out-performed the larger research instruments that had been developed only a few years earlier.

Before the development of the A60, chemists had known that the effectiveness of NMR was outweighed by the size, cost and complexity of the instrument. Most chemists were leery about purchasing an NMR instrument unless they were part of a major laboratory. Actually, the price of the A60, when it came out in 1961, was around twenty-three thousand dollars. At that price, the A60 was quite attractive.

In 1961, I was in the process of moving from applications into marketing. Emery wanted to get out of the marketing business. I was asked to take the marketing manager job because Emery had decided to join Hewlett-Packard. I had some misgivings about getting out of the laboratory because I really liked what I was doing there, but I always sought new challenges. So I thought, "I'll give it a try."

During that time period, Emery and I had to set the price on the A60. To estimate the price, I assumed that if an instrument was considered useful enough to become a necessity in a consumer's laboratory, then the price would determine the number of people who could buy them. Therefore, I compared the number of analytical instruments, deemed necessary for a lab, sold in any given year with the price of each particular instrument. When I analyzed the data, I discovered that the price of any "necessity" instrument varied inversely with the amount of sales of that instrument. It was a parabolic curve. By comparing sample prices of the A60 instrument against that parabolic curve, I estimated that Varian would sell about one hundred and twenty-five instruments if each instrument cost twenty-five thousand dollars. That turned out to be the exact number of instruments that we sold in our first year. Altogether, we sold about fourteen hundred A60's during its five-year lifetime.

The A60 was a vacuum tube instrument. After about five years, vacuum tubes were getting hard to find, and transistors were the way to go. The A60 was superseded by a transistorized instrument, the T60, which used a permanent magnet instead of an electromagnet. Designs of magnets had improved to the point that an engineer could make a 60 MHz instrument that required 14,100 gauss using a permanent magnet. The permanent magnet eliminated the need for the complicated stabilization circuit that we had used in the A60. The instrument was stable enough that an operator could put a little trace of something in his sample and use it as a reference. We used a little drop of tetramethylsilane—that's silicon with four methyl groups on it—that has a unique peak at one end of the spectra.

We were able to sell the T60 for less than the A60 because the T60 had a less complicated design than the A60. After about five years, the price of the A60 had risen to about fifty thousand dollars; whereas, the price of the T60 was below thirty thousand dollars. The T60 was simply a better instrument. It didn't require cooling water, which was messy, and it didn't require a stabilized electric supply. The T60 became the real workhorse of organic chemists for a number of years; some of them are still in use.

BROCK: Were the permanent magnets developed in-house or elsewhere?

SHOOLERY: They were developed in-house. All of Varian's magnets were developed in-house up through the 300 MHz superconducting magnets. When we got to that level, it became very expensive to develop magnets from newer materials that allowed higher fields. About that time period, Oxford Instruments [PLC] in England had been working on superconducting magnets for numerous purposes. PLC became very good at developing magnets, and designed some excellent magnets for the spectrometers.

Around that time period, Varian executives had adopted the "make-or-buy" decision process, and it came down to, "If you can buy it, don't try to make it, assuming you have a reliable supplier." Oxford made a quality product, and Varian signed them up for such a large amount of business that they were virtually a subsidiary. But Varian is vulnerable in a sense, because our major competitor, Bruker Instruments [Company], in Germany, makes their own magnets. Suppose there was a big fire at Oxford? There are other magnets, but trying to adapt them into our complex system would not be easy. Varian would be in very bad shape if there were a major disaster at the Oxford factory. I suppose people have been there and looked at it. I think the odds of a fire are pretty small, but it still worries me. We're at their mercy. In a way, they're at our mercy too, because we're one of their major customers. [laughter]

BROCK: Did you find that the organic chemists who had used Varian's instrument became early champions of NMR?

SHOOLERY: Yes. Chemists who had used our NMR instrument were always enthusiastic about it afterwards. Our instrument was new, different, and easier to use than traditional methods. The data that a chemist receives from an NMR instrument are usually very specific and straightforward, once an operator understands the rules that govern them. Early users of the instrument were relieved that the results of spectral analysis could be explained very easily. Further, sample preparation was always very straightforward. An operator had to simply find the solvent that the sample would dissolve in without interfering with the experiment. We did many experiments in the applications lab trying to find the proper sample-solvent relationships. Once we solved a tough problem, a state-of-the-art problem, the solution to the problem was sold; much to the delight of our customers.

BROCK: Do you remember any of those early champions in particular?

SHOOLERY: Certainly Carl Djerassi at Stanford became very positive about NMR; although, he was more likely to sit in his office and let his graduate students do most of the work. But he would tell them what he wanted and he soon appreciated that NMR was the way to solve problems. Sometimes, I would contemplate the results of NMR experiments with Djerassi in his office, and we published a fair number of joint papers (4). Joint publication was always valuable because then we could take that reference, spread it around, and give away the reprints as advertising for our instrument. We had some big names on those papers too; like Djerassi.

We also used the Corey problem that we solved in the early days, but then we didn't work much more with him; probably because he got his own spectrometer. While he was very pro-NMR, he didn't particularly go out of his way to work with Varian or to promote Varian, but he did promote NMR. He talked about it and solved a lot of difficult problems with it. Ultimately, he got kind of angry at Varian for one reason or another and became slightly hostile. He ended up buying quite a lot of competitive equipment, which worked all right for him, but I don't think it worked any better than ours. We never did fully understand why he became so angry with us.

Some chemists get very attached to one company or another, and then they become fanatical about it. Of course, we love it when they're fanatical over Varian, but we think they're really stupid if they're fanatical over somebody else.

BROCK: I believe that it would be fair to say that you were instrumental in the push for the A60 project. Is that fair? Can we talk about your role in that project?

SHOOLERY: That's definitely true. I played a fairly major role in the development of a set of specifications, with Emery Rogers and others, that defined what the instrument should do.

Then, I worked very closely with the engineers to make sure that they focused on achieving those specifications. I was pretty hard on them, and they dreaded when I would come to the factory and urge them forward. Before the project began, we made the employees agree that we could and would accomplish all the goals set forth in my specifications. If the work got too hard, the engineers always wanted to reduce the specifications slightly, but Rogers and I wouldn't let them.

BROCK: As one of the main forces driving the development of the A60, did you consider your position as encompassing both employee and consumer-related roles?

SHOOLERY: Yes. I was very consumer-like in my evaluation of the instrument's progress and final design. I assumed a customer wouldn't like a particular aspect of the instrument if I didn't like it. That assumption worked very well. A term we coined to describe the other role that I played within the company was "product champion." I would make sure that the A60 got enough resources to achieve what needed to be done; so it wouldn't fail because there weren't enough engineers on the project or not enough money was allocated to it. If somebody thought that the instrument couldn't make a particular specification, I would defend that product against him or her quitting and backing down too easily. We used the term "product champion" for the person who felt responsible for the product's future success on the market.

BROCK: Did the role of "product champion" start with the A60 project?

SHOOLERY: Yes. The A60 was the first instrument we built that ran parallel to a well-known and narrowly-defined market. As a result, we were able to determine, with decent accuracy, all of the characteristics that would make the A60 successful in the market.

BROCK: Did you continue that practice after the development of the A60?

SHOOLERY: Yes. I would then move to other projects as we went along because NMR didn't stop with the A60. If anything, the rate of development increased tremendously as more capable, powerful instruments were designed. I didn't try to solve problems for everybody, because I didn't know just what they needed or wanted. I didn't understand other scientific fields in the same way that I understood organic chemistry problems, so I stuck with the instruments that we were trying to sell to the organic chemists.

BROCK: In 1962 you become the marketing manager for the entire instrument division. Is that correct?

SHOOLERY: No. I became manager for the NMR division of the instrument group. There was a GC [gas chromatograph] instrument. When I became the marketing manager, we sold only NMR products and magnets. We made the magnets in our factory and sold them to anyone who was interested; usually physicists. We were earning about twelve million dollars a year in sales initially, and then twenty-two or twenty-three million dollars a year, about five years later. It was then that I decided to go back to the laboratory, though I continued to spend a lot of my time acquiring other companies.

I spent a lot of my time traveling; looking for other companies to purchase. I never considered myself a good person for such a task because I had no experience in that field. Regardless, when I finally left marketing, we had established an applications laboratory in the Pittsburgh [Pennsylvania] Airport, one in New Jersey, and one in Zurich, Switzerland. We had expanded from a two-salesman organization, out of Palo Alto [California], to a worldwide sales organization. In that sense, I made a fairly major contribution to the marketing effort by broadening it into a worldwide lab that had the ability to make demonstrations and connections to foreign leaders in the field.

The labs in Pittsburg, New Jersey, and Zurich, were field labs. The employees there tended to work with real customers that really were planning to buy something rather than somebody who wanted to write a joint paper, and maybe raise money to buy an instrument afterward. However, some of the field labs in Europe did a lot of work with scholars interested in writing joint papers.

Though Varian had expanded on to the worldwide market, selling became grittier, and our salesmen had to chase orders. They had to stay on top of the customer and make sure that the competition wasn't polluting his thinking or offering him better deals. It became a much more competitive business because, by the mid-1960s, other companies had started to make NMR instruments. Varian's patent had been issued about 1946 or 1947 and expired in 1963, when Japanese instrument makers entered the market. We had built the A60, and the next year they built the C60. It wasn't as good a machine, but it worked and it was cheaper.

BROCK: Who made that?

SHOOLERY: JEOL [Ltd.]. I think that was our major competition during that time period, though Perkin-Elmer had started making a permanent magnet instrument in England that was also beginning to take a piece of the market. We got most of it back with the T60, our first permanent magnet instrument. Certainly the Perkin-Elmer R12 was one of the reasons we had to upgrade the A60 after about five years. There was always competition, and so sales played a much bigger role. I had to hire a sales manager.

I discovered an interesting thing about sales managers: they are very ambitious people. They aren't content remaining in their positions; they want to move up. They want to be marketing manager next, and after that, general manager. They're ladder-climbers. I suddenly realized, "I'm not a ladder climber. The last thing I want is to go from this marketing managing job to the general manager. I wouldn't have his life for anything." [laughter] I had to think about that, and I thought, "I'm sitting in the way of my other people, and this is an unstable situation. I can't sit and block people, and yet the structure and the size of this organization didn't give them other opportunities. My job is to get out of the way of my sales manager." There was only one way to do that, which was to change jobs, and there's only two ways to do that.

[END OF TAPE, SIDE 7]

SHOOLERY: I understood why sales managers wanted to move up. Their life was just hell, particularly because the corporate structure sitting over them kept them from being their own bosses. Often, the sales managers weren't permitted to make the right decisions because they were overridden by their supervisors. I didn't want any part of sales management, so I had to choose between leaving the company and going back to what I was doing. I felt more comfortable doing technical work than making strategic decisions. The decisions usually came down to a choice between the more pleasant of two miserable alternatives, and I was uncomfortable with such choices. I decided to go back to the laboratory. Everybody agreed, and I returned to managing the application part of the company in 1965.

BROCK: During that time period, hadn't Roy Johnson been managing the applications lab? How did he handle your new business relationship?

SHOOLERY: He still reported to me after I switched back to my old position, and he was satisfied with that. I got out of his way a few years later when I left Varian for about three years. When I came back, three years later, I remained part-time for four years, and then rejoined the company full-time. By that time period, I had realized that I didn't want to manage either the marketing department or the applications lab; actually, I had lost the desire to manage altogether. I felt that research and development made better use of my skills and interests, so I decided to work there. I passed up the opportunity to come back as the applications laboratory manager and said I'd much prefer to come back as an applications chemist. I continued in that role for the rest of my career.

BROCK: Were you left open to explore whatever you wanted in your new position?

SHOOLERY: I was so used to deciding what to do and doing it, and was successful enough at what I did that very rarely did anybody say, "No, you can't do that," or, "No, we need you to do this." As time passed, managers put me in charge of projects that I normally wouldn't have volunteered for because they knew that I could get the job done. I had to pay my dues there, but even then I had a lot of freedom.

I am very grateful for the freedom that I was given while working for Varian. In thirty-seven years of working for the company, I almost always did what I wanted to do and not what somebody else thought I should have done. Except for certain projects that absolutely had to be done, I basically wrote my own job description. One project that had to be done, for example, involved programming the computer of an instrument with a menu system so that an idiot could run the instrument by pushing the right buttons. I was assigned to do the programming because I was the only person who knew the proper steps to operate the instrument. The project was to develop an "artificial intelligence" using a menu system. While I didn't really enjoy doing it, I tolerated the job because I realized how important it was.

I had one manager—I won't mention his name because I wouldn't want to put this on him—who was an officious person and needed to have authority. We often thought differently because we were very different people, but he was in charge, so I had to do what he said. He wrote my evaluations regularly, and it was not fun working for him. It was, at times, quite aggravating. Luckily, there were other changes in the lab, and pretty soon I was working for somebody that I got along with much better. In any event, that was the only time I was ever uncomfortable working with a lab manager.

I often worked on my own time and I rarely ever spent the company's money. I often worked late and on weekends as needed. My managers generally didn't care about my side projects because I finished all of my necessary projects on time. Often, I came up with new things that could be taken on through development and added to the line. I had the best of both worlds, but I think that I earned that over quite a few years. People were used to the way I operated, and so I was given freedom as long as I didn't abuse it.

BROCK: Were you then the "product champion" for the T60?

SHOOLERY: Not really. I had a lot to do with the T60 concept, but not that much to do with the development of the unit. It was kind of a routine machine that filled a niche, and it didn't really need a "product champion" battling to maintain it. Emery Rogers and I created the concept for the T60 in Zurich, Switzerland, when we were at a meeting of the application laboratories. Those workshops were very popular in Europe, and top chemists from all over Europe fought for a spot to get in to them. I went to the meetings to give lectures, as would some European chemists. They were really very good workshops, and they strongly established Varian as the supplier of NMR in Europe.

During our visit to Switzerland, Emery and I visited a tearoom in downtown Zurich. While sitting at a table, we discussed the future instrument that was to follow the A60. Both of us agreed that it needed a permanent magnet design, as well as other particular specifications that were significant improvements over the previous design. Since we had thought of the concept in the tearoom in Zurich, we decided to name the instrument the T60.

Our original concept for the T60 was elaborated with engineering designs and developed by Varian. The T60 was finished just in time to be installed in both the applications lab in the Pittsburg airport, and the New Jersey field laboratory. In addition, we installed one in my California lab, which I used as a demonstration instrument for our small customer base in that area.

Right after my three year hiatus, an instrument had been developed to which I had contributed in the early planning stages. While I was away from the company they actually converted it into a reality. The instrument was called the CFT20 [Carbon-13 Fourier Transform]. That instrument was designed to analyze carbon instead of hydrogen. The concept was simple. With previous instruments, we had been analyzing the protons attached to carbon atoms. However, the CFT20 analyzed the carbon directly, giving a much better analysis of the sample. It hadn't been technically possible to directly examine carbon atoms until the early 1970s, when it was then incorporated in to the design of the instrument. It was named it the CFT—FT meaning Fourier Transform, because that technology was indispensable for getting the extra sensitivity we needed for the Carbon-13 analysis. The frequency was 20 MHz, so we named the instrument the CFT20.

A major design flaw of the CFT20 was its inability to analyze hydrogen. The instrument needed to have two, interchangeable frequencies so that it could analyze both elements. When it was modified, we renamed the instrument the FT80. It could tune either to 20 or 80 MHz and analyze the two elements at their field strengths; carbon at 20 MHz, and hydrogen at 80 MHz.

Varian executives made me the applications chemist for that product line, and a different applications chemist was assigned to the NMR instruments that were becoming more and more attractive to chemists. They were more powerful and used higher fields: 200 MHz or 300 MHz. I worked with the FT80 for several years, and had developed a microprobe for it; one that used capillary tubes, and looked at very tiny samples. We were able to show organic chemists that if they had a very small sample, they could still do experiments with the FT80. That characteristic was very important for natural-product chemists, whose samples were limited by how much of a sample they could gather in nature.

I became very interested in natural-product chemistry; and later in marine natural-product chemistry. Working with the latter involves analyzing complex organic molecules, many of which have strong biological activity. They're the source of powerful drugs, anti-cancer compounds, and all sorts of valuable chemicals. I tried frequently to persuade Varian to develop certain accessories and techniques that would hasten the advancement of natural-product chemistry.

For the remainder of my career, natural-product chemistry, and its various subfields, remained my main interest; although, every so often that work would be sidetracked by the needs of the Varian instrument division. No matter what work I was doing, my goal had always been to solve difficult chemical-analysis problems so that consumers remained interested in Varian spectrometers.

BROCK: Were those accessories and modifications for the FT80?

SHOOLERY: We started making them with the FT80, but then we found that the price of an FT80 wasn't that different from the price of a 200 MHz instrument based on a superconducting magnet because the prices of superconducting magnets had dropped. For quite a few years in the early part of the 1970s liquid helium wasn't available. If it was, it was twenty-five dollars a liter, which made it expensive to run a superconducting magnet. After the helium supply increased, the price for liquid helium dropped and an efficient distribution system was developed, which made superconducting magnets the absolute best choice. The FT80 suddenly became too expensive for the market, and its sales decreased dramatically. Chemists were willing to spend a little bit more money to buy the much better superconducting-magnet instrument. The future belonged to superconducting magnets after about 1974 or 1975.

Every couple of years, the field strength of NMR instruments would increase. For instance, we went from 200 to 500 MHz instruments in about six years. Six Hundred MHz instruments were developed about four years later because they had serious technical problems. Oxford Instruments couldn't solve those problems on schedule, but they finally came through a year or so late. Then 600 MHz instruments were created, which had ten times the field strength of the A60 and produced ten times the dispersion in the spectrum. Since that time period, engineers have created a 750, 800, and 900 MHZ instrument. There is only one 900 MHz instrument, and it costs about ten million dollars. Generally, applications instruments that expensive are used in the biochemical field were there is a lot of research funding.

The average organic chemist has at least a 500 MHz machine and a fair number of them are purchasing 600 MHz instruments. There is a dramatic price increase for instruments stronger than 600 MHz, and I doubt that manufacturers of the higher field instruments will build a volume production model any time soon. The number of dollars available in the marketplace typically remains constant, but every time a new instrument is introduced all of the upper echelon labs <u>need</u> to purchase it; so the number of sales always exceeds our expectations.

BROCK: After the FT80 had been developed, were you involved with the development of any new instruments?

SHOOLERY: Yes. We designed an easy-to-operate superconducting magnet system to replace the A60 on the market. When compared, the former had much more dispersion in the spectrum, much more sensitivity, and a greater natural stability than the latter. In addition, the superconducting magnet system had a computer system, which had just been developed by that time period, that could do all of the very complicated experiments involving multiple pulses and two-dimensional spectra.

We developed a machine called the Gemini also. It was a 200 MHz instrument, which was quickly followed by a 300 MHz model: a premium model that gave the operator even more performance. The 300 MHz instrument was designed to do all of the exotic new experiments that had been made possible with Fourier Transform technique and the invention of multi-pulsed, two-dimensional NMR. The instrument greatly increased the information we could gain about the system, and allowed us to solve very complex problems with concise and accurate techniques.

As a detractor, the instrument appeared more difficult to operate than previous models. Consumers usually put it in a spectroscopy lab and assigned a spectroscopist to run it. If the spectroscopist was lazy, he wouldn't take samples from everybody, work them through quickly, and return them; rather, he would be picky about the samples he selected. As a result, it was very common for analytical chemists to complain about the centralized services in large analytical laboratories.

Gemini was a simplified instrument that ran with computer menus. An operator activated macro commands by pushing a menu button, and then the instrument performed a predefined sequence of operations, and interpreted the results. We had an advanced computer language, which I didn't understand, developed specifically for the menu functions. I just had to know which operations could be performed with which commands, and then I could write a macro command that with a string of operational commands. I never enjoyed working with computers, so I hadn't been keen about my responsibilities for the development of that particular instrument; but I knew what operations needed to be done and in what sequences, so I was assigned to design the menu system for the Gemini spectrometer.

When I began the programming for the Gemini, I found it quite challenging and enjoyable; though I was glad when I finished the project. I had wanted to solve chemical problems, not write the software. That was the only software I ever wrote, but it worked out very well. Since the development of that menu system, numerous variations have appeared that increased the automation of NMR instruments. Contemporary operators simply select their experiment and work space in the computer, insert their sample, and press "Go." Then, the operators can tend to other work while the instrument runs the experiment for them. We even have a sample changer on the machines that will pick a new sample, drop it in the machine, and make sure it's spinning. It will automatically optimize the homogeneity of the magnetic field also. Further, it will run the experiment, print out the results, and put them in a hopper like an automated lab assistant!

The technology used in the Gemini was developed during my last few years at Varian. Though most of the technology had been developed simultaneously, we had to test everything. I quickly discovered that testing new hardware and software was a very repetitive, uncreative, and tedious job; needless to say, I didn't enjoy it, but I survived by switching tasks occasionally and not letting the testing dominate my work.

BROCK: How do you feel about the NMR field's contemporary competitive market?

SHOOLERY: It is a very healthy situation. Of course, now that I'm not in the marketing department, I no longer have to listen to a customer tell me how well a Bruker machine works, and worry that I might lose a sale; I'm able to be more objective. Consequently, I've grown to believe that the market is much better with Bruker, because without them, Varian wouldn't have to develop new products aggressively to beat the competition. Because of the competition, the NMR market has expanded faster than ever, which has resulted in increased revenue for all the parties involved.

BROCK: How has the recent competition in the market affected Varian's growth?

SHOOLERY: Varian's executives have never hired as many organic chemists as they've needed. They've always been conservative about manpower because it's expensive. Nowadays, Varian has to watch its expenses because Wall Street is very sensitive to bad business moves, and management is very sensitive to Wall Street. Profit projections must be met to be successful, which usually requires spending cuts and leads to running lean. Such business methods have their disadvantages, but they also decrease the vulnerability of Varian if it fails to reach one of its objectives. I don't want to put a value judgment on conservative business practices, but I can see their consequences. Varian probably misses a lot of opportunities, but the rewards for taking risks aren't worth the penalties for failures; the penalties are always much more severe.

That was the most frustrating part of my consulting work: after I was no longer in the thick of things, I was useless. Though I could step back and see the opportunities out in the marketplace, we rarely had the resources to take advantage of them. In addition, no one wanted to start a project that I was promoting because they were too busy doing their day-to-day work. I was able to get a few things done as a consultant, but I was never able to get as much done as I really wanted to. Actually, I've grown tired of the consulting position's shortcomings, and I've been thinking about retiring from that type of work; I'm getting too old for it! [laughter]

BROCK: For how long have you been consulting?

SHOOLERY: From 1990 to the present.

BROCK: I'd like to ask you about that brief period when you stepped away from the firm. What were you doing during that period?

SHOOLERY: In the late 1960s, Varian's NMR development programs became stagnant. The stagnation occurred because the company was having trouble finding managers who could handle the company's challenges, and executives who could make good marketing decisions. Without strong leadership and capable management, the company lost direction and quit producing new products.

[END OF TAPE, SIDE 8]

SHOOLERY: My work just seemed to be less and less challenging. As the progress of the company slowed, my dissatisfaction with my job increased. Simultaneously, I was having some real personal problems in my life. I had decided to make a major change in my entire life by spending more time with my wife and doing the things that she found enjoyable. She was interested in bringing about change in peoples' lives and I felt that I could join her in exploring various groups that were working towards that goal. Of course, one still finds many people trying to do that. It's a movement that began in the late 1960s and continues today.

I left my job at Varian and tried a number of other things that involved personal growth through group activities. For instance, I worked to improve the parenting skills of others through a group that used some of psychologist Carl R. Rodgers' principles. Rogers is quite famous for "active listening," which was what my group taught to parents in Los Angeles [California]. In fact, I learned how to do it to a degree, and I can see how it works. How well one "active listens" depends on how mature one is. It is difficult to do properly, and if a parent does it wrong, he or she seems patronizing to the child. Nevertheless, it can be a very powerful way to get through to a rebellious child. Incidentally, the same technique is also the basis of some management training seminars; so it is a technique worth knowing.

I tried teaching the technique for a while, but I really didn't like how the program was organized. As a teacher, I had to organize my own class, whose members would then pay me to teach. So right in the beginning there was a disconnection between me and my students. I found also that it was a huge amount of work. I couldn't charge enough to really make it pay anything, and besides, I found out that I couldn't do it with my own kids. I thought, "if I can't do it, what am I doing teaching it?" [laughter]

That was a good experiment and I don't regret doing it. I learned a lot about group dynamics by being in many different kinds of groups. I learned how groups operate, how they come together, and the motivations of their members; there were lots of people who really wanted to be gurus. Most of the teachers were after power more than anything else, which I only figured that out by getting beneath the surface. I realized that the program was a humongous morass and nothing constructive was ever going to come out of it. Ultimately, I decided to go back to work at Varian

Fortunately, there was a job open for me at Varian. When I originally decided to leave Varian, I had been offered a leave of absence, but didn't take it because I didn't want a safety net; I wanted to take a chance at something new. When I went back to Varian, I simply asked if they had any openings. I was told that the applications lab's managerial position was about to become vacant, and I was offered that position. I didn't want to be the lab manager, so I requested a position as a chemist under the future lab manager. My request was granted, and I remained an applications chemist from 1973 to 1990. During that time period, I did my most exciting work because the instruments had become much more powerful, and the market was much bigger.

Once I had come back to Varian, I realized that that was where I belonged. The period where I was kind of disillusioned and felt things were stagnant was temporary. I ultimately realized that my marriage really wasn't going to work. My first wife and I divorced, and I found a life partner to whom I've now been married for thirty years. It's really been a very fulfilling thirty years.

My three year break was simply something that I had to do. I would have had a nervous breakdown had I not done anything. I decided to do it that way, and I think it was for the best. It ended up well, and I was very fortunate to be rehired at Varian. They knew me quite well at Varian, and were glad to have me back. I suppose I didn't completely cut the string. I had continued to consult and occasionally traveled to workshops for Varian. I kept up on the new developments technically and I traveled with people who were working on those workshops. It wasn't like I had disappeared, but those workshops were the only thing I did during that time period.

BROCK: Those workshops must have been important.

SHOOLERY: They kept me in the flow of things. If I had come back without having participated in the workshops, I probably would have been lost. But everything worked out fine, and I learned a lot of things about people that I had never learned before; I learned about their needs and what's really important. So it was the right move.

BROCK: I think I've asked all the specific questions that were focused on the course of your career, but I would very much be interested in your general reflections in a number of areas. One is the enormous change in the status of NMR from when you began working on it to the present. I'd be very interested to hear your thoughts about where it started, what it has become, and what you feel the consequences are of that development.

SHOOLERY: You have to realize that you're definitely getting a biased point of view. When Emery Rogers, Martin Packard, and I started working with NMR, we strongly believed that high resolution NMR could be important to chemists if it were made more useful than infrared spectroscopy. I think that we achieved that goal when superconducting magnets became widely available.

Today, most organic chemists would agree that NMR is more useful than infrared spectroscopy; certainly, they would agree any modern chemical lab requires an NMR instrument. Though both IR spectroscopy and NMR are designed for different kinds of research, it seems that IR spectroscopes are being used much less than they have been in the past. NMR has given organic chemists the ability to get results quickly and clearly. It has increased the productivity of organic chemists enormously; but, it has had a disproportionate effect on the whole field of chemistry. At the same time, NMR has proven recently to be more and more applicable to inorganic chemistry because inorganic chemistry involves atoms other than carbon and hydrogen. Most of the important catalysts in organic chemistry are now inorganic complexes. The structures of the inorganic complexes are extremely hard to figure out with ordinary spectroscopy, but NMR tells a scientist where all of the atoms are located, so it's really the most potent structural tool for compounds, such as catalysts, that are stable in liquid solution. In a way, I think it has almost revolutionized the way that chemists approach their work and increased chemists' productivity.

Also, NMR technology has continued to expand into biochemistry. The structures of proteins are now routinely determined with NMR. While one can get structures of some of the most complicated proteins only by x-ray crystallography, crystalline proteins do not resemble the actual configuration of the proteins when they're active in solution. Even though it helps to know the structure of a molecule in crystalline form, chemists are more interested in the structure of proteins in solution. X-ray crystallography doesn't give a chemist the complete picture. Conversely, NMR has had a huge effect on the understanding of protein structures, protein folding, and so forth. It also has been applied to the various DNA [deoxyribonucleic acid] and RNA [ribonucleic acid] molecules.

Regardless, the two methods are more complementary than competitive. For example, unlike NMR, x-ray crystallography can examine insoluble proteins and very low concentrations of proteins. Of course, as analytical instrumentation has become more powerful, instrument sensitivity has become less of an issue. Computers have simplified the analysis of very large amounts of data from complex molecules in both the NMR and x-ray instruments.

Pharmaceutical companies have found that NMR is probably their most indispensable tool because it's applicable in determining the structures of natural products. At one point, we started out to make a microprobe for the superconducting systems. It was difficult to make a microprobe for a high field instrument because the small sample size used in the high frequency instruments required us to build a small, compact coil. However, we discovered that spinning the sample at a high rotational speed, at a particular angle to the magnetic field, would homogenize the sample and provide natural line-widths in its spectrum. The process was very effective at producing high-quality spectra from samples that had produced terrible spectra previously. Eventually, we changed the name of the microprobe to nanoprobe because it was too sensitive for samples smaller than a nanogram.

The spinning method was very helpful for scientists working on polymerization synthesis. The polymerization process is quite amazing: amino acid chains are formed on polystyrene beads by combining amino acid reagents, one at a time, to a previously “washed” surface, which subsequently builds a chain. When that process was first introduced, the synthesis was very efficient, but it was also very difficult to interpret the spectra of the molecules that were attached to the beads. A scientist couldn’t use NMR to analyze the sample because the polystyrene beads that the amino acid chains were attached to had a different magnetic susceptibility than the surrounding fluid and that would broaden the lines of the spectrum by distorting the magnetic field. To solve that problem, we spun the beads, with the attached amino acid chains, while we ran the spectrum. The spinning nullified the magnetic susceptibility of the polystyrene beads, subsequently sharpening the spectrum. That technique has since been used in the combinational chemistry business to make analog compounds for the testing of new drugs and their biological effects on people.

I was interested in developing combinational chemistry during the last part of my career with Varian. I failed to ever get it established as a project while I was still working at Varian, but once I became a consultant, I managed to talk Varian’s executives in to setting up such a project and letting me direct the engineers on the project. We have a product on the market currently. Starting that project and building that product was my biggest accomplishment as a consultant.

NMR has been accepted into almost every branch of chemistry. It has become a valuable tool in the polymer field because scientists can see the details of polymer structure; before, scientists could only speculate about such structures. NMR has been a revolutionary instrument for organic chemistry and other related fields. It has increased productivity so much that chemists have seen a virtual explosion of new knowledge.

BROCK: It seems that the NMR spectra are generally considered as almost synonymous with the structure. Is that true?

SHOOLERY: Yes. The connection between structure and spectra is a very straightforward one by comparison with any other spectroscopic technique. NMR spectra are really only connected to a couple of parameters in the structure, so the relationship between them is very easy to see. NMR is so appealing to chemists because they don’t need to learn a lot about spectroscopy to interpret the NMR spectrum. In addition, we have provided chemists with instruments that don’t require a lot of electronics knowledge to get the data from an experiment. At this point, NMR has probably had the biggest impact on the field of chemistry than any other analytical instrument. Of course, that is subject to debate and I am arguing from a somewhat biased position, so I qualify it.

BROCK: Would you reflect on your entire experience at Varian?

SHOOLERY: Varian is the only company I've ever worked for, so I can't really compare it with other companies. Since its foundation, Varian has been successful because its executives have always been willing to invest in product research. Compared to its competition, Varian has been able to produce a superior product and charge more for it because of Varian's research investments. While Russell Varian had managed the company, he strongly believed in that business strategy. Subsequent managers have absorbed enough of Russell's philosophy to continue it. Of course, they've had to modify that philosophy to meet the requirements of the Wall Street analysts, but on the whole, they've done a very good job of walking that tightrope.

In particular, it was a very good move to separate the company into three independent corporations because it eliminated a huge overhead of corporate staff whose services were unnecessary. Further, the original company's bureaucracy inhibited the flow of information and decreased the staffs' productivity. Any contemporary business must react quickly to market changes, and Varian couldn't do that when it was a single company.

I have to give the credit to J. Tracy O'Rourke, the new CEO [chief executive officer] who was brought in when Tom Sege finally retired. Tom Sege came after Ed [Edward L.] Ginzton. When Tracy O'Rourke was hired, he knew very little about NMR, but he did know how to manage a company and turn it in to a profitable operation. He managed to do that with Varian. He certainly had to clamp down quite a lot on some of the ideas of the technical people, but he did manage to keep enough research going to meet the objectives in that area and to bring the profit position into line with what people were expecting. While we technicians sometimes thought that O'Rourke put more effort into business decisions than technicians' problems, he did get the job done. He was the prime mover in separating the company into the three parts that are now operating so successfully.

The consequence of splitting the company was that Varian stockholders' holdings tripled. The sum total of the three companies stocks was thirty-three the day after the split. At the moment, the sum of those three numbers is about one hundred and forty-seven.

BROCK: In three years?

SHOOLERY: In about three years. The most successful corporation of the three is the Medical Systems. It's been the most tightly managed company, and it has doubled its value in about three years.

BROCK: You have the semiconductor equipment business, the instrument business, and the medical equipment or medical instrumentation business. How did those businesses emerge within Varian over time? How did the product line in medical instrumentation begin, or in semi-conductors?

SHOOLERY: After 1960, Varian established a central research department that focused on exploring new technology. Soon after, Varian created a vacuum department and eventually became the leader in vacuum technology. Then, the vacuum department's engineers discovered how to implant ions in to silicon wafers to create blanks for computer chips. The vacuum department built a line to manufacture those silicon wafers even though they were uncertain how large the market was for that product. At this moment, that Varian offshoot is one of the major suppliers of computer chips and ion-implanted silicon wafers in the country.

After the development of ion-implanted silicon wafers, the Varian research department went into three-dimensional x-ray imaging, and developed a CAT [computer-aided tomography] scanner. Though the machine was wonderful, Varian didn't have the faintest idea how to market a medical instrument to the diagnostic clinical market. All of their marketing attempts had failed, so they gave up and sold the design.

[END OF TAPE, SIDE 9]

SHOOLERY: Varian Inc., was the first company to make a CAT scan instrument. After the CAT scan project failed at Varian, the people working on that project decided to go off on a different project. Wes [Weston A.] Anderson, who had been a major contributor in the early years in NMR, but who had moved out of NMR years before, went to Central Research. While he was working on the CAT scanning project, he became interested in ultrasonic imaging and developed an ultrasound scanner; however, Varian's attempts to market that instrument also failed. Subsequently, Varian sold the instrument to a company that now manufacturers and markets ultrasound scanning instruments.

MRI [magnetic resonance imaging] was developed soon after then CAT [computerized axial tomography] scanner. Varian didn't have anything to do with the development of MRI; we had been focusing on the analytical instrument market for NMR during that time period. After the marketing failures of two expensive instruments, the board of directors was unwilling to invest in another large, expensive instrument; especially when such an investment would have put Varian in competition with GE and Siemens [AG]: two companies that were willing to pour several billion dollars into that field.

Varian couldn't afford to compete with the mammoth companies that already had a well-established marketing position in the medical instrumentation field. Instead of competing directly with those companies, Varian manufactured small micro-scanners for the vertical bore solenoids used for high resolution NMR. They made an imaging accessory also used for examining minute objects that an operator would want to image. Recently, Varian has developed a whole body scanner for research purposes. The instrument uses a much stronger field than a typical diagnostic instrument. Varian has marketed that instrument very well because they are selling the instrument to a consumer market they understand: the medical research market. More specifically, Varian is selling to researchers who are using new modalities for scanning to get a better understanding of what's going on inside the human body.

Varian has recently become involved with fMRI [functional magnetic resonance imaging]. The fMRI instrument can measure the blood flow changes in a patient's brain by shining a light in the patient's eye. The detail of the image depends on the strength of the magnetic field emitted from the magnet: the stronger the magnetic field, the more detailed the image. Using fMRI, scientists have discovered many new things about the functioning of the brain.

Many engineers and medical specialists have acknowledged Varian's competence in the cancer treatment field since Varian's development of the Clinac high-energy x-ray generator. Still, Varian Inc., the scientific instrument company, has not transferred any imaging products to Varian MS, the medical company, because the technical knowledge for the NMR aspects of the Clinac instrument has always been in the instrument company. Maybe someday the two companies will form a joint venture; or maybe Varian, Inc., will shift some of its resources to Varian MS. Regardless, a significant share of Varian's total sales have resulted from the fMRI scanners, which incorporate sophisticated analysis equipment, advanced computers, and a very powerful, expensive magnet in one instrument.

BROCK: Discuss some of your interests and pursuits, over the past thirty years, outside of Varian.

SHOOLERY: Well, my second wife [Judith L. Shoolery] and I have six children between us, each of us bringing three to this marriage. When we first married in 1971, thirty years ago, our youngest child was about six, and the oldest was about eighteen. The oldest one is now approaching fifty and the youngest one is now in his thirties. During the first years of our marriage, my wife and I were very busy taking care of our kids. In the 1970s, which was a rather turbulent time for raising kids, we had five teenagers in our house, so we didn't have a lot of time for each other. Illegal drugs had been prominent in American society, and so it was a constant struggle to keep everybody on track. Now that they are all grown up, our children are doing quite well, and my wife and I are very proud of them.

My wife graduated from Kalamazoo College, a liberal arts school. She received a very good classical education from the school, and she makes up for my lack of formal education in that field. We're both interested in liberal arts, so we're fairly active in trying to experience lots of cultural activities. We try to stay active by reading books and try to keep the cultural side of our lives healthy and active. She's been working on a book with Edward Teller—his memoirs—and I have been consulting, so we really haven't been retired. Although both of us retired officially and had drawn our Social Security in 1990, we went right on working as consultants. Not full time, but enough to keep us pretty busy.

We've done quite a bit of traveling. Having traveled all over the world while working for Varian, I made a lot of very close friends in foreign countries. It was a real privilege to travel so extensively for Varian; to arrive in a foreign country and not be a tourist who was prey for the locals, but rather a person who was making a contribution. I missed some of the great tourist sights in some of those countries, but I really came out way ahead by becoming friends with the people that I met.

My wife and I are hoping to go to Europe in the fall to see some of these dear friends. We're all getting old now, so we're hoping to travel to Germany, Switzerland, and Italy to see our friends one last time. My wife and I have gone to the Soviet Union on three different occasions, and we made some very dear friends there; but they've warned us that it is too dangerous to visit these days. [laughter] My wife and I have gone to Japan and China together once, also. The Chinese treatment of women was a real eye-opener for my wife. She noticed that the wives of the businessmen I was meeting with were obviously treated as inferiors. We're kind of interested in the social side of international relations.

After having lived in Half Moon Bay, California, for three-and-a-half years, we're just beginning to make friends in that community. While we have some new friends in that community, until recently we've been too busy to socialize with the rest of the community. My wife just joined the University Women's Club, and I'm in the process of joining the Rotary Club over there, so we expect to be more active in the local affairs. I've rarely been involved with social clubs, except for my membership in the YMCA [Young Men's Christian Association] during my thirties, so my membership in the Rotary Club will be a relatively new experience for me.

BROCK: Has your wife always worked as a writer?

SHOOLERY: In one way or another she has. She taught junior high science classes for a few years after graduation from Kalamazoo College. For several years after we married, she didn't work because we had our hands full with the kids. For a couple of years, I only worked part-time because my wife had been overwhelmed initially by the responsibility of caring for six kids. I worked part-time, and my wife and I worked on expanding our house in Palo Alto and taking care of the children.

I eventually went back to work full-time, and my wife, deciding that she needed to have more contact with people outside of our home, went back to college to get her Ph.D. degree in developmental psychology. She had a very good aptitude for it. She learned statistics because human behavior has to be dealt with statistically. She enjoyed going back to school, but she just had to drop it. She was well on the way to picking a thesis subject and going for the gold, but instead she decided, at a critical time in raising the children, that she really had to spend more time applying her knowledge at home. I think it was a wise decision, but she had had to give up her dream of getting a PhD as a consequence.

She got an interesting job at Hoover Institute at Stanford as an editor of Edward Teller's publications. She became very close to him and published several books with him (5). She finally managed to talk him into writing his memoirs, which he had originally avoided because he thought such a book would make him seem conceited. She convinced him that that was an important thing to do, and so they've worked on that for about ten years. Now that it's in the bookstores, she's ready to retire. She's acknowledged as co-author on the cover. Though it may not sell like a Stephen King novel, the book was a labor of love that she can point to with pride.

Judith did some writing in the late 1950s and early 1960s also. She contributed to a young person's encyclopedia on science. She took a degree in biology at Kalamazoo College, so she was an obvious resource for writing some of the information for the encyclopedia. In addition, she's edited an environmental publication, and has done some other writing here and there.

As my wife and I have become older, it has become more important for us to pace ourselves. I've reduced my consulting workload, and I'm probably pretty close to terminating it completely. I have been working for the last six years on another consulting job with a small company that's been trying to develop a home-based medical instrument that uses NMR. The instrument, when completed, will be used to measure a patient's glucose level without having to stick a needle in his or her finger; something which diabetics simply won't do, regardless of the health consequences. With a non-invasive method for blood-sugar measurement, diabetes sufferers would be more likely to check their blood consistently, which could reduce the terrible side effects of diabetes by 70 percent. We thought the project was worth undertaking, but unfortunately we have found that NMR signals from the glucose in human tissues are extremely difficult to detect.

In fact, I am unaware of any instrumental method that has detected glucose in human tissue other than the brain; and even those brain observations are questionable. Nevertheless, we've done some excellent NMR research and we've found that we can detect the glucose levels present in the body at the molar concentration present in serum. We can detect glucose in the water of the human body, but it has twenty thousand times more protons per unit volume and sits right next to the glucose peaks. We have yet to see the glucose in the blood, so we haven't started working on the tissue problem. We're running out of money, so the project will probably be set on the sidelines. We won't be able to do any more work on it unless somebody comes up with a better idea.

The project has been very interesting and challenging. I've learned a great deal, worked with some very interesting guys, and been compensated very well for what I have done; I'm just sorry that it hasn't been successful. I like to bring a project to a successful conclusion, but there is no guarantee in research. It's unusual to conceptualize a project, form a company, and get venture capital for an unproven instrument. We're trying to do the original research with venture capital in a separate corporation. It's not a particularly efficient approach because we don't have a principal investigator; rather, we have a bunch of independent consultants. I would call it a failed experiment, but it's been very interesting and has kept me busy. We're really at the point now where we're sticking our nose out to see what's around, and if we don't see our shadow, we'll come out of the hole and get involved in some other projects.

BROCK: Is there anything that you would like to add?

SHOOLERY: I am very grateful for all of the opportunities that I have had in my life. Some of those opportunities were simply the result of my being in the right place at the right time; being able to recognize an opportunity and pursue it. I came in on the ground floor of NMR, and worked through its formative stages to where I can see the mature product and realize its significance to science. The impact of NMR on chemistry has been tremendous, and it has been a rare privilege to contribute to its development. I can walk away from it now and feel like I really spent my life doing what I wanted to do.

As I look at NMR instrumentation, I don't see any limits on it yet. It doesn't look like it's slowing down; it's still accelerating as the applications broaden. Scientists keep finding new areas of application as the instruments become more powerful. As the definition of the nuclear energy levels becomes sharper, scientists are able to be more and more selective about the information they want to extract. It seems like anyone entering the NMR field could go another fifty years and still find opportunities. It's the most open-ended type of instrumentation that I've ever seen, and so it's nice to know that I haven't come to the end of the road and stopped. I've just stepped off the train, but it keeps roaring on. [laughter]

BROCK: Thank you very much.

[END OF TAPE, SIDE 10]

[END OF INTERVIEW]

NOTES

1. See for example:
   Joseph William Mellor, *Introduction to Modern Inorganic Chemistry* (New York: Longmans, Green, 1914).

2. Gilbert Newton Lewis, *Thermodynamics and the Free Energy of Chemical Substances* (New York: McGraw-Hill Book Company, Inc., 1923).

3. J. T. Arnold, S. S. Dharmatti, and M. E. Packard, "Chemical Effects on Nuclear Induction Signals from Organic Compounds," *Journal of Chemical Physics* 19 (1951): p. 507.

4. See for example:
   Carl Djerassi, T. Nakano, A. N. James, L. H. Zalkow, E. J. Eisenbraun and J. N. Shoolery, "The Structure of Genipin," *Journal of Organic Chemistry* 26 (1961): pp. 1192-1206.

5. See for example:
   Edward Teller and Judith L. Shoolery. *Memoirs: A Twentieth Century Journey in Science and Politics*. (New York: Perseus Publishing, 2001).

Addendum I

150[illegible]erin Avenue
Albany 6, California
July 13, 1951

Rec'd 7/1[illegible]

PASS TO	DATE READ
HMS	7-18
CGM	

H. Myrl Stearns, General Manager
Varian Associates Inc.
San Carlos, California

Dear Mr. Stearns,

During a recent visit to a display of electronic equipment in Los Angeles I was impressed by the variety of products now offered by your corporation, particularly the nuclear magnetic resonance apparatus. This suggests the possibility that a still more extensive line of scientific instruments of interest to the research worker in physics and chemistry may be forthcoming. It has occurred to me that a need for men with training in the fundamentals of these fields may be arising in your organization; in particular, the field of physical chemistry would seem to tie in quite closely. Therefore, I have decided to inform you of my interest in your company and to list briefly my qualifications.

I have just received my Ph.D. degree in physical chemistry at the California Institute of Technology where I built a microwave spectroscope and carried out some work on molecular structure with its aid. My undergraduate work was taken at the University of California at Berkeley. In addition, I have had contact with radar equipment through the training program of the U. S. Navy during World War II.

I believe that my training in chemistry and physics provides me with a basic understanding of the phenomena underlying the development of new instruments. Although my training in electronics has been of a more practical nature, I have recently found it profitable to begin to acquire a knowledge of the more powerful formal approaches to the subject. I feel that this background, coupled with a consuming interest in instrumentation, qualifies me for consideration.

I shall be at the California Institute of Technology during the coming year, associated with the Kellog Radiation Laboratory. Specifically, I will be available to assist with problems of a chemical nature arising in the work with the Van de Graff machines and to carry out suitable research that I may conceive that would utilize the special facilities available in that laboratory.

If you would be interested in a more complete account of my academic record I would be glad to fill out an application for employment, or, if you wish, to arrange a personal interview. I will be in this area until July 20th, from August 20th to 31st, and at various times throughout the coming year.

Yours very truly,

James Nelson Shoolery

James Nelson Shoolery

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