CHEMICAL HERITAGE FOUNDATION

ROBERT E. FINNIGAN

Transcript of an Interview
Conducted by

David C. Brock

at

Los Altos, California

on

4 December 2001

(With Subsequent Corrections & Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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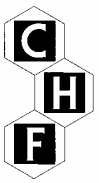

ROBERT E. FINNIGAN

1927 Born in Buffalo, New York on 27 May

Education

1949 B.S., United States Naval Academy
1954 M.S., electrical engineering, University of Illinois
1957 Ph.D., electrical engineering, University of Illinois

Professional Experience

United States Air Force
1949-1954 2nd Lieutenant, 1st Lieutenant with assignments to strategic Air Command, University of Illinois (student officer), U. S. Air Force Institute of Technology (Instructor), Ph.D. Special Student
1954-1959 Captain

University of California-Lawrence Livermore Laboratory
1957-1959 Head, Nuclear Reactor Control Group
1959-1962 Senior Scientist

Stanford Research Institute
1962-1963 Senior Research Engineer

Electronic Associates, Inc.
1963-1967 Director, Scientific Instruments Division, Palo Alto, CA

Finnigan Corporation
1967-1990 Founder, President, Vice Chairman, Senior Vice President
1990-present Vice Chairman Emeritus, Consultant

Honors

1975 Distinguished Alumnus Award, Department of Electrical and Computer Engineering, University of Illinois
1980 Alumni Honor Award for Distinguished Service in Engineering, College of Engineering, University of Illinois
1994 Pioneer in Analytical Instrumentation-Mass Spectrometry, Society for Analytical Chemists of Pittsburgh and Pittsburgh Conference on Analytical Chemistry

1999 Winston Churchill Medal of Wisdom
1999 Wisdom Hall of Fame
1999 Instrumentation Hall of Fame, Pittsburgh Conference on Analytical Chemistry and Analytical Chemistry Society
2002 Robert E. Finnigan Professorship established at Keck Graduate Institute of Applied Life Sciences, Clairmont, CA, by outside donors to Keck

ABSTRACT

Robert E. Finnigan begins the interview with a description of his family and childhood years in Snyder, New York. Finnigan developed an interest in military service and science while reading *The Dave Darrin Series* about a new recruit at the United States Naval Academy [USNA] and while building World War II model airplane replicas as a young boy. After entering the USNA in 1945, Finnigan became fascinated with electronics and realized that he wanted to continue his electrical engineering [EE] education at a graduate level, so he enrolled in an Air Force Institute of Technology program, which allowed qualified officers to enter graduate school after three years of service. While in the AFIT program, Finnigan met and married Bette E. Finnigan. In 1952, Finnigan became a "student officer" in EE at the University of Illinois at Urbana. After receiving his Ph.D. in 1957, Finnigan joined the Lawrence Livermore National Laboratory [LLNL], formerly the Lawrence Livermore Radiation Laboratory. While at LLNL, Finnigan worked on the development and application of nuclear ramjet reactors such as the TORY II-A and TORY II-C. Subsequent to working on ramjet reactors for five years, Finnigan decided to pursue process controls research at SRI [Stanford Research Institute]. At SRI, Finnigan became interested in the prospects for the quadrupole mass spectrometer as an advanced instrument for process control. As awareness of the quadrupole grew, Finnigan and his division were persuaded by EAI [Electronic Associates Incorporated] to leave SRI in order to start a process-systems group and quadrupole development. Finnigan remained at EAI, in the Scientific Instruments Division producing quadrupoles for academic and industrial use, for four years. In 1967, Finnigan resigned after EAI's attempt to sell the Scientific Instruments Division failed and EAI rejected his idea to venture into the GC-MS [gas chromatography mass spectrometry] market. Later that same year, Finnigan formed Finnigan Corporation with assistance from Roger Sant and T. Z. Chu. Via Finnigan Corporation, Finnigan continued to research and develop quadrupoles and GC-MS. Finnigan concludes the interview with a discussion of his hobbies and family, reflections on Thermo Instrument Systems' acquisition of Finnigan Corporation, and thoughts on the Finnigan Corporation of today.

INTERVIEWER

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.

TABLE OF CONTENTS

INTERVIEWEE: Robert E. Finnigan

INTERVIEWER: David C. Brock

LOCATION: Los Altos, California

DATE: 4 December 2001

BROCK: I'd like to start at the beginning, as they say, or even before the beginning, and talk a little bit about your family context, you know, in which you arrived in Buffalo, New York. You were born in 1927. I believe that your parents were Charles [M.] and Marie [F.] Finnigan.

FINNIGAN: Yes.

BROCK: Could you tell me a little bit about your parents, to begin with?

FINNIGAN: Yes. My father was a service engineer with New York Telephone Company, and he grew up in Utica, New York. My great-grandfather came to the U.S.A. [United States of America] from Ireland in 1850 (around there) during the famous potato famine, and settled in upper New York State. My grandfather worked for New York Central Railroad, which was the main railroad at that time in New York State. He was an engineer for the railroad, and raised his family first in Utica, and then moved to Buffalo where my father grew up. My father worked for New York Telephone Company his whole life. My mother died when I was about eight years old. I had one sister and five brothers, so my dad had quite a load after that part. I don't remember as much about my mother as I probably would have had she lived longer.

My dad brought us up in a suburb of Buffalo called Snyder, New York. We went to a Catholic grade school and then on to a public high school. I must say, I think, my dad was a very driven—I'd say—person who really required a lot of us. He was, at times, we thought, a real disciplinarian, as many parents were during that period, but he was always pushing us to do better things than he had been able to do, and was interested in seeing us go to college. We had to find our own way to do that because he had very limited means, especially with a family of seven.

BROCK: Did he get his technical training from the Telephone Company?

FINNIGAN: Yes, he did. He had been in various jobs with them, and he had basically learned on the job. His job was, you know, taking care of a bunch of customers, providing the service,

seeing that they got the service. Maybe at one time he did service himself, but he usually was the person that they would contact in big companies like Bell Aircraft [Corporation] and Curtiss-Wright Corporation. He had various large accounts like that. He didn't share too much of that with us. I guess, when he got home, he had enough to worry about. [laughter] Anyway, he really instilled in all of us, you know, that he wanted us to really do well, get a good education, and work hard. We grew up without housekeepers—actually doing a lot of the housework ourselves with his direction. [laughter]

BROCK: Did he remarry?

FINNIGAN: No, he didn't.

BROCK: Wow!

FINNIGAN: With seven kids, that was not an easy thing. But for whatever reason—I don't know all of his reasons—he didn't.

BROCK: He raised all of you himself?

FINNIGAN: Right. So he really had to be father and mother.

BROCK: I would imagine that would have been pretty uncommon, at that time, for a male head of household to also have the responsibility of raising the children.

FINNIGAN: It was. He was pretty unselfish. It's something I don't think I could do. It would not be very easy. He might have had a few marital prospects, but when they saw all of us, they said, "Forget it." [laughter]

BROCK: Were his interests also in the technological or science at all beyond work? Did he read in those areas?

FINNIGAN: He was quite good with his hands—he had a shop in the basement, and he could really make anything. We have lamps in this house and various other things that he made that are quite beautiful. That was his hobby, and he was really good at it. He also read a lot in many areas including technology. He always had a high interest in technology.

BROCK: Did he share that with you and your brothers?

FINNIGAN: I'd say somewhat. Yes. He showed us what he was doing, and we could watch him while he was making lamps or whatever. But I don't think any of us really took to that to the degree that he did. With seven children, it was not easy to find a lot of time. I think, very often he was looking to get a little relief from his job and from his responsibilities when he was working in his shop. He worked in his shop a lot. He had to take care of the house and everything because there was no period when we had a lot of help.

BROCK: Yes. Maybe we could take just a moment to talk about your siblings. I'd like to pounce on this now, because, I know from other interviews I've done, we often lose the thread of the interviewee's siblings and their stories. So I wonder if we could just talk about the life course of your brothers and sister.

FINNIGAN: Yes.

BROCK: Poor sister!

FINNIGAN: Right, right. I was the third of seven. I had two older brothers, and then there was one younger brother, my sister, and then two younger brothers, who were the two youngest. All of us had a close relationship. We lived in the suburbs of Buffalo, and we had a large piece of land around us that we didn't own, but it wasn't used. So we were able to build baseball diamonds, a football field—we played a lot of sports together. I would say that some of us—there were three of us in a row, a brother just older than me and younger than me, Jerry [Gerald C. Finnigan] and Denny [Dennis M. Finnigan], who were very close. We did everything together and were very protective of each other. Our oldest brother, Jim [James T. Finnigan], was assigned very often to be the caretaker in my dad's absence, you know, to look after us. So that sort of set him a little bit apart. We were about a year and a half apart on average, so we were pretty close in age. We all went to the same Catholic grade school, with the exception of my oldest brother, and all went to the same high school, which was quite a good high school called Amherst Central High School in Snyder, New York. It was, I would say, a very outstanding high school.

BROCK: Was that a Catholic high school?

FINNIGAN: No, it was a public school. In fact, my oldest brother went to the Catholic seminary. He went into a seminary—what they called the "Little Seminary"—which was maybe 4 miles from our house. The priest, who was our parish priest, wanted me to go there, but I didn't want to go there. He wanted to know why. I said, "Well, I'm going to go to the [United States] Naval Academy [USNA]." This was like in the sixth or seventh grade. He didn't think much of the idea, and tried to force my dad to make me go there, but my dad told him that he wouldn't do that. It was my decision, and he didn't think the priest should be interfering with it.

My oldest brother, Jim, actually wound up in a public high school (Amherst). He went into the [United States] Air Force, became an Air Force pilot, spent his career in the Air Force, after the war, stayed on to get in his twenty years, and retired as a major in the Air Force. He had many, many hours of flight time logged. I'd say he had a good career as a flying safety officer during the last part of his career in the Air Force. This was around 1943, I think, that he enlisted, so the war was on. We were all facing the prospect that we were probably going to either get drafted or have to find something else. My second brother, Jerry, who was closest to me, for many years went in the Navy's V-5 and V-12 programs. They were college programs that qualified him to become a naval officer. It took probably three years, on the average, to get through. These programs were also known as the Holloway [Aviation Midshipmen] Program, which Rear Admiral [James L.] Holloway [Jr.] was the superintendent of when I was there and had set up. That was one of the really good ideas that somebody had during World War II. A lot of people got to go to college that might not have—like some of us. Jerry went into mechanical engineering. He wound up graduating from Cal-Berkeley [University of California at Berkeley, UC-Berkeley].

BROCK: Did he go on the G. I. Bill [of Rights] then afterwards?

FINNIGAN: Well, he stayed in as an ensign and then a JG [Lieutenant Junior Grade], I think. He stayed in three years after he finished. By the time he finished school, the war was coming to a close. I think, it was during 1945 or 1946. He spent the required three years on a ship, then he came out, and went back to graduate school—also at UC-Berkeley—and wound up being the person who influenced most of the family to come to this area (northern California), because he went to work for a company called Owens-Corning Fiberglass; it was a new company at that time.

BROCK: Yes.

FINNIGAN: He was one of the original people selected by Owens-Corning—they brought in a bunch of young people whom they trained for maybe nine months in Newark, Ohio. Then sent them to various locations, but the plant he was assigned to was here in Santa Clara.

BROCK: Wow.

FINNIGAN: Which was why he came out to California. He came out to work there and still lives a few miles from there. He influenced most of the rest of us into first seeing California, then northern California, and then ultimately coming here.

BROCK: Including your father?

FINNIGAN: Yes. My father, when he retired, came out here, and lived in San Jose until he died. The brother just younger than me, Denny, also came out here. He went in the service after I did, and spent four years in the Air Force, where he worked on computers, which were IBM [International Business Machines Corporation] punch cards at that time.

BROCK: Right.

FINNIGAN: He ran a big computer center in Honolulu at Hickman Air Force Base [AFB]. Wound up working for a company doing that, a company called Sunsweet [Company], while he was going to San Jose State [University] on the G. I. Bill. Then he was asked to go to Stanford [University] to take over—basically run their computer facility, which was quite small, comparatively, at that time. He did that while he worked on his bachelor's degree in business at Stanford. So he graduated from Stanford and spent a large part of his career at SRI [Stanford Research Institute]. He became a general manager of SRI, vice president, and general manager—the operations manager—for some four years.

BROCK: So what was he in charge of in that role then, overall administration?

FINNIGAN: Yes. He actually started out doing operations research and data processing. He worked in a pioneering data processing group. That was sort of SRI's expertise in the early days, and they were doing a large project for Bank of America [Corporation]. It was to computerize, you know, the whole process of recording checks, reading them, etc. It was called Project ERMA [Electronic Recording Machine Accounting]. SRI had several divisions working on it, including the part of SRI that I ultimately worked for. They built a system that could process all the checks and, ultimately, computerize the Bank of America's whole system. Denny worked on that project for several years. I think, he was more on the management information systems end of it. He later became head of MIS [Management Information Systems] in the managing systems division of SRI. Ultimately, he became a general manager of the whole Institute.

BROCK: The whole operation? Wow.

FINNIGAN: Yes. He and another fellow, Dr. Don [Donald] Schueck, who was an engineer, were co-managers, and, I think, for four years they did that job. Denny wound up doing that same task of computerizing and installing management information and data systems for various other companies, including SAS [Scandinavian Airlines Systems], and, ultimately, the whole "empire" of Marcus Wallenberg, who was the wealthiest person in Sweden, and one of richest people in the world. He did similar tasks for Axel Johnson's companies, which were leading industrial companies in Sweden.

BROCK: Right.

FINNIGAN: Marcus Wallenberg was the leader of those enterprises, followed later by Marcus [Wallenberg] Jr. and other family members. Denny worked for them. He had a group from SRI, around thirty-five people, that worked in Sweden full-time. He commuted back and forth, and did very, very well for SRI. He left after twenty-eight years. He continued to do that job after leaving SRI until a couple of years ago. He basically commuted to Sweden once a month for some twenty-five years. There were many parts of these Wallenberg and Johnson empires, and they were basically going to SRI for the latest technology in the computer area, management systems area, et cetera. He ultimately became very much a top-level management consultant on his own. He could bring people in that were even more specialized than he was.

BROCK: And this was systems—just to coordinate this diversified sort of financial—

FINNIGAN: Yes. SRI did a lot of work that wound up as the first worldwide computer-based reservation system for SAS. It was the basis for later reservation systems such as American Airlines' Saber System. How much of that they're credited with, I don't know.

BROCK: Yes.

FINNIGAN: So a lot of his work started out with that system and then grew and expanded into other areas of Wallenberg's businesses (and also Axel Johnson's business).

BROCK: I see.

FINNIGAN: Wallenberg owned half of SAS—he probably owned half of Sweden. [laughter] Anyway, that's Denny. He's living in Oregon now; he moved up there about five years ago. Kathleen [M.] Finnigan, my only sister, was trained to teach at Buffalo [New York] State Teachers College. She has worked in various jobs. We'll just say that she was not technical in the things that she did. She wound up working for companies such as Macy's. I think she worked some fifteen or twenty years in Sacramento. She's currently retired over there, and she lived in this area for a while. I think she likes it better over there; it's a quieter place. My brother John [P. Finnigan] was next in line, second youngest. He moved to this area to go to San Jose State, was also influenced by my brother Jerry and then, at that time, Denny. He wound up working at Lockheed [Martin Corporation] in their management information systems group—I think he became pretty good at building data management systems for hospitals. And there's a local hospital called El Camino Hospital that worked with Lockheed's computer systems group—and started automating all the hospital functions.

BROCK: Wow.

FINNIGAN: They started very early doing that. Lockheed, I think, had a pretty good business going there, and spun it off to another company—I'm not sure, but I think Technicon [Data Systems, TDS] bought it. I can't tell you where it is right now. But John spent most of his career doing that at Lockheed. He retired from there a couple of years back.

My youngest brother, Paul [F. Finnigan], went through the same schools as did all the others in Snyder, New York. He came out here to go to San Jose State as well. He wound up following more in Denny's footsteps in terms of his interests and specialty in school. He worked at Stanford Research Institute for around four years in the finance department. He left there to found several companies in this area. The principal one was called Voicemail International [VMI]. They were basically doing voicemail answering services, such as you use today. It was very early that he started in this, and there were a lot of companies that came along later and really sort of took over that business.

BROCK: Automated voicemail sort of business systems?

FINNIGAN: Yes, voicemail services for companies. There's still a voicemail company called Voicemail. They still, I think, provide answering services, but it's splintered. I think, ultimately, an Italian company bought the company that he had. But there are still parts of it in Portland—one of the members of our family runs that one. It's still running as an independent company now. Paul still has a consulting company in the area of voicemail, and is still pretty active in this. He really had a lot of original ideas. I would just say, if he had gotten good business help at the time, I think his company would have been the company that really made it big. But he didn't try to, for whatever reasons, and didn't succeed in making it a strong-going

company. So they were more an idea company, and then other people came along and took those ideas and ran with them.

BROCK: Right. Proved that such a business was possible.

FINNIGAN: He still has a lot of ideas and is very creative in that area.

BROCK: Well, that strikes me as a pretty remarkable similarity amongst all of your brothers: coming out to the Bay area, and getting involved with pretty forward-looking technological areas. And also—maybe with the exception of your brother, who was the career Air Force officer, although there's a lot of technology in the Air Force and the technology in this period was huge—all getting involved with commercial contexts of technology. How would you explain that with reference to growing up together in Snyder and the schools you went to? Or was it more being here in this location at that time?

FINNIGAN: Well, I think it has a lot to do with being in this area.

BROCK: Right.

FINNIGAN: Where there's a lot of entrepreneurism. Perhaps having other members of the family who were in various technical endeavors. I would say we all had the same basic training at parochial school through eighth grade and then Amherst Central High School, which was a pretty broad-based background. If you wanted to go into engineering or if you wanted to go into whatever business, it probably prepared you with the right fundamentals. I think that I probably was the most technical, in terms of interests, of all of the brothers in going on in engineering, both in undergraduate and etc. But Denny's business background worked well in a very highly technical organization at SRI and ultimately he co-headed all of the technical aspects of it. So he grasped all the aspects of the Institute's business, both management and technical. But I don't know the answer as to the reason for gravitating in that direction. This area has a lot of effect in that regard, that's for sure.

BROCK: Models of that sort of career in the area. And then even amongst the group, the brothers were reaping or showing the possibilities.

FINNIGAN: Yes.

BROCK: Well, maybe we could go back for a minute to Snyder. We talked a little bit about the field where your siblings and yourself had free rein and the sports. Please talk a little bit more about Snyder as a community—what are your thoughts on growing up there in the thirties?

FINNIGAN: There was a lot of snow. [laughter] When you're in Buffalo, you get used to that. I would just say we grew up, more or less, with our noses to the grindstone in school, where the requirements were really rigorous. I would also say that at home, as well, we had lots of responsibility because of the fact that we didn't have a mother there. We were really the ones doing, by necessity, a lot of the family work, even preparing meals, housework, and stuff. So there wasn't a lot of time to spend even on sports, you know, to go out for a high school team or whatever. It was a nice community, a very nice suburb. It was a fairly well to do suburb where most of the people were from good families and were intending on going to college. It was sort of an all-American place—it just had a good hard core American community aspect to it. The people there were like how you often think of some of the Midwestern people in the United States as being the salt of the earth. And I'd say that was true of Snyder, that people were just good Americans, you know, patriotic.

The era we were in, with World War II coming on and then happening during our time, made people pretty serious, ourselves included, about wanting to become part of the military or part of the war effort. I think that had a big bearing on our childhood. I was in high school as we entered World War II; and similarly for several of my brothers, three or four of them.

[END OF TAPE, SIDE 1]

BROCK: We have talked a little bit about the place of science and technology in your home, but I was wondering if you could talk just as we were talking about the seriousness of the culture at the time. How would you characterize growing up in reference to your first decade of life—the place of science and technology in that culture? Was it big? Did you have an image of the great engineer or the scientist?

FINNIGAN: Well, one thing that I think most of us did in our family was read a lot. My father provided a lot of books. I read voraciously, and I think some of my brothers did as well. So I would say we really had our interests stimulated by reading. Some of the books were classics. We always got classic books for Christmas, and so on. I think they provided a curiosity that was stimulated at the right period in life. A lot of our spare time was spent reading good books. Some of them dealt with science and technology.

BROCK: Classic literature? Or were you reading non-fiction too?

FINNIGAN: Some fiction, some non-fiction—a lot of variety. At that time, you know, those pocket books started coming out. We read a good many of those. But my father usually bought us classics, including those by Mark Twain, [Charles] Dickens, Jonathan Swift's *Gulliver's Travels*, and many others (1). We all read *The Hardy Boys* (2). Those were pretty light reading. But there was a series of books by an author—I can't recall his name—it was about Dave Darrin's first year at Annapolis [United States Naval Academy] and second year, et cetera (3). There's a group of guys who went to [United States Military Academy at] West Point, Annapolis, or the [United States] Coast Guard Academy. These were fictional, but they were quite good reading. And they were sort of typical of a lot of the things that we had access to, and I don't even know where they came from. Our grandmother or somebody had read them before—let's say before we were born. We were exposed to some technology, but there wasn't a lot of it. When the War started, we lived right near the Curtiss-Wright aircraft factory—maybe two miles from it, which was at the Buffalo Airport. We became interested in airplanes. And we built model airplanes—a lot of them, I mean dozens of them. That was one of our favorite hobbies. Then we'd fly them and, you know, some of them would fly and some of them wouldn't.

BROCK: Were they motorized or gliders?

FINNIGAN: Rubber band driven. At that time it was about as much as we could afford.

BROCK: Right. Sure.

FINNIGAN: But we built a lot of those and became very interested in aircraft. Because we were building models of almost any one you want to name, whether it was a P-40 or, Cessna. If it was not a Cessna, it was Piper Cub. That was maybe as close we got to technology. But we were interested in the airplanes that were being built for the war. I think at one point—I worked for Larry [Lawrence D.] Bell, who was the founder of Bell Aircraft [Corporation], which made helicopters at that time near Buffalo.

BROCK: Right.

FINNIGAN: And became the manufacturer of other warplanes nearby.

BROCK: You worked for him?

FINNIGAN: Well, yard work.

BROCK: All right.

FINNIGAN: A lot of the people were involved in companies in our community, and so we really had the chance to be exposed.

BROCK: From your description it sounds like the parochial grade school through eighth grade was a pretty serious academic workload. But I was wondering if you had any particular experiences or mentors from that period of time before you got to the public high school?

FINNIGAN: We had nuns for teachers. I don't think I necessarily considered any of them mentors. But we had good teachers. I'd have to say they were very good. I was always motivated to do well in school. Similarly, I think most of my brothers and my sister were. But I wouldn't name anybody as a great mentor.

BROCK: All of you were performing very well, got good grades, and all that?

FINNIGAN: Yes. I always wound up being first in my class all through grade school. I think most of my brothers and sister did well. We were expected to. It often boiled down to how interested were you in the subjects and in doing well. For me, I just found that I could always do best in the classes that I was in.

BROCK: Did that continue for you when you went to high school? Did you have the same experience?

FINNIGAN: Yes. I finished high school in three years. I did that, because I thought I could go into the Naval Academy a year earlier. It turned out that I was too young by a few days, so I couldn't. Yes, it continued for me. I was an honor student in high school, and I did have a mentor there—several of them. One was a teacher who was a counselor for the high school—we only had one counselor. There were probably twelve hundred students in our school, or something like that, but he took a real interest in me. His name was Mr. Ford. I was interested in the Naval Academy, but I didn't know exactly how you went about doing that.

BROCK: Right.

FINNIGAN: So he ordered their prospectus. Fortunately, I was taking the right subjects, which were science, math, and so on. He made sure that I had all of those. If you got an A or B, you were not required to take the full exam to get into Annapolis. I was a member of the National Honor Society then and found that I could do pretty well. I was a good student there. Several of my brothers were as well.

BROCK: Did you just find that you had a facility for certain things—things came easy for you, or was it a mixture between that and long hours—sort of doggedness?

FINNIGAN: Well, I would say things came relatively easily. It was a family of seven children, so there weren't very many quiet places to study. We wound up studying around the dining room table, as I remember. Since we all slept in one bedroom—all the boys—we didn't have any other place to study. So it was not too easy to find a lot of time alone. But I think it came relatively easy. I already had a strong interest and good ability in math and science, so I found that I could pick them up rather easily.

BROCK: Were you taking the normal sort of sequence in math and science—chemistry, physics, and the normal mathematics sequence?

FINNIGAN: Yes. I was actually accelerating a good bit of the time. In New York State, if your teacher let you take your Regents Exams, and you got 90 percent or better on the exam, you could drop that course and get credit for the full year at, say, the half-year. So I was taking a lot of those tests early and not having to take the full two years—like Latin or whatever. I did that in quite a few subjects. Some of them, like chemistry, I never even was in a class. The chemistry teacher tutored me when I was trying to finish up courses required for Annapolis. You know, I studied for maybe a couple of months with him as a tutor. Fortunately, I got a very high grade on the exam.

BROCK: Was that during school that he would tutor you because you'd already sort of finished the course?

FINNIGAN: Yes. Well, I think what happened was that I did some of it in the summer, you know. I would study in the summer—I took maybe two or three courses that I was doing almost on my own in the summer but with some tutoring.

BROCK: Right.

FINNIGAN: So I had enough at the end of three years to graduate. I wound up taking chemistry after that because I found that I needed it for the Naval Academy. I remember passing in less than a half a year, where it was normally a five-year course, and maybe four or five other courses as well.

BROCK: Wow. Did you work during high school? What were your extracurriculars?

FINNIGAN: I worked as a soda-fountain jerk. I did lawns and yard work for people—sometimes both of those. The last couple of years, I worked on a pretty regular basis in a dairy where I helped make ice cream, and out in the restaurant, you know, waiting on customers and a variety of things for the same company for two or three years. Later I worked part-time in a men's clothing store. It was a pleasant change, you know, from all the school.

BROCK: Maybe your interest in Annapolis goes back to that conversation with the priest at age eight, maybe partially inspired by the book that you mentioned that you read. Did you have any other experiences with sailing, the ocean, or were there any other draws to the Navy and Annapolis?

FINNIGAN: Not really. We were relatively restricted in, how far we could go in the summer. We lived near Lake Erie, but I don't think I was ever in a boat on Lake Erie before going to Annapolis. So it was mainly through reading. I'd say that was the stimulus for me. It's very rare, at least watching my kids, that, you know, you decide like in the sixth grade what you're going to do and you go ahead and do it. It was a goal I had from about the sixth grade on. I think it was from reading something fictional that sort of, sounded very nice. I also was stimulated by the idea of a career in the service.

BROCK: Right.

FINNIGAN: I just decided very early on. The parish priest—incidentally, from that point on called me "the admiral." [laughter] The closest to "admiral" that I've ever been is in the American Airlines Admiral Club.

BROCK: As we were talking about before, World War II really overlapped with your high school experience. And, again, we talked about the sort of aura of seriousness that was in not just your community and high school, but probably many places.

FINNIGAN: Right.

BROCK: I wanted to just ask what your experience of the War was like? Did your brother Jim fly during the War?

FINNIGAN: Yes, he did.

BROCK: So the experience of the War for your community, yourself, and your family—do you have some thoughts on that?

FINNIGAN: Well, it really was a background that was very similar to what we have right now with the terrorist thing, but even more so, because, you know, many, many people in our community were going off to fight. And some of them were killed. It was something you were constantly reminded of at school, at home, in the news, which was only radio at that time, and by our father. He said pretty early on that he thought all of us older ones, including me, would probably wind up serving in the military during the war. So it points somewhat to an urgency in regards to our education, our learning, and our doing well. It also, I think, provided opportunities that maybe weren't there before, such as the Navy ROTC [Reserve Officer Training Corps] program or V-12ers, I'd call it, that became ROTC. We saw opportunities for going to college that we might not have had otherwise. I would say the War provided incentives to get into those programs and to do well in school, so that you'd be qualified, because they were very selective. Then the other aspect, wanting to join the military to be, you know, in a patriotic sense, you wanted to be part of it. I would just say everybody that was decent, and that would include most people, saw this as something that they should contribute to. So it was a background that was very much present all the time. For me, personally, that created an incentive, to do things sooner than I might have otherwise, and it gave me goals that might not otherwise have been there, or have been as obvious.

BROCK: So you completed high school in three years. Was it in 1944 that you finished up?

FINNIGAN: Right.

BROCK: The counselor got you the prospectus and the application materials for Annapolis.

FINNIGAN: Right.

BROCK: You found out that you missed the age cut-off by four days. What happened then with the application procedure or the nomination procedure?

FINNIGAN: Right. You had to be nominated by a congressman or senator. I basically found that out when I was applying to take the civil service exams. I found out pretty much towards the end of my last year in high school—my third year—that I really wasn't old enough. When we got all the prospectuses, it became obvious, you know, that I was going to be too young. I, at that point, went to a preparatory school in Annapolis, Maryland, for approximately two months to prepare for the civil service exam, which you had to take in order to get an appointment. You might get an appointment from your congressman, senator, or both if you did well on the exam.

BROCK: Right.

FINNIGAN: I took the course in a prep school at Annapolis, and it was quite a good one. I decided that I would fill in that time between then and the admission date of the next class, assuming I could get an appointment, working and filling in the blanks like chemistry that I didn't have.

So it turned out that I got three or four appointments. Included was a principal appointment from Senator [James M.] Mead of New York State, which was the one that I accepted. I had others as well. One of them was a statewide congresswoman-at-large and the local congressman. But anyway, I then started filling in the blanks in my background.

BROCK: This was after prep school?

FINNIGAN: Yes. It was a cram course, sort of, you know, where they had all the previous civil service exams. It was a smart thing to do. I met another fellow from our grade school and high school who had gone to Annapolis and had made it in. He wasn't what I considered real bright, but I found out how he had gone to the "prep" school. So I talked to his father and found out the details and went there as well. I subsequently went to another preparatory school for a couple months in Washington, DC, in this interim period. It prepared you for the entrance exams to Annapolis and West Point. It was a very, very good school. It was similarly somewhat of a cram course. I only had to take exams in English and math.

BROCK: Because you had taken the Regents?

FINNIGAN: Yes. If you qualified with a B or better average in high school that was all they asked for. If you had a year of college you didn't have to take any exams. You could go in on that. But I didn't have time to get a year of college. So, part of the time was spent preparing for the exam. I worked and filled in the courses and stuff that I didn't have. I took the Naval Academy entrance exam in April 1945 and I did well on that. That was the last sort of obstacle. I entered Annapolis in June 1945, which was just before my original class in high school graduated.

BROCK: June 1945?

FINNIGAN: Yes.

BROCK: Was that pretty much the first time that you were living away from home? You were working, going to school, and pretty far from home. Well, relatively far.

FINNIGAN: Yes. It was a good experience. Most of the people preparing for the Naval Academy exam went for eight months, whereas I spent only two, which was all I could afford. But it was the right two months. They were doing the cram course then. It turned out to be a long enough time to learn what I had to learn to sort of "hit" the exam. It's hard to believe some of these things now. In the case of Senator Mead, where I was his single appointee that year—I think he had a thousand two hundred applicants for that appointment. A lot of people were doing this as a preference to going in the Army or whatever. At that time, you'd be drafted, if you didn't go into the Navy V-5, V-12, or one of the academies. So to get an appointment under those circumstances was pretty incredible. Anyway, I was really happy to be able to qualify for that appointment and pass all the exams.

BROCK: You began your course of training and study at Annapolis. I have a number of questions about Annapolis. Were there decisions early on about a track or course of study to get into? That is to say, was the first year or two just core curriculum or core studies, or did you track right away to the engineering side?

FINNIGAN: In fact, the Naval Academy, when I went there, had only one curriculum.

BROCK: All right

FINNIGAN: The variable was the language. You could take different languages. They had everything from Russian to Portuguese and others. It isn't that way now. You can select your

major now. But I would suspect that it probably isn't all that different. So we wound up the first couple years taking really liberal arts courses and sort of getting a variety of courses that were introducing you to the discipline of the U.S. Navy, you know, leadership courses, et cetera. I think it was a very good broad education—liberal arts, engineering, et cetera. There were some really incredible aspects. Some of the best education you could get was attending lectures on Monday nights called the Mem [Memorial] Hall Series. They had this large auditorium in Memorial Hall. They'd have guest speakers such as Admiral [Chester A.] Nimitz, [William F.] Halsey, [Raymond A.] Spruance, [Douglas J.] MacArthur—and many others. They'd come and talk for an hour or more and answer questions for as long as we midshipmen would stay.

We have a similar celebrity series here in Los Altos each year. You probably have never heard of it, but the last three speakers included Madeline [K.] Albright, Richard Holbrook, and a fellow named Wynn—Steven Wynn, who is the founder of the Mirage Hotel in Las Vegas, Treasure Island, and a couple others. It brings back memories of the great celebrity series at Annapolis. Those people were the heroes of World War II who shared their wartime experience with us. There was usually somebody at least every month or so. To hear those experiences from those who made history was simply incredible. But my interest was strong in engineering and mathematics. I always did best in those two.

The first couple years were, I would say, a very good liberal arts sort of education. We had in my class, I think, seventy-five people out of nine hundred or so who already had bachelor's degrees through other Navy programs, such as V-5, V-12, or et cetera. Fifty percent of the class had at least a year of college, and I think we had a large number of valedictorians from high schools. It was a pretty intelligent group of people. So you came here from Snyder, New York, where you thought you did pretty well and boy, you were just one of many. There were some very, very bright people.

BROCK: That was an eye-opener for you, I'm sure. But did you continue to excel relative to your peers?

FINNIGAN: In engineering and math, I did. I didn't study very much at the Naval Academy a good part of the time. There were a lot of things that interested me besides the courses we were taking.

[END OF TAPE, SIDE 2]

FINNIGAN: I was reading the alumni magazine called *Shipmate* yesterday. It had a picture of a guy who was two classes ahead of me there. He used to beat my rear end when I was a "plebe" (first year) regularly. [laughter] Every night, practically, we went around to somebody's room and did forty-nine pushups, forty-nine deep knee bends, or whatever (our class number was 1949). This guy took great pleasure—when our shower wasn't clean or

whatever—in just beating our rear-ends with our clacks—those were the wooden shoes that we would use in the shower. I just wanted to punch him in the nose. To this day I would like to meet him in an alley somewhere. We were really being hazed the first year.

It was really a tough go. I mean, the class started out at twelve hundred twenty or so and it wound up with seven hundred ninety graduating. We lost a lot of people. There was a sort of programmed attrition rate. Then there were people that resigned because they couldn't put up with all the crap that went on, especially in the first year. We were hazed. It was not a fun thing in that regard. It took a lot of stick-to-it-iveness. I was glad that my dad had raised us in a highly disciplined setting, because I don't think I would have put up with it. It was hard enough as it was.

So studies were not my major interest. It was just surviving, you know. I was relatively small; I weighed only 120 pounds at the time I entered Annapolis. I had to eat bananas to get up to that. That was the minimum weight. I was pretty thin and fragile, I guess you'd say. They were, I think, trying to change that. I became interested in sports there. So I had a lot of new interests, where studying wasn't necessarily at the top of my list. But I did okay.

BROCK: Right. Sports. What did they call the incoming class?

FINNIGAN: Plebes.

BROCK: Plebes, right. Was there sailing?

FINNIGAN: Right. We were taught to sail—everybody. You had to learn how to handle a sailboat on your own. There were a lot of things like that that we were introduced to that were really fun and a real good divergence from, you know, hitting the books. I think for me something that was really eye opening and a great thing to find, was the golf course. The last two years, I think I played golf almost every day. There was a bus that would pick up the golf team. I wasn't on the varsity golf team, but I was on the battalion golf team. They'd take you over to the golf course and then would bring you back—it was across the Severn River—before dark. I did that every chance I could get. I was also playing other sports like handball and squash, which were just great fun. I did reasonably well at some of those sports, and so it provided a real sort of release.

BROCK: Yes. So the curve from being the hazed to the hazer sort of shifted over the four years.

FINNIGAN: Right.

BROCK: What was the place of science, technology, and engineering at Annapolis or the vision of the Navy that you were being instructed in? Then as you were coming to the end of your career at Annapolis, what were your thoughts about what you would do next?

FINNIGAN: In answer to the first part of your question, there was a high emphasis placed by USNA on science, technology, and engineering, in particular as related to naval applications, such as navigation, gunnery, naval propulsion, and naval aviation. As to the second part, I really had a pretty clear vision of what I wanted to do. I was finding that I stood almost at the top of the class in electronics and electrical engineering. I always did well in math and found that those were areas that I really enjoyed. I felt that I wanted to go to graduate school after I got out, because I felt that I had a technical professional career ahead of me. For me, at any rate, it at least equaled the idea of a military naval career and in some ways it really took first place. If the two could be combined that was all the better.

So I wound up selecting the Air Force. When we were graduating there was no Air Force Academy. The Air Force was a separate service by then. They talked the Navy, West Point, and Annapolis into allowing them to take a part of their classes. We had fifty-five of us from Annapolis and I think maybe double that from West Point that went into the Air Force. So I applied for that service and was one of those selected.

BROCK: Now did you make that choice for the Air Force because you saw a greater potential there to combine electrical engineering and a military career?

FINNIGAN: Yes. The Air Force worried that they couldn't attract people. They sent a lieutenant general over to describe what programs they had in mind for those that selected the Air Force. So for the first three years they would sort of manage, or help you to manage your career and allow you to go back to graduate school, if you qualified. Then they described many of the specific opportunities. It was a young service, so they really needed technical people. There was no guarantee from the Navy that, following graduation, you would go to the Naval Postgraduate School, although it was possible. The Air Force had a civilian post-graduate program as well as the Air Force Institute of Technology [AFIT]. The latter was a small part of the Air Force program. So they described—I would say pretty accurately and honestly—what they had in mind. And it interested me and this was a strong reason for picking the Air Force. For me it turned out to be a good decision. In the Navy you could almost tell where you were going to go, you know. It was pretty programmed, and it was very much a function of where you stood in your class. So I felt that I was programmed to become a fleet sort of person. I, frankly, wasn't that turned on by the prospect of that. It's always hard to say what is going to be best, but I think for me it turned out to be the right thing for sure.

BROCK: So you avoided having to go on that cruise, right, as a lot of your fellow graduates had to go on?

FINNIGAN: I might add that the best times I ever had at Annapolis were on cruises.

BROCK: Really?

FINNIGAN: Yes. I was on the *Mighty Mo* [*USS Missouri*], another battleship, [*USS*] *North Carolina,* and a carrier. I had a fabulous time. I really loved that part. We spent a whole summer on each of those ships. Each summer we were on a different ship basically for the entire summer. It was exciting and really challenging.

BROCK: So that was more of a sacrifice for going to the Air Force?

FINNIGAN: Yes. I was giving up that prospect in the Air Force setting.

BROCK: Did your initial assignment with the Air Force begin right away or did you have a break?

FINNIGAN: I had a month off. Then I went to Tyndall Air Force Base in Panama City, Florida to the Air Tactical School (ATS). It was a company officer's school. But they spent about a month prior to ATS with the graduates of West Point and Annapolis. We were placed together. I think there were nineteen of us from Annapolis and somewhat more than fifty from West Point. We were, first of all, flown around the U.S. in Air Force planes and taken to different USAF bases to be shown what the different commands did. This was very informative. Then they put us through a series of tests that were designed to determine our interests, our abilities, and our aptitude for specific fields, and other tests, which were psychological measurements. It was a very meaningful endeavor, which somebody had really thought through. At the end, their hope was that your interests and your aptitude would lie in the same areas. For me it was like electrical engineering was way up here and, you know, it was a pretty big drop down to the next choice, both in terms of interest and aptitude. I guess that I really was one-dimensional in that sense.

So, in this company officer's school (ATS) there was a lot of learning about the Air Force as well. But I found I wasn't all that interested after four years of the Naval Academy. I played golf every day that I was at that school, and I didn't study very often. But I did find more or less what I wanted to do. However, I wound up going into flying school from there, so I didn't immediately go into graduate school.

BROCK: So you went to flight school. Was it still as an Air Force officer that you went to the University of Illinois [at Urbana, UI]? Was that in 1952?

FINNIGAN: Right. That was in 1952. In all of 1949 I was in Florida at the Air Tactical School. I went from there into flying school, which took about a year. So all of 1950 and part of 1951, I was in flying school. It was sort of late 1951 that I came out of flying school and was assigned to Strategic Air Command [SAC] at March Air Force Base in California. It was during that period in SAC (nine months) that I decided I really wanted to go back to graduate school. They didn't exactly agree with me within SAC. Really, SAC didn't allow Air Force officers, who were regular officers, to go to graduate school. I was the first one that received that assignment. It was because I was part of the initial USNA group to go into the Air Force. There was a major in the Pentagon who controlled our assignments. I talked to him and he arranged for me to go to graduate school. I applied and was selected for the University of Illinois, and there was no over-riding control from SAC headquarters.

BROCK: So the major interceded on your behalf to let you be detailed or assigned to go study electrical engineering at Illinois. I want to know what was your job at SAC, and then why do you think this major, you know, interceded on your behalf to let you do that?

FINNIGAN: Well, I was the intelligence officer for a bomb wing at the time. I was temporarily off flying status because of allergy problems, so I wasn't on a flight crew at that time. I realized that I wanted to get back into graduate school; that it was something really I wanted to do. The major interceded for me because SAC wouldn't let me go. There had never been a single regular officer, according to what I found out later, that had been allowed to go back to graduate school. General Curtis [E.] LeMay was the head of SAC at that time and he didn't believe in graduate school. He had top pick of all the officers, and SAC obviously had an important mission at that time. The major in the Pentagon was the one directing the careers of the first Annapolis and West Point graduates, specifically those that had been through this special testing program.

BROCK: Right.

FINNIGAN: For three years they gave him control over our assignments. So I got in touch with him by phone and I told him my circumstances. I had applied and been accepted to Illinois. He said, "Well, I'll arrange for you to go back there." And he did. I got orders from the Pentagon that overrode SAC and my local commanders. I was fortunate in getting to go back, because my three year control period was expiring just about that time.

BROCK: Do you think that he thought it would be good for the career track he was envisioning for you in the Air Force, and he was worried that if they didn't do it for you you'd just leave whenever your term was over?

FINNIGAN: I never threatened anybody. If I had it wouldn't have done any good. [laughter] I probably would have left. I think he probably knew that and felt I could contribute in technical assignments in the USAF following graduate school.

BROCK: Right.

FINNIGAN: I had really done well in the qualifying exams and my aptitude was high. I think that the Air Force sent officers only to Illinois, Stanford, and MIT [Massachusetts Institute of Technology] for EE [electrical engineering]. They were the top three schools in engineering. So they needed people who could qualify for these schools. He sensed that I really wanted to go to graduate school, that I did qualify, and that I should do pretty well at it. So he gave me a hell of a boost at a time that I really needed one.

BROCK: You went out to Urbana in 1952? Were you still in active service?

FINNIGAN: Right. I was a "student officer," as they called us. So they paid our regular salary, which wasn't very much—but probably better than a lot of civilian graduate students that were around us. I was there for a two-year tour of duty, because it took two years to get your master's degree. I found out right away that anything I had had before at USNA was meaningless in terms of EE. I mean, I was expecting to go right into graduate school. We had one professor advisor for all of the military students. There were probably thirty total in EE. The advisor suggested I take some undergraduate courses. I told him that I had already had undergraduate EE. He said, "Oh, well then let me give you a little exam and we'll see how much you remember." I couldn't remember anything; there was nothing there. I couldn't even solve the simple problem he gave me, which was to draw an equivalent circuit and figure out the voltages, currents, et cetera. So I was there in a small group with three West Pointers and two other Air Force officers. The West Pointers were in the Army Signal Corps. So we all started in a special course where we took undergraduate courses for one whole year and did in essence the whole undergraduate EE curriculum in one year.

BROCK: Wow!

FINNIGAN: It was the right thing really. It was a very competitive group of six people. And the EE department kept us by ourselves because we were sort of special. They were accelerating most of the background courses.

BROCK: Am I right that 1950 was when you got married?

FINNIGAN: Right. I met my wife in Florida when I was at Tyndall Air Force Base. We met and were married six or seven months later. She was with me, obviously, at March AFB in California and when I went back to Illinois. We had two children by that time. We had just had the second son before we left California to go to Illinois.

BROCK: A busy time.

FINNIGAN: Yes, it was.

BROCK: Your wife's name is Bette [E. Finnigan]? And was she originally from Florida?

FINNIGAN: Yes. She's from Panama City. She went to Florida State University and the University of Florida before I met her. She was living in Panama City with her mother at the time I met her.

BROCK: How did you meet? It seems like a whirlwind sort of romance leading up to marriage.

FINNIGAN: Well, surprisingly, it was at a drive-in restaurant. I was coming home from playing golf with a friend, and he knew her girlfriend. They were in the car next to us and we met there. We wanted to listen to the Navy-Tulane [University] game, if you want to know the details. I didn't have a radio in my car, and we thought they did. So we moved over to their car, and that was when I first saw her. It was a very lucky occasion for me. [laughter] The <u>most</u> important event of my entire life.

BROCK: So the four of you were there in 1952. You were taking that accelerated course work. Did you have a research project for your master's at that time?

FINNIGAN: Yes. I did a research project on something called Tchebischeff functions, which you've never heard of, I'm sure. But these were functions that you run across in antenna theory, which was an area that I was quite interested in. The ILLIAC I [Illinois Digital Computer] was at the University of Illinois at that time. Maybe it was two-years old at the time that I did my thesis—or maybe just one-year old. I had taken computer courses and learned how to use it. So I did a thesis project where you needed a digital computer. It was the fastest computer in the world at that time. I was basically solving an antenna problem using complex mathematical functions. And that was my master's thesis.

BROCK: Did you have to compete for time on the machine pretty heavily?

FINNIGAN: Yes, you did. You also had to know how to use it. In the course I took in digital computing, they taught you. You know, we had software that our professor, who was from England, had put together that made it possible for somebody to use it in a fairly short period of time. It was software that was user friendly and allowed you to do pretty complicated problems using the ILLIAC. You basically had to go in there with maybe a punch-paper tape and your problem. And then later you got a time when they'd let you run it, usually late at night. So it was very competitive.

BROCK: Right. Was the computer in the control of the electrical engineering department?

FINNIGAN: No. It was in control of what I think they called it the computer-systems labs. It was available to anybody at the University. It ultimately became part of what they now call the electrical and computer engineering [ECE] department—the department I was working in. So probably they have more control now than they did at that time. But they had a very famous guy—Dr. Jack Nash—who ran that operation. It was extremely well run. He later became one of the top officers at Lockheed Research out here in Palo Alto. But you had to compete and you had to show that you had a problem worthy of it. [laughter] So you weren't in there just using time. But the problem that I solved couldn't have been done without a digital computer.

BROCK: Because of so many simultaneous equations?

FINNIGAN: They were complex differential equations that required a fair amount of computing power. Today, you could easily do it, you know, on a PC [personal computer]. But at that time, the CSL [computer science laboratory] took up a whole building, and they used CRTs [cathode ray tube] as the storage devices. They had vacuum tubes too. They used Teletype for the input. It was pretty slow. Hard to believe.

BROCK: That sort of antenna problem was obviously going to be—it was a research project that would be of interest to the Air Force as well.

FINNIGAN: Right.

BROCK: As you were getting your master's and that two-year tour was coming to a close, how did that work out with your thinking of staying on for a Ph.D., your career, and the Air Force?

FINNIGAN: Well, I wound up taking a lot of extra courses while I was there. I was trying to qualify to take the oral exam for the Ph.D. before leaving. Then I figured at my next base, I would do the thesis. I wound up, in fact, taking and passing French, German, and a lot of extra courses. I was under the control of the commanding general of the Air University Command and the Air Force Institute of Technology, actually. I'd written to them and asked them if I could stay on for another semester to finish all this and I'd been turned down by them. But towards the end of my time on the two-year tour, the general, who was a major general, called me and asked if he could come visit me. General Ralph P. Swofford Jr. was his name. He had just established what was called "General Swofford's Special Ph.D. Program." I might have even applied for it, but they already had accepted somebody else—a classmate of mine who stood first or second in my class at Annapolis (academically).

[END OF TAPE, SIDE 3]

FINNIGAN: This fellow was very bright. He went out to Wright-Patterson AFB as part of that program. He wouldn't come back to take the oral exams for the Ph.D. He just froze at the thought of it. He was a good friend, so he told me this. So General Swofford came over to the University of Illinois maybe a month before I was due to leave Illinois—I already had orders to go to Rome, New York, to an Air Force base there in an electronics engineering position. He asked me if I would like to join his Ph.D. program. He said I could stay on at the University of Illinois for a year (up to a year) and then come teach at the Air Force Institute of Technology, which wasn't too far, while writing my Ph.D. thesis. There's about 200 miles between the two places. Initially, I told him to shove it. You know, I'd spent all that time taking these courses, studying two languages, and this and that. But he said, "Well, just lighten up. Think about it a while, and give me a call if you want to go ahead." And I said, "I already have orders to go to Rome AFB." He said, "We can change that." So ultimately, when I came back to my good senses, I accepted that, and it was a good choice. I stayed on for another nine months at the University of Illinois. Then there was sort of a logical break when I finished my oral exams and started a thesis to move over to the Air Force Institute of Technology at Dayton, Ohio. I had almost a perfect average at Illinois and, I think, that was one of the reasons that he wound up asking me. There were about five officers that were in this graduate program that were selected for it. One other from Illinois, then another from Stanford, and MIT.

BROCK: In electrical engineering?

FINNIGAN: No. They were all in engineering, but just three of us in electrical engineering.

BROCK: So at the end of 1954, you go to the Air Force Institute. And that was right after your oral examination?

FINNIGAN: Yes. It was actually in June 1953. I'd say about four months or something like that after the oral exams. I was already starting on my thesis at that time. At the Air Force Institute, we were assigned under General Swofford, as well as the EE department head. We met with him every Monday to report our progress. He was a really good guy. He had been the Air Force project officer/engineer for the first jet fighter the Air Force developed. I think it was the F-80. He had quite a good background in engineering, and was a very competent person. He spent just enough time with us to make sure that we knew he was watching, and then he always wanted to know, "What did you accomplish thus far?" He knew pretty well. To get me in the program when I had initially turned him down, he said to me, "Well, let me make you an offer. I can't guarantee this, and you have to take it on my word. But if you finish—I haven't had a single officer finish this program yet—I'll do my best to get you any assignment in the Air Force you want. I don't know if I'll be able to do that, but, you know, if I can I will." He was a major general at the time. So as far as I was concerned, it sounded like it would be better than, you know, doing it on your own. [laughter] But it turned out to be, for me, a very fortunate circumstance.

BROCK: Two questions. One, did you have an advisor that you were primarily working with at Illinois? And then I wanted to know about what your research—what was your dissertation?

FINNIGAN: I had done all of my work in antenna theory until the thesis. I would say my mentor there was a professor named Ed [Edward C.] Jordan. He became head of the department of electrical engineering. He was one of the most outstanding antenna persons in the world. I think most people would agree. He has written several books on antenna theory and practice. I used to play handball with him—he always won. But he was truly a marvelous individual. I always considered him my mentor. In the summer, I worked in his antenna lab and did projects that were of interest to the Air Force—such as an antenna that's now used in every airplane. I was not the primary person. I was working on the project but had made some progress.

Then when it came to my thesis, another professor approached me, Dr. Gilbert Fett, who was a servomechanism control systems person. He said, "If you want to get your degree and graduate with a Ph.D. you'd better come to work for me." He pointed out that Dr. Jordan was

just too busy and that he couldn't afford the amount of time that Dr. Fett himself said he was willing to give. So I did my Ph.D. thesis in servomechanism theory, actually, non-linear theory of servomechanisms (4). Dr. Fett was the one in charge of all the Air Force students at the University of Illinois, in terms of deciding who would be admitted and then working with us once we were there—a very nice person.

It was an unusual situation with your thesis advisor when you were off campus and coming back. It was very difficult at times. He understood and he would make time for me. Before the oral exams, he gave me a practice oral exam every week for about two or three hours—every single week. He'd look to see if I had sweaty palms. By the time I got to the real oral exam it was a piece of cake. He had already done it in about ten different ways, you know. So I was able to anticipate—I guess first of all you were sort of not clutched, and you were also being tested, you know, on a weekly basis on your feet without having a chance to go read up for this or that. He was a very good advisor. So I did my Ph.D. in non-linear servomechanism theory, which was an area that I had studied at UI under a real famous guy, Norman W. McLachlan, a professor from England. It was more of a theoretical thesis, but I used both analog and digital computers in carrying out the work.

BROCK: To prove the math or something?

FINNIGAN: I would say in the case of analog, you could simulate complex non-linear systems by actually putting in the non-linear functions and looking at things like backlash and other non-linearities to see their effect on control system performance and the limitations imposed by the devices that you were using. The digital computer allowed you in some ways to check the analog, but in some cases it would do things that you just couldn't do with the analog computer. In the process, I learned both digital and analog computation pretty intimately, and the strengths of each.

BROCK: Did you have access to computers at the Air Force Institute?

FINNIGAN: I used the ILLIAC at Illinois for the digital computation. Then I used the analog computers right in our department at the Air Force Institute of Technology. They were good analog computers, and a professor who really knew them ran the lab and taught me to use them.

BROCK: You had teaching responsibilities on top of that?

FINNIGAN: Right.

BROCK: What were you teaching?

FINNIGAN: I was teaching transmission line and antenna theory and practice. I can't remember the course name. But it was basically a course in which you would figure out impedances, voltages, and currents in wave-guides, on transmission lines, and so on. It was sort of a basic course for a lot of other courses; you needed it before you could take antenna theory. But I was actually teaching antennas from Dr. Edward Jordan's book on antenna theory (5). I was also teaching servomechanisms, usually one course a semester and usually one lab a semester.

BROCK: Pardon my ignorance about the Air Force Institute, but you were teaching other officers, right?

FINNIGAN: Yes, most of them were in graduate school.

BROCK: Because the Air Force Academy was still several years off?

FINNIGAN: Then, the Air Force Academy was just getting started. I think in 1954. I don't think they had any graduates till late 1956 or 1957. It probably opened in 1953 or 1954. However, the Air Force Institute of Technology continues to this day to provide graduate courses for USAF officers in engineering.

BROCK: All right.

FINNIGAN: So I was teaching graduate courses, and they were all officers working on their master's degree in EE. Some of them were hoping to go on for a doctorate, but not at the Air Force Institute. They were mostly very sharp people and you had to really be on your toes to handle them. Many of them had very good experience before coming to AFIT and they were serious-minded people.

BROCK: Right.

FINNIGAN: Several of the astronauts—[L. Gordon] Cooper and [Virgil I.] Grissom were students that I had at one time or another. There were a lot of people that became pretty upstanding officers in the Air Force that went through AFIT. So it was an important function for the Air Force. There were a lot of professors and civil-service people, who had been there a

lot of years and weren't necessarily up on the most recent things. So we Ph.D. candidates/instructors injected the latest of everything. I think it really turned out to be a good idea that General Swofford had, for us as well as for the people there. Because our courses tended to get top billing—we had more people sign up than we could handle.

BROCK: Right. So in 1957 you were finishing up your dissertation, and you were teaching. Was this another point, then, when you started thinking about career directions? Because, from my notes, I have that 1957 was when you came out to Lawrence Livermore [National Laboratories, LLNL].

FINNIGAN: Right. They called it the Livermore Radiation Laboratory (Rad Lab) then.

BROCK: Could you tell me the story of that transition?

FINNIGAN: Sure. I finished everything, including my final exams for the Ph.D., in 1956, but I didn't get the degree until 1957, you know, February or something like that. When I had finished my final exam, I called General Swofford, who at that time had become deputy chief of staff for the Air Force. I told him I was through and I'd like to come to visit. He said, "You don't have to remind me why." So I went to the Pentagon where he was one of the top people. I think he was deputy chief of staff for all of research and development and, you know, a large part of the Air Force reported to him. I had already decided what I wanted to do: I had heard about assignments at the University of California Radiation Lab at Livermore, where officers would go in and become, basically, lab employees—you could really do technical work. Most of the Air Force assignments I would be considered for were administrative where you basically were managing projects.

Also, my whole family was out in northern California by that time, so the proximity to them had something to do with it. So I told him I would like to get an assignment at Livermore. He said, "Well, I never even heard of Livermore." Livermore Lab was only two years old at that time. So it wasn't unusual that it wasn't very well known. I explained to him what I thought these projects were, and he was really against it. He felt I should go to Patrick Air Force Base, which is now Cape Canaveral, and work in missiles there. He thought that would really be the best thing for my career. I said, "Well, you know, I would prefer to go to Livermore." So he sent me down to talk to some people in USAF personnel. They said that yes, there were possibilities of assignments but it was dependent on the lab. At that time, the Air Force was putting up a lot of money for Livermore because they were not happy with Los Alamos [National Laboratory]. So they were putting it into AEC [Atomic Energy Commission] to set up Livermore. So there were some connections.

I just stated that was what I'd like to do and otherwise I would take anything they'd give me. So I went back to Wright Field to wait. About two weeks later they said, "We have orders

for you to go visit Los Alamos, Livermore, Rand Corporation," and some other place I do not remember. So I went to Kirtland Air Force Base, which was the control point for people going to the labs. I told them that I would like to go to Livermore and I didn't want to look at the others. The two colonels in charge at Kirtland had both worked at Livermore. They said, "That's a good choice you've made." The one I was working for directly had just come from there, where he had headed up B division, which was a very big job at Livermore, you know. It was Dr. Herb [Herbert F.] York and Dr. John [S.] Foster that had that job before him. So he said, "I couldn't agree with you more." He loved the place, so he sent me out, and I was interviewed by a lot of people at the lab to see whether they wanted me. I passed the interviews okay. The only question was whether I'd go into the EE department at the lab or the physics division. I preferred the physics division because it was the division sort of running things. They especially wanted me with my EE background.

BROCK: The physics group?

FINNIGAN: Yes. Because they needed control systems help on the specific project I was being considered for. So I went back and let the Air Force know my preference. They already knew that Livermore really wanted me. I got orders shortly after to report to Livermore through Kirtland Air Force Base. For a while I was managed out of Kirtland, which is at Albuquerque. Actually in the interview process I became aware of a nuclear ramjet reactor development that was intended for a nuclear ramjet jet missile the Air Force was very interested in.

BROCK: This is the Pluto?

FINNIGAN: Right. The ultimate weapons system was SLAM [Supersonic Low-Altitude Missile]. The Pluto was the reactor for the engine—the ramjet part.

BROCK: Now this was the idea of the nuclear ramjet-powered missile. They planned to have a long flight time where it could just be up there?

FINNIGAN: Right. It could stay up for many, many hours or days. Because once you got it to Mach 2 or more, it was able to propel itself with a nuclear engine unless the tail fell apart or something. At that time, you know, you could go into the U.S.S.R. [Union of Soviet Socialist Republics] from the South Pole if you wanted to.

BROCK: Right. And was this [Edward] Teller's idea?

FINNIGAN: No. Actually, it was a project envisioned, I think, by the Air Force and Air Force contract people who were building BOMARC [Boeing and Michigan Aeronautical Research Center]. BOMARC was a ramjet missile that the Air Force was using at that time. Edward Teller didn't have much to do with it. He was at the lab when I was there, most of the time. It was an Air Force instigated project. Originally, they didn't know, at the time I first visited the lab, whether they were going to do Kiwi or Pluto. Kiwi was a nuclear rocket engine, which used hydrogen as a propellant, whereas Pluto/SLAM used air as its medium. So by the time I got to the lab, it had already been decided that Los Alamos would do Kiwi, and Livermore would do the nuclear ramjet.

BROCK: The ramjet was a physics department project or a project that spanned the different divisions?

FINNIGAN: Yes. They set up a special division called R division. Initially R stood for rover, which was the nuclear rocket. It took people from all different parts of the lab, but it was mostly physicists that were involved initially. I would say almost every key person was a Ph.D.-physicist from U. C. Berkeley. Berkeley is obviously a good source of people.

BROCK: Now you were working on the control system for the reactor for the missile, right?

FINNIGAN: Right. We made two reactors in my period there—the only two that were ever made: Tory II-A and Tory II-C. Tory II-A was a prototype that basically proved in principal, but was not flyable. The Tory II-C reactor and controls were flyable. I guess I went initially as a staff member in the control systems group and then was put in charge of it after about three months. It was a pretty huge responsibility.

BROCK: In charge of the whole ramjet?

FINNIGAN: No, in charge of all the controls and instrumentation. We used people from EE, mechanical engineering, and physics—so we had people from various parts of the lab. But most of these people I hired for the lab. There was no Air Force connection, except there was an AEC/Air Force office that managed the project from Washington, and they'd come in every six months or so. At the lab, nobody made any attempt to cover up the fact that I was an Air Force captain, but it was not advertised, and I would say most people didn't know that I was an Air Force officer. The people in charge did, but most of the people working there didn't know, or if they did, it didn't really matter. It was a real outstanding opportunity, because I was able to get in and design control and instrumentation for systems. Then we built the system, and some of it we farmed out to aerospace companies, and so on. A lot of it we did ourselves, and we wound up testing it at the Nevada Test Site. It worked really well. It was a pretty unusual opportunity.

I had to learn nuclear engineering on the fly, so to speak, because the process was a nuclear reactor. We were putting control systems on it, and we didn't know at the time whether we might have a fast reactor. Are you familiar with nuclear reactors?

BROCK: Not that well. But fast means fast neutrons?

FINNIGAN: Yes. Normally, you would control a reactor using slow (delayed) neutrons.

BROCK: Right.

FINNIGAN: They have different decay times. We hoped that we did have some delayed neutrons that would allow us to control it, but we didn't know it. So we had to design a control system that would allow us to control it even if it were a fast reactor, which was sort of a bomb (almost). The worry was that the delayed neutrons would have blown out the tailpipe and that you would be stuck without them. They helped to make it a more controllable process.

BROCK: Right, so the idea was that at the end you wanted to control the power output, I guess, of the reactor—to drive the air and make the thing into an engine.

FINNIGAN: Yes. That's right.

BROCK: So it was a control system that had to operate within the parameters of a flying reactor, but it was controlled using rods, you know?

FINNIGAN: In Tory II-A we had a single control rod and then eight control vanes. They were circular. What we were doing was adjusting the negative reactivity. You know, these were neutron-absorbing devices, including the control rod. The rod was very fast moving. In a tenth of a second or so you could shut it down in an emergency. There were also several "scram" rods to accelerate the shutdown.

BROCK: Right.

FINNIGAN: You used the control rod to fine-tune the reactor power level. Then the control vanes turned more slowly—this was Tory II-A, the prototype—and allowed you to take the reactor to higher power. Typically, we had to take it from around 1 megawatt up to 300

megawatts in thirty seconds or so, which was quite a fast, and a potentially dangerous power swing. On Tory II-A we used hydraulics to actuate the control rod and vanes. Then we measured the power with neutron detectors, and adjusted accordingly using feedback control systems. On Tory II-C everything was flyable. So we had pneumatic actuators for all control rods. They were automatically controlled while running at 1200º F, which was the temperature of the incoming air, whereas in Tory II-A, those were run at room temperature. So there were some real differences. But both of them we managed to build successfully and test an extensive program at the Nevada Test Site. So it was quite an opportunity to learn. I have to say we put together a really good group of people. I was part of the project team on the whole thing, so being in the physics division—I was even classified as a physicist by the lab—made good sense.

BROCK: Right. So you were the representatives from controls on the overall project team.

FINNIGAN: Right.

BROCK: Was it a control system that you wanted for it to be able to fly autonomously for sure? Did it also need to allow for somebody to take control of it wirelessly to control the reactor?

FINNIGAN: Actually, in the test phase, we had the nuclear reactor located about two miles from our control point. So we had long transmission lines that went down to the reactor, which allowed us to control from the control point. We set in the desired power program, which in turn set the voltages that would control the position of the rods and vanes—or in the case of Tory II-C, just the rods to hold the power. That was done with a two-mile transmission line in between. All the appropriate data was coming back to the control room, so you knew where you were at, and you could override it if you saw something go wrong. Ultimately, in the missile, all of that would have been done with microelectronics, so it would have been right there next to the nuclear reactor. It would actually be a lot simpler problem if you had radiation-hardened components. And that was not a simple problem to find those.

BROCK: Right.

FINNIGAN: We were also controlling airflow rate from a tank farm, which provided air to cool the reactor. At 300 megawatts, we would be putting approximately 2,000 pounds per second through the reactor.

[END OF TAPE, SIDE 4]

FINNIGAN: The project was a challenge in that there was no book to go to to read up on what we were going to do. There was no book you could go to to find anyone who ever tried to take it to such high power levels in such a short period (seconds). They would take hours or more to do it in the industrial nuclear power plants.

BROCK: So nobody had done that with a nuclear reactor?

FINNIGAN: No. The only other people working on one were at Los Alamos. They were pretty secretive about what they were doing. I did go out and visit them, but they were not too open. The two labs were very competitive. So basically, you know, we were doing a project where we didn't know in advance everything we could expect. It wasn't, say, that we didn't do experiments before we took this thing to full power to make sure that we could control what we had. There's that unknown element there always. The first time that we went to full power was a pretty hairy time even though we knew we could hang onto it, even if it had no delayed neutrons. But you knew you had sort of a bomb on your hands rather than an easily controlled nuclear reactor. So I learned nucleonics on the job, if you would. My colleague, Mike [P. Michael] Uthe [Jr.], who, ultimately, went with me to SRI and EAI [Electronic Associates Incorporated], was a nuclear engineer from the Air Force Institute of Technology.

BROCK: Was he in the physics division as well?

FINNIGAN: Yes, he was. We had offices side by side. He knew the nucleonics end of it really well, and so we were a good team. I could get him to explain it in words that I could understand and equations that we could put into our analog simulations or digital simulations. We used a lot of analog simulation and a lot of digital simulation—the lab had just incredible facilities in that regard. We acquired for the lab most of the analog computers that we used. It was paramount, you know. So the background I had really was helpful at LLRL with both digital and analog computer know-how.

BROCK: As I'm sitting here thinking about it, you know, for the ramjet, you had to know how to build a nuclear reactor. But just in terms of reactor design, one that would fly had some different specifications. It sounds as if the control systems were going to be one of the major concerns. Well, one of the rate limiters was solving that control problem for the project as a whole because that was where a lot of innovation had to take place. So to be able to control the reactor over such a—what would you call it?

FINNIGAN: Rapid excursion.

BROCK: Yes, you know, over those quick changes in the range. Do you think that it's fair to say that the control systems were a critical part?

FINNIGAN: Right. It was. Because we had only a supply of air for our test that would last five minutes—it allowed you to stay at full power for three or maybe four minutes at the most. So you had to have a safe way of getting it from, say, 1 megawatt to 300 megawatts, which was a hell of an excursion, hold it there for a few minutes, and then get it back down without thermally stressing the reactor. The problem was that if you thermally stressed it, that is, if you didn't do this at the exact correct rate, it was going to cause thermal expansion of fuel elements; things that crack and basically destroy the reactor. The reactor itself was a challenge to build; I'd have to say, with a hundred thousand or more fuel elements that have a small hole going down the center of them. But, yes, the experiment was not possible, I would say in a practical sense, without a very good control system. So that's what we had to build, and we didn't really have any precedent for that—especially on Tory II-C where it was all built using pneumatic actuators for the reactor control system that were running cherry red at full power. This was in 1962 and 1963 that we were doing the flight version of it at the Nevada Test Site.

BROCK: You tested the Tory II-C in 1962?

FINNIGAN: I think it actually went on in 1963 and 1964, and I went back to work at the test site during that period from SRI. But we basically built that whole system by early 1962 and were waiting to test the reactor at the time I left the lab. It was a real challenge, and the lab was a pretty wonderful place to work in that time.

BROCK: My next question is about the environment at work.

FINNIGAN: Yes. The head of the lab, when I got there, was Dr. Herb York, who was around thirty-four years old when he was selected as lab director. He was young and everybody was in their twenties or early thirties and very bright. My director, Ted [Theodore C.] Merkle—was about thirty-two, I think, and I was just thirty years old when I came to the lab in 1957, but I wasn't the youngest guy around. At that time, I guess most of the people were in their thirties. You had to look hard to find somebody in their forties. It was a very dynamic place.

BROCK: Right. I was wondering—Herbert Brown ran Livermore, right? Or was that later on in the 1960s?

FINNIGAN: The first director was Herb York. He was succeeded by Harold Brown, who was the second director of LLRL.

BROCK: Herb York and Harold Brown—I'm confusing the two.

FINNIGAN: Herb York was the first director. Later, he became chancellor of the University of California at San Diego. Both York and Brown became deputy directors of DOD [Department of Defense] for Research and Development. York was succeeded by Harold Brown when he left for DOD R and E. Johnny Foster became director after Harold Brown left. They all worked in the bomb-end of the business.

BROCK: I guess Harold Brown is the person I'm more familiar with. He was an extremely young man when he—

FINNIGAN: Yes.

BROCK: I had even written down that young man's name, you know, with a question mark here on my notes. Well, anyway, I wondered if it was diffused across the whole shop.

FINNIGAN: Yes. It was. When I arrived at the lab in 1957, I think, there were six hundred people there. We used to have colloquiums every other week or sometimes every week that you were sort-of invited to. If you were in the professional ranks you were welcome to go. You got to mingle with Edward Teller, Herb York, Harold Brown, and all these people that were pretty outstanding. Most of them were very young, so they were my age, give or take a couple of years. But we used to have guest speakers such as Hans Bethe (Nobel Prize Laureate), [Isidor I.] Rabi, Henry Kissinger, et cetera. Many of them had Nobel Prizes—another was [Herman] Kahn, who spoke for two days on the next twelve world wars. They always had somebody pretty outstanding. Edward Teller and Harold Brown would sit in the front row and drill them when they were through talking. It was a great experience, in that you were welcomed into a very small group that was really good. The topics ranged broadly, you know, sometimes it was about nuclear weapons and sometimes it was about other things completely different, such as politics. We were always in a classified area, so there was no limit on what you could talk about.

BROCK: Right.

FINNIGAN: The way the lab ran, it was kind of cutthroat, in that if you screwed up on your job, you got fired. And you knew that, and that was just the way it was. You were expected to know everything in your area, and periodically you had to report on progress within your

division. Our division would have to report once a year or less on the status of the control systems with numerous reports that were written along the way, where your colleagues and the director quizzed you at some length and some depth. The head of our project (Ted Merkle) was a director of the lab. Then, periodically we presented to the top people at the lab. Before we could run our reactor, we had exams people from the top—at that time it was either Harold Brown or Johnny Foster and Duane Sewell, who was the operations manager of the lab. We had to present to them what we were doing, why, show that it was safe, and withstand a barrage of questions from really tough technical people. If you couldn't stand the flak you'd just better leave, because if you couldn't convince them that what you had built was really safe, they weren't going to let you run it.

I mean it was a serious responsibility they had. We also had to present this same detailed work to the project officers from AEC and the Air Force. You were accountable for what you did. If you screwed up, you just knew you were going to leave, whether you were an Air Force officer or a member of the lab. I mean, it was well known and that was the way it was. So it was a good environment and you had complete freedom to screw up. That, for me, was a real turning point in my career I think, in that we were with probably some of the smartest people in the world, with plenty of Nobel Prize winners amongst your audience.

You were matching wits, some of the time, with them, but most of the time you spent with your nose to the grindstone, making sure that what you were doing was right and was going to work. We had to present to the Air Force too. We had an Air Force general, Major General [Irving L.]"Twig" Branch, who ultimately was in charge of the whole weapon system. We all presented our results, our systems, and explained why they were going to work. Ultimately a lot money was coming out of his office.

BROCK: Earlier, when we were talking about your high-school experience and sort of a sense of urgency that characterized that point in time, you know, while the War was on. What reading I have done about the history of Livermore during this period is that there was a similar sense of urgency at that laboratory. So when you made your comment about if you didn't do well on your job you were just out—I was wondering if you thought part of that was this sort of very high performers working together sort of situation; do you also think it was that sense of urgency, you know? This was a dicey time in the world.

FINNIGAN: Yes. I think so. You know, we realized we were potentially in a fight with the U.S.S.R. and that what we were doing was pretty important. Thank God, it never was needed—that weapon system was a terrible weapon system. But at the lab, you know, you were with an elite group of people who had a high sense of urgency and you just didn't want to fail. I mean, just your pride was driving you more than anything. The fact that you had even brighter people than you'd ever seen before made you all the more want to succeed and not just do well but do outstandingly. It was sort of the culture of the lab, especially in the early days, you strived to be distinguished and not just passing. At the lab, just passing was a failing grade sort of, in our part of it anyway. I would just say though that your own pride, for me and for my associates,

was an important part of it—you just had to do well. That was all there was to it. And you had all the opportunities and whatever money you needed, help, or whatever.

I pretty much hired most of the people that were working in my area, whether I was an Air Force officer or a lab employee. I stayed on as a civilian for the last two years I was at the lab. I chose people from the EE department and ME [mechanical engineering] department. The people that worked for me—most of them worked for one of those two departments. In the formal sense, they were assigned full time to my group in R division. We were a close-knit team. I'd say the leader of our activity, Ted Merkle, was a good leader, extremely brilliant, demanding, and unforgiving in some ways, but had a good sense of humor.

BROCK: So in 1959, you had to really make a decision about your Air Force career? Is that around the time?

FINNIGAN: Yes. It was 1959. I was facing a new tour of duty, going back to Washington to be the AEC/USAF project officer on the project that I was working on. Whether I was immediately that or ultimately that, I didn't even find out, but I would become an administrative person in essence. The fellow who had that job at that time was a West Pointer from the same class as me (1949). I knew him from our first assignment in Florida. I knew what his job was and I didn't have any interest in it at all. It was a situation where also—we really were tight in terms of dollars and cents. I think we had about five children by then.

Most of all, I didn't really want to leave the lab in the middle of the project. So I decided to talk to my commanding officers at Kirtland AFB. They said, more or less, that the job you have right now at Livermore would be the equivalent responsibility of a colonel in the Air Force, and you're just a captain, so you're going to have to accept some come down in responsibility—not necessarily in the amount of dollars you control or whatever. But at the lab, I could start out as a senior scientist, and I was classified at the top rating of the lab when I switched over. I did the same job before and after, but it was more responsibility than I would probably have had in the Air Force as a captain. So I didn't really run into any resistance at all. They said, "We would really hate to lose you." But both of them had worked at the Livermore lab and really knew the situation from first hand. They were both generals in the USAF by then and they really didn't fight it. I said, "This is what I want to continue doing," and I left the Air Force with their blessing.

BROCK: So the Tory II-C project was completed in 1962, and you didn't get to test it until 1963. So there was a natural closure to the project.

FINNIGAN: Right. My part was finished. When I left, we had our part of the system running. Coors was making our reactor fuel elements—Coors Porcelain [Company]. That took a little

longer than was expected. So we had our part actually up and running by the time I left the lab, and so it was a logical time for me to leave.

BROCK: Was Coors Porcelain the same people who made you the quadrupole later on?

FINNIGAN: Right. We, in fact, hired the very group in Finnigan [Corporation] to make our quadrupole component that made our fuel elements at Livermore.

BROCK: Really?

FINNIGAN: Yes. A guy named Bob [Robert L.] Smith was the person in charge of making those. Our last reactor was made up of two hundred thousand fuel elements about four inches long with a hole down the center for cooling. Coors built a whole new plant for that. It had huge walls that were eight-feet thick. But anyway, that same group was the one that we used at Finnigan to make our ceramic spacers for the quadrupole—a very critical part.

BROCK: Sorry. Just the name rung a bell.

FINNIGAN: You're pretty sharp.

BROCK: Well, I was reading things again today before coming over, so it's short-term memory. But to go back, was there a chance to stay on at Livermore in 1962, or did they make you an offer there?

FINNIGAN: Yes. I was offered the head of the electrical engineering department by the lab in sort of a phase in—they had somebody there that they wanted to retire. The director of the lab made me this offer. I thought about it pretty seriously before deciding to move to SRI.

BROCK: How did that conversation start with SRI? I mean, you were probably weighing the two options, I guess, between staying and going.

FINNIGAN: Yes. I had talked to SRI earlier when I thought I might be leaving the Air Force, before we got into all the testing phase. I had talked fairly extensively with them, and they had sort of made me a standing offer, and then called me periodically to say that they were still really interested in having me come. So I was aware that there was an opportunity that was

completely different and, was not aerospace-oriented. It was a part of SRI that didn't do any aerospace work. I guess I always felt that LLRL's Pluto/SLAM project would never fly because it was too dangerous to flight-test, you know, the ramjet nuclear engine. There was no place to launch it and so on, so I felt it was a—I wouldn't say a dead-end project, but that at some point we'd wind up having to do something else. I guess I considered the management sort of position there, which was a department of a thousand people by then. It was a large group. But it didn't really appeal to me.

BROCK: So being the head of electrical engineering there would be 100 percent. You wouldn't be working on a project.

FINNIGAN: Yes. It was primarily administrative.

BROCK: SRI's standing offer was to come and work on process control systems—just control system research, or they'd give you a group or something?

FINNIGAN: Yes. They wanted me to set up a process controls group within the control systems lab, which was the group that did ERMA, the Bank of America project. I'd met a lot of those people, interviewed over there, and decided I was interested. I knew a little bit about it, not a whole lot. But I saw it as something completely different from Livermore, and more, let's say, commercially oriented in terms of projects.

BROCK: Right.

FINNIGAN: But, you know, leaving the lab wasn't easy. It was such a good place to work, but you were devoting your career to AEC and military type projects. That was the way it was. That was what the lab had always done and it's what it will probably always do. So I didn't take the job at the lab.

BROCK: Right. And your collaborator Mike—how do you pronounce his name?

FINNIGAN: Uthe.

BROCK: Uthe. He left Livermore to go with you up to SRI?

FINNIGAN: Yes.

BROCK: All right. To continue your working together?

FINNIGAN: Right. We really got along well. I think we both were ready to leave—I guess we both felt that the project at Livermore was going to come to an end, and it turned out Harold Brown was the one who killed it a year or two later. He was secretary of the Air Force at the time. It was decided that it really didn't make that much sense to keep it going when it got to the expensive flight testing stage. When we were proving feasibility and building the prototype, you know, it was a lot less expensive than once you were into launching and flying one of those things, which was the next logical step.

BROCK: Right.

FINNIGAN: So anyway, we both decided to go to SRI to pursue process controls research. We had considered starting a company with some colleagues from Bendix [Aviation Corporation] Research Labs who were working as a contractor to build the pneumatic controls that we used on Tory II-C. I'd looked at other opportunities with EG&G [Edgerton Germeshausen & Grier, Inc.], who had various things they wanted to do. We talked about starting an operation for them here in the Bay area, but ultimately went to SRI instead.

BROCK: I don't know as much as I should about SRI at that time or any other really. Could you tell me a little bit about it as you came to join it—just sort of its size, its nature as an organization, and then as a place to do forward-looking research?

FINNIGAN: Yes. In this period, which was 1962, SRI was the premier research institute in the world. It really was. Probably its only strong competitor was Battelle Memorial Institute, at that time. There were other research institutes, but SRI was number one. They were doing half military work and half commercial kinds of projects, like the Bank of America project at the time we joined them.

BROCK: And those were very sort of cutting-edge contract research projects?

FINNIGAN: Really cutting-edge. They were allowed to carry out projects like the quadrupole mass spectrometer development that were getting support from the Office of Naval Research, NASA [National Aeronautics and Space Administration], and people like that. But they were also trying to do a project, a microelectronics development that would have commercial

implications. So about half the money came from the military, NASA, and so on, and the other half from industry. At the time I went there they were probably two thousand people strong and had a hundred ninety million dollars in revenues. They were doing work all the way from economics, market studies, management systems studies, to almost pure research. One scientist there Doug [Douglas C.] Englebart had a prototype of a personal computer with a mouse running in the early to mid-1960s.

BROCK: Yes.

FINNIGAN: It was unbelievable, even to his boss. They also had good people in the control systems area and in instrumentation, but it represented almost a hundred-and-eighty-degree change from what I had been doing.

[END OF TAPE, SIDE 5]

FINNIGAN: It was different in the group I went into, in that you were expected to pay your own way, in other words, raise your own funding. There was nobody to raise dollars for you. In starting a process-control group there, we were expected to find our own projects, our own money, support ourselves, and do good research in the process, as opposed to Livermore, where it was funded by Air Force or AEC dollars and you knew where your next paycheck was coming from. There was no element of having to go out and solicit your own projects at Livermore, because you were on a project that had already been funded. The whole atmosphere in the part of SRI I went into was, "You are starting this and you'd better find some support pretty soon or you're going to have to find another job." It wasn't stated exactly that way, but it didn't take long to figure it out. They kept track of how long you were "on the beach," as the saying goes. If you were on the beach more than 30 percent of the time, you were being funded from overhead and somebody else was paying your way and so, you knew you had to find a job.

BROCK: So process controls, initially, was you and Mike?

FINNIGAN: Mike Uthe, right.

BROCK: It was the two of you setting up some activity in process controls. Were you thinking of all sorts of industrial process controls, or were you thinking in a particular sector, like electronics?

FINNIGAN: Well, we were thinking industrial process controls. I'd been the only one working on control systems *per se*. But Mike was a good physicist and had a good understanding of process control generally, and particularly the process. So I represented the controls end and he could understand the process, and working together we figured out better ways of controlling it. We were looking at forward ways of doing process control and we didn't, I would say in all honesty, understand much about what industry was doing until we came to SRI.

The day we arrived there, there was a person named Allan [E.] Lee, who was an economist, asked us if we would come work on his project, which was a market study he was doing for IBM, 3M [Corporation], and several other companies. The subject was industrial process control, specifically "Market for Industrial Process Instruments and Controls, 1960 through 1966" (6). IBM was the original and the largest supporter of that. IBM was trying to decide whether or not to get into the process controls area. Allan Lee was absolutely the best person in marketing research and conducting a market survey that I've ever come across, and I've met quite a few. He was an EE from Stanford (undergraduate) with an MBA from Harvard [Business School]. He was very sharp and had been doing this as his career at SRI.

BROCK: Market research in industrial markets?

FINNIGAN: Right.

BROCK: So this first project was essentially to do market research on the people you had approached to fund projects on process control.

FINNIGAN: Yes. We were really trying to define what the market was, and I guess if you wanted to get right down to it, help IBM decide whether or not they wanted to get into that market, whether it was big enough. It boiled down to dividing the markets into various instruments and different process industry sectors. You have power, chemical, petroleum, food, et cetera.

BROCK: Right.

FINNIGAN: So we were absolutely at the feet of a master. He was, really. It was a very valuable lesson that we learned in this area. SRI had it down to a real science. We took one industry at a time and boned up on it. We were looking at the markets for various kinds of instruments, both existing and future. So you didn't limit yourself to just things that were there at that point—like chromatography was just becoming used in this area.

But this turned out to be almost a yearlong study that we did, and we learned a lot about those markets. We recognized that it was an opportunity to learn about the things that we should be addressing from a process controls instrumentation group.

BROCK: So you spent that year working on this research. Next door to you was the group that was using a quadrupole mass spectrometer as the detector for making those electronic devices.

FINNIGAN: Right. We had two offices, one in economics, which was in a different building, and then our office in control systems lab, which was next door to the physics group—applied physics lab (APL) they called—working on the quadrupole and microelectronics program, in effect. At SRI you had a morning break every day when everyone would go out and you could talk to anybody about anything. They had free donuts, as I recall, and coffee. But the thing that interested us (Mike Uthe and me) was we'd hear all this screaming and yelling always coming from the lab next to us. And our lab was just about asleep—you know what I mean. You could have fallen asleep and you'd never know the difference. I would go over to the APL and Mike too. We met those people and asked them what they were doing that was pretty exciting. Pretty soon they invited us into their lab and show us what they were doing.

The quadrupole mass spectrometer was the first thing we saw because it was working already, and ultra-high-vacuum systems were already developed which used the quadrupole. So we became very interested in it. I'd say by the time we finished that market study we were very interested in the prospects for the quadrupole as an advanced instrument for process control. We wrote a proposal that I also have in this package that I'm giving to CHF [Chemical Heritage Foundation] that we sent to Shell Development [Corporation], 3M (people we met in the study at 3M), PerkinElmer [Inc.], and maybe one or two others, where we proposed to develop the quadrupole as an instrument for process control (7). And 3M had a start-up group that was going to be looking for products to build for the process control/instrumentation area. They were going to be the 3M process-controls group. We wrote that proposal and none of them accepted it.

BROCK: So that was a proposal for research that you would do at SRI for them—to look at the quadrupole mass spec as a new process-control instrument detector?

FINNIGAN: Yes. It didn't necessarily have to be limited to that. It could have been used by PerkinElmer as a scientific instrument.

They were active in mass spectrometry, selling Hitachi [High Technologies America, Inc.] instruments at that time. We thought this might be something they would want to do. So we saw that as a possibility, even though our emphasis was process control. The proposal proposed a three-phase program—fifty-four thousand dollars was the phase-one cost for whoever accepted it. As I think I mentioned in my paper for analytical chem, a chemist who

was a good friend until he died recently, Dr. Phill Wadsworth at Shell—had decided that he would support it.

BROCK: Yes.

FINNIGAN: If he had, I don't know if we would have ever started Finnigan. He called after we had met him during our study at his lab in Emeryville—and he said, "This seems like a good project for Shell Development to do." Then his boss came back from vacation and killed it. Shell Development later moved to Sugarland, near Houston. That's where Shell Development is still located. But we didn't get any takers, to make a long story short.

BROCK: So that must have taken a long time to develop. You sent it out to a number of people.

FINNIGAN: Right.

BROCK: You probably had every expectation that somebody would—because in terms of Shell's R&D project, this was not a huge—year one was not a huge amount.

FINNIGAN: Right. We thought we'd probably find somebody who would be interested. It was, I guess, during this study that we visited with EAI, Electronic Associates, in Long Branch, New Jersey. Almost from the first time I went in their door they tried to hire me, and really wanted me to join them and head up their process controls activity. They were, I would say, somewhat leaderless in that area. They had analog computer products that had application there, and on the basis of this study, we wrote a paper that got a broad coverage on modern process control. It was published by *International Science and Technology*, and it was published by Van Nostrand [Company] in a book (8). It talked about the advantages of digital and analog control, both of which we considered in this study.

EAI was very interested in what we found, and they basically had plans to get into that market area. So when we finished the study, they were really breathing down our throat, trying to get us to come and join them. So we really didn't have a very long period. We probably wrote the proposal in a month's time or less, and were pushing and introducing the idea to Shell and a few others. But we really, I would say, had sort of the "bums' rush" from EAI. I told them I wouldn't consider moving from here. They said, "Well, we'll set up the operation there then." We wound up electing to do it before we really got going at SRI. I would just say it looked more exciting and it turned out to be pretty exciting.

BROCK: Now they were primarily an analog computer manufacturer?

FINNIGAN: Right. They were leading in analog and probably were considered the IBM of analog computers at that time because they owned probably 80 percent market share.

BROCK: Wow.

FINNIGAN: It was not as large a market as digital computers are, obviously, but it was a very good quality company. I had used major amounts of their equipment at Livermore in running Tory II-A and Tory II-C. We actually used their equipment on-line, their process control equipment. We had a lot of it, I mean, probably a million dollars worth of it in our reactor simulation systems. When we didn't have the reactor plugged in we could run with the reactor simulated on their process-control equipment, such that from the operator standpoint, you couldn't tell the difference. We saw EAI as an opportunity to do process control in maybe a little more down to earth way than doing research kinds of activity at SRI.

BROCK: Right, and they were pressuring you and Mike too?

FINNIGAN: Yes.

BROCK: More you, but they wanted him too? Were they selling it as a, you know, bring your team over to our team?

FINNIGAN: Yes, I would say that. I had formed a friendship with one of the founders of EAI, named Fred L. Martinson. They needed the help, I think, if they were going to go in that direction. We saw it as an avenue for developing some of our ideas, like with the quadrupole. So I didn't plan on leaving SRI that soon—it was only a year and some odd months—but it looked to be a very interesting situation. EAI had large customers at NASA Ames [Research Center] and other organizations for other activities that we started at EAI, Palo Alto, such as the analog computer laboratory, doing analog computer services. We started a process-systems group and then quadrupole development. Then there was a sales group here as well. So we started and combined all of that in Palo Alto.

BROCK: So did you operate, then, as a division of EAI in a way?

FINNIGAN: Yes, an "independent department" was what it was called; the integrated controls department was the name of it. It was attached to the research part of EAI, which had its headquarters located in Princeton, New Jersey.

BROCK: So they agreed to set the shop up out here?

FINNIGAN: Right.

BROCK: Did you agree to those three lines of work that you were going to pursue?

FINNIGAN: Yes. We agreed to set up a computer facility to do computational rental work. Ultimately we wound up with thirty people actually working at NASA Ames, most of them working over there on-site, running NASA's computers. In addition, we had a large computer facility right here in Palo Alto, just a couple miles from my house. That effort was sort of paying the rent, you know, while we started the process-systems group, which became quite a successful activity as well—doing work for Aerojet General's [Corporation] nuclear facilities and others. We were pretty well known among a lot of the nuclear community doing work for Aerojet at San Ramon, Sacramento, and subsequently even at their headquarters at El Monte in southern California.

BROCK: So those systems, the process control systems, were for what?

FINNIGAN: These were for NERVA [Nuclear Engines for Rocket Vehicle Applications], the nuclear rocket. We had a lot of different contracts for other—what I call commercial controls—process control systems too. But those I described happened to be for the NERVA project. I guess we could have taken as much work as we could have handled there. We sort of limited that. But it was an area where we already had expertise, so we could easily take on that. Then I was heading the process controls activity for all of EAI—here as well as back in Long Branch, NJ, their headquarters. They had—I think about a dozen people involved in engineering and marketing of process equipment there. That wasn't counting manufacturing, which was really run separately. So I was commuting to Long Branch and Princeton about once a month to basically run the activities back there. We had people from manufacturing, engineering, sales, marketing, and R&D involved. During my visits there, we would meet and I would try to direct more of the activities to include instruments. Mike Uthe and I laid out a program for EAI that would make them more of a broad-base process-control instrumentation company. It was a pretty ambitious program. The first real significant product other than process analog computers was the quadrupole mass spectrometer that we had already become familiar with.

BROCK: So the idea was that they would apply this expansion of the EAI business more broadly, you know, to include systems and, well, the computer and the instrument for process control. Is that the idea?

FINNIGAN: Yes. We went to people like Phillips Petroleum [Company]. They were very close to EAI in a business sense. We tried out a lot of our ideas on them and they were receptive to this, and were helpful in saying, "Well, this is something we could really use." So we were talking to customers, many of whom we had met during the study at SRI. We had, I would say, big objectives for people as unknowledgeable in the business world as we were. We had talked to various companies like Wilkins Instrument Company (Aerograph Company), who were making GC's [gas chromatograph], and PerkinElmer. We really did a survey that sort of continued the focus from what we had been doing at SRI to try to define, you know, the market of the future in the process instrumentation area. At that time this market was dominated by Honeywell [International, Inc.], Foxboro [Eckardt], Taylor [Precision Products], and to a lesser degree by GE [General Electric Company] and Westinghouse [Electric Company], you know, for steel mills and power plants. But our ideas were revolutionary. I think what we were trying to do was nearly impossible. It was too revolutionary for such a conservative, staid industry as the process control industry.

BROCK: What was it about the analog computers and the instruments for process control? Maybe it's just I'm too habituated to thinking about digital computers. But the idea was to sell the customer a system where the instrument would take valuable measurements of the process. That information got fed into a computer system, right? So you could have an analog system to keep it in a certain band.

FINNIGAN: Right. At that time they were using pneumatic controllers in most process plants. I mean they were systems that had probably been around for thirty years or maybe longer. Some companies, like IBM, were trying to introduce electronic equivalents to that and basically gave up on it early on, I think, partly based on our study at SRI (9). We (SRI) really didn't see a big market for their computers or for their electronic integrators, electronic controllers—it wasn't a market of a size to be of interest to IBM. Their own people were recommending strongly going into it. We did not recommend going ahead. We didn't tell them what to do. We just said, "This is the size of the market. This is how we see it happening, looking ahead the next six years." They were considering doing this at IBM in order to expand the computer market such as that for IBM 360s, 1800s, and so on. But, you know, they would probably sell those to them anyway.

BROCK: Right.

FINNIGAN: So what we were doing from EAI was saying, "We really think that there is a need for analog controls that would really do the job better. They're relatively inexpensive and you could have, ultimately, a digital decision-maker back here—it would be sort of hybrid. But you need different and better sensors. You could really improve your process, if you had better sensors." And ultimately we saw a quadrupole mass spectrometer as one of those instruments which would give the composition of the products of a process plant, allow you to control levels of certain organic components, and so on. I think they're doing a lot of those things now forty years later. There was a colleague of mine at the Air Force Institute of Technology, Ted [Theodore] Williams, who built the first all-digital controls after he left the Air Force. He went to Monsanto [Company] and pioneered direct-digital control. It was considered really revolutionary and encountered great resistance. Today, it would be considered *passé*. I'd say a lot of the things, including quadrupoles, are used in process control now. But this was thirty-something years ago. And, then, that was too far out.

BROCK: Well, my understanding of it is that the digital process control or whatever the correct term is—the leaders in that were only doing that in maybe the early 1980s, in some big Exxon [Mobil Corporation] refineries and things like that. So it's interesting that IBM was considering making a go at that twenty years before it actually turned out that people did it.

FINNIGAN: That was in 1963. IBM had a process controls group right in San Jose. They were doing a competitive market study to the one we did at SRI. They were just pushing as hard as could be. They felt there was a large market there. We both made presentations to their top management. Their management seemed to believe our results. We also presented our results to Beckman [Instruments, Inc., now Beckman Coulter, Inc.] and the 3M Corporation. In Beckman it was the SPID, scientific process instrumentation division, who also backed off from the process controls market based on our recommendations.

BROCK: Did they?

FINNIGAN: Yes. Joe [Joseph] Lewis was the guy in charge. He was a really solid guy.

BROCK: Yes. We did an interview with him for the book that we did on [Arnold O.] Beckman.

FINNIGAN: We presented results of the market survey at each of these places that had sponsored the study. I would say with IBM—they knew the markets better than SRI. They wouldn't tell us what they were doing, but all along the way we had to report to them every three months or so with our results. So they already had our results. We were really put on the spot by the management who had the benefit of having their group, who knew what we

did—"Question this, this, and this." We knew those numbers so well. We knew those markets so well. They would have had to go out and make hundreds of visits in the marketplace, like we did, to have the same confidence in their results.

[END OF TAPE, SIDE 6]

FINNIGAN: Just a sideline—but an important one—one thing we really learned early in the game was that you look for the "gurus" in the business. The whole time you were visiting people, you were trying to find out who was the guy who <u>really</u> knew that business. No matter what field it was in, after awhile you could spot the guru pretty early on. Then you just forgot everyone else and went only to him, and you stuck with him until he threw you out. Like I'm about to do to you. [laughter] I'm kidding. Then you'd try to be nice, even though you were quizzing him and picking his brain. Then you'd call and say, "Can I come back now," after a couple weeks. Usually they would say, "Sure, we were just a little worn out the other day." Then you picked his brain clean. Pretty soon, you'd know from top to bottom that market area, petrochemical, power, or whatever it was. So we learned that. You did your whole market study if you were really honest with that one person (guru), but cross-checked like crazy, because sometimes you could be fooled or miss something. But when you were talking to a lot of these industrial folks you were not going to be fooled very much or for very long. But we wound up knowing them. The IBM market people didn't leave their office. They did their study from their office or by telephone. There was no competition in fact. Anyway, so we really knew that we had to find the potential customers and find out what they wanted. You could sort-of predict the timing of the market. You could predict the end result. You could predict what was needed, when it was needed, how much they'd pay for it—market size to a gnat's ass, and over a period of five or more years. We basically went out to ten years. You could really have a feeling of confidence about it, and that was just an absolutely invaluable lesson. Like with IBM, we had to tell them that there wasn't much of a market there for what they were trying to do. Ultimately, even in our own case with the quadrupole instrument, we recognized that it was a ways off.

BROCK: But you also probably thought that the time to develop the quadrupole technology—I mean, you recommended to IBM, "Don't bother going into it." You gave Beckman the report and they backed off. They were making computer systems, some analogs, some digital, and some hybrid. They only sold a few—I think they sold one to Shell Emeryville that was an analog digital hybrid. They thought there was going to be this big market, but they didn't find it.

FINNIGAN: Yes. And at that time several companies were selling gas chromatographs (some from Beckman)—process chromatographs. We would run across them every now and then and, you know, the customer expected the vendor to keep them working for their lifetime, more or less.

BROCK: Right.

FINNIGAN: Our clients at SRI were trying to decide if they should go in with computer systems—analog, digital, or both—should they introduce other instruments, and so on. We discouraged them without telling them what to do. Joe Lewis was as smart as anyone we met during the study. You know, he asked all the right questions. And he decided that they didn't want to extend their process control activities. You know, they were better off sticking to what they knew and he realized that this market was too far away and too small for a fairly long period of time. I think he steered the SPID division in the right directions. It was an important part of Beckman.

BROCK: Right. Yes.

FINNIGAN: He was one of the smartest people at Beckman, I thought.

BROCK: I'm trying to remember. I think he served in lots of roles.

FINNIGAN: He was working for [William F.] Ballhaus at the time we were doing the study. Arnold was chairman of the board and was not involved in the day to day, so he had brought Ballhaus in.

BROCK: Right. I think it was the competition there—a succession issue.

FINNIGAN: Yes. I can't remember whether Arnold sat in on our presentation. I know Ballhaus did. Joe was the one running it. The others were there to learn and they didn't interact with us. Joe had a couple of people that were quite good. He was very good himself. Solid guy.

BROCK: Yes. I'll have to go back and look at the transcript of the interview we did with him. Because when Arnold Beckman was having problems with William [B.] Shockley, he brought Joe Lewis up, and, I think, offered him a chance to try and calm those waters. I think Joe Lewis said, "No thank you" to that.

FINNIGAN: Yes. I think that's true. I don't think anybody could have handled Shockley at that point. It became a disaster in a hurry.

BROCK: Yes. I think it was unraveling at the time they made him that offer.

FINNIGAN: Gordon [E.] Moore could tell you more about that than just about anybody. Yes. Gordon, Bob [Robert N.] Noyce, and others—the traitorous eight—were all there. I crossed paths sometimes with Bob Noyce, but we've never talked about that one. In 1968, Ken [Kenneth R.] Shoulders was trying to get Bob Noyce to put money in for Ken to start a company to do some really way out things in microelectronics. That was the same year that Noyce and Moore were setting up Intel [Corporation]. They didn't tell Ken that because they were not saying anything about it. So Ken and Bob Noyce talked at a people-level and technical level. I think Ken was as bright as they come and, obviously, Bob Noyce was. That was a black period for Beckman because he put a lot of his own money in it. Didn't he?

BROCK: I'm not sure, but the company invested. Just the company alone invested a lot of money and his entire life was invested in the company. So it was a lot of his money. So with your market research, though, you thought that with the timing and the development of the quadrupole that there would be a market for the quad and the quad analog computer process control system?

FINNIGAN: I think a better statement there, David, was that we saw a good possibility and we were willing to investigate it on somebody else's nickel. You know what I mean? EAI wanted a process-control activity in the company, or so they said. It turned out to be not necessarily true, but they had one toe in the water, I guess, with their PC-12 analog process equipment. We were aware of the market, where it was going. We were realistic about it. I think I was always realistic about it—when, how long it would take, how it would maybe unravel, or how it would work out, and not feeling that it was some big hundred million-dollar market right around the corner or billion-dollar market.

Realizing that Honeywell, Foxboro, and Taylor had control of it by the throat and that nobody was going to make any real inroads unless they approved of it, but, I guess, I felt that there was a real need for direct digital control, *a la* Ted Williams, for more advanced analog control, such as EAI, and for instruments beyond the simple instruments they were using, which were very rudimentary instruments—but realizing that from here to there was not a straight path nor a very quick path. We felt that EAI had a very close connection, even through family. The president of EAI was a man named Lloyd F. Christianson. His sister was married to the person at Phillips in charge of process control. [laughter]

So through that connection they had already gotten into Phillips. We felt that if we could work with Phillips, a good company—they were a premier company at that time—we

would have a chance of trying out some of those ideas. We weren't proposing to set the world on fire with quadrupole or process-control equipment, but rather work with an industrial company such as Phillips—specifically Phillips, but also Shell—to try those ideas and work together to prove them before we went and tried to put them into a plant running on-line.

BROCK: Right.

FINNIGAN: I think that was probably realistic. We saw at SRI that the chances of doing it from SRI were just about zero. Because these companies already had their research groups. If they wanted research, they'd hire you to do a research contract. It appeared in most cases they'd rather do it themselves than work with an instrument company, whether it was IBM, EAI, or whoever it might have been. But we did see that long term there was a market for those things.

I think probably it's worth mentioning here, as we left off, I went back, and I directed the process-controls activity for the company (EAI) with various other people from back east—Long Branch, Princeton—who were involved. Then we presented a plan, Uthe and I did, for broadening the process control activities at EAI, including a budget. It wasn't going to be cheap, although by current standards it wouldn't have been very much. We presented it to the management committee, including the president of the company. We had made these big graphic aids—at that time you didn't have good projectors, so we had cardboard presentations. But it was very nicely done. We showed a series of products we thought that the company would need if they were really going to be serious in process control, and it included gas chromatographs.

We'd really done sort of a mini-market survey that tied onto the SRI survey we had already done. We didn't really get a great reception. Nobody said it was terrible, we disagree, or anything like that. But we didn't see any great reception towards it. The attitude was, well, we'd rather sell our analog computer equipment—PC-12 was what they called it. The company was formed by four or five Army Signal Corps officers (including the president) that came out of a fort right near Long Branch, NJ, the Signal Corps station.

BROCK: Sandy Hook, Fort Lee, or something? I can't remember.

FINNIGAN: I don't remember now. But it was right nearby. They all had the same Army background. They were very conservative, the president especially. The company was doing well, probably as a result of some of that. But we sort of read it like this was not anywhere near the top of his list, and he really dominated the company. So we came back to California, and Mike Uthe and I decided we wanted to go ahead and do the quadrupole and do it sort of with or without their support. We obviously had to have some support because we wanted to sponsor research at SRI in order to have some money to develop the product. I think at that point I had pretty much decided that this company wasn't really committed to process control, unless it was

going to make a lot of money fast, and that they really wanted to sell their analog computer equipment, which probably made sense. But there was no grand plan. They didn't mind us visiting Phillips and talking to their people—Tom [Thomas] Wherry and so on. But we really saw somewhat the handwriting on the wall.

So we came back and decided we would try to work with SRI to get the quadrupole out of SRI. We wound up accepting the proposal we had written at SRI, which I have in this package of stuff for CHF, for development of a quadrupole for process kinds of instrumentation (10). We wound up asking them to submit it to us at EAI in Palo Alto. They even submitted it with the same cost that we had arrived at—fifty-four thousand dollars which was the cost that we had computed for the first phase, the evaluation phase. So I took it to my boss, Romeo Favreau, who was vice president of research and computation for the company, and then Fred Martinson, my closest friend in the company who was vice president of manufacturing. I told them all the reasons for wanting to proceed. I had also talked to the various people in the process controls activity. And everybody agreed it really made sense to work with SRI, to try and develop it as a product, and maybe it would have application to process control.

The long and short of it is that the company was willing to give us fifty-four thousand bucks. In other words, we were taking it out of our own profits, if you will, because we had already made a profit early on in Palo Alto from computation and process analysis kinds of things that we were doing. So this was our project, and the president of the company didn't really know about it. Nobody exactly said that they were keeping it from him, but it became pretty obvious. I remember my boss, Ro Favreau, said, "Well, you know, there's no use telling Chris a lot about this. Let's just go ahead and do it." So we sponsored this research with SRI. They were producing reports, test data, et cetera. Then about that time SRI got a new president, Karl [A.] Folkers, who's quite a famous guy. He was vice president of research for Merck [and Company], the discoverer of Vitamin B-12, and various other things—a really good scientist. At SRI, they had made about a dozen of these quadrupoles for various parts of SRI to do research, and delivered one or two to the Office of Naval Research, NASA, or whoever. I don't really remember that part. But they'd built about twelve of them and they needed two more. Karl Folkers let it be known that he didn't think SRI should be in the production business. He could probably foresee, looking at it, that there would be lots more instruments produced down the road and that you might as well stop it now.

So they came to us—the very same people we had worked with—Ken Shoulder's group. It was headed by a guy named Charlie [Charles A.] Rosen, whose name you'll see on the report here, was the head of the applied physics lab. So they called me and asked, "Would you build us a couple of quadrupoles because we can't build any more." I said something like, "I don't have the foggiest idea of how to build one." They said, "You don't need to because we'll provide you with the people. We have two people who would like to moonlight. They'll come over and work at EAI at night, if you'll pay them." I said, "Tell me how much to pay them." They said, "We'll provide you with the drawings and stuff. And since we're working with you we don't see any problem with it." So Mike Uthe and I were the only two there at the time that were involved. We had all those other activities at EAI that ultimately comprised some two hundred people. But at that time we had only fifteen or twenty people.

BROCK: In Palo Alto?

FINNIGAN: In Palo Alto. So we said, "Okay, we'll do it." They said, "Here's how much we're going to pay you for the two quadrupoles," and named the price, which was probably eight thousand dollars or some fairly small amount of money. But their people were going to do it, and they knew better than we how much it would cost. They said, "In the process you might find that you will learn quite a bit about it." So those two people came over and started moonlighting. They helped us build two of those systems that we subsequently delivered back to SRI, which was really sort of a crazy business. But we were learning—from watching and being involved we were learning how to build it. We didn't have a manufacturing capability, and we weren't really in the instrument business—I don't think we had an EE on the staff other than me. So I asked the process-control team back east to help us. There was an engineering guy, Jim [James] Sanders. I asked Jim if he would try to make a commercial version of it that we could introduce as a product.

Sort of in the middle of that, a lot of things were going on. I've already mentioned to you—the person at SRI that asked us to build those quads asked me to bring an empty bag over during my next visit. He dumped all the drawings—everything I'm providing to Chemical Heritage—into my bag. And he said, "There's nothing illegal here because it's in the public domain. It will take two years if we try to do it through channels. You need this to build the quadrupoles that you are going to build for us. Everything is on those drawings that you need." So I said, "Well, fine. That makes sense to me." Then he said, "Get another bag out." He dumped into there names of a hundred customers or so that wanted quadrupoles and had contracted SRI.

BROCK: Wow.

FINNIGAN: They were people collected over a couple of years from various scientists who called people—Ken Shoulders, Charlie Rosen, or somebody at SRI. So he dumped those into a separate bag and said, "That's your first hundred customers there." We probably sold to every one of them, you know, in the subsequent two years. So anyway, we built and delivered two units to SRI. The engineering guy at EAI (Long Branch) who was designing the process control equipment, took the SRI electronics and built more of a commercial version of it that was probably a lot better designed and built than what SRI had done (which I would call more of a breadboard) but it was certainly a prototype. That would be the most you could call it. So the quadrupole heads (ion source, quad filter, et cetera) with the ceramics and all that, we were just taking what they had, using the same supplier that SRI was using. We didn't try to change that. So this was probably 1964. We had from EAI product drawings that looked pretty decent. This was the making of the Quad 200, which was our first product in this area. It sort of came to a

head at this point as to whether this was something that EAI should have done back east or out here.

BROCK: One quick question just so I'm following correctly. It was the fellow back east who developed a more refined design for the quad to make it a commercial instrument as opposed to this prototype? Then he got that information back to you out here in Palo Alto, and then you subcontracted out and had a small manufacturer building then?

FINNIGAN: No. The moonlighters built the systems for SRI. I think we gave Jim Sanders, an EAI engineer in Long Branch, a copy of the same drawings that I'm giving CHF and said, "We'd like this done in a prettier package and done right." So Jim did that. He had a small group of engineers under him, and he probably was involved in it himself as well. He did what we asked, basically. Then he just delivered that back to us. I think he probably had built one or two of them. But he delivered one of them to us, with drawings. A lot of the things were the same as SRI were using—power supplies, and so on. But it was a prettier package in that it lent itself more to commercialization.

BROCK: Right.

FINNIGAN: So at this point we didn't have any people on board to do the work. In the company, I think, the president still didn't know what was going on. We were all doing this, *sub rosa*, so to speak. I was still the head of process control for the company. I managed to talk to the VP of manufacturing and tell him what we were doing—VP of research, computation (my boss), and so on. They were all completely aware of what was going on.

So it sort of came down to "was there any interest back east in doing anymore with the quad?" We were becoming aware with customer lists from SRI and having a good association with various scientists that there were a lot of people out there that would like to get one. And they were aware of us as well. The company made a decision—following a proposal I made in 1964—to set up what we called the "scientific instruments division" here in Palo Alto, where we would have as our objective to actually build a commercial quadrupole. We were aware of the residual gas analysis market for this instrument, which was being funded by NASA, universities, et cetera—where people were just going to use the electronics and the quadrupole head and put it onto their vacuum system. They were physical chemists, chemical physicists, and physicists—all three, in organizations like Livermore, and so on.

We were pretty savvy market-wise from our experience at SRI and were aware that there was a sizeable market opportunity there. At that point, I was authorized by my boss to go ahead and hire a few people. It was strictly a bootstrap operation. We brought in a young fellow named Tom [Thomas R.] Conklin; his background was math and a little engineering. He was

out of Litton [Industries, now Northrop Grumman Corporation], which was a local company. It was a very good company then.

[END OF TAPE, SIDE 7]

FINNIGAN: He was more of a salesman—we figured that out pretty early. He mentioned Mike [Michael S.] Story, who worked with him at Litton, and said, "This guy is really the person you're looking for." So we brought in Mike Story—hired him away from Litton. He had been aware of the quadrupole from the group at SRI. Mike was good with his hands. He was a chemist—including being a vacuum chemist. We really needed somebody like that. So we kept Conklin to help. He helped by being a gopher and doing a variety of things. We recognized his potential as a salesman somewhere along the line. So subsequent to that, we hired a manufacturing fellow who became manufacturing manager, Richard [L.] Greenan, and we hired Loren Wright, the person from SRI who had been doing the moonlighting with their agreement. We hired him full time. He was an EE. He was sort of a "Mustang-type" EE. He had come up through the ranks, and I don't think he had a degree. But he had worked for Ken Shoulders at SRI and was very good.

BROCK: What was his name?

FINNIGAN: His name was Loren Wright. Loren, Dick Greenan, Mike Story, Tom Conklin, Mike Uthe, and I were all at this point working on the quad project. I found people to take care of a lot of the things, like the analog computer lab, where we were doing rental work. We continued to do this process control stuff, but I'd have to say we probably de-emphasized it at this point, recognizing the quad might be a huge opportunity.

BROCK: Right.

FINNIGAN: Fortunately, we were in a building where we had space to expand, so we made plans to take over various parts of the building. Actually EAI wound up helping us to get everything that we needed over the next year or so. At that time, we had almost next door to us, right down the street, a company called Ultek [System International], which was a ultra-high-vacuum company selling ion pumps and other associated equipment for applications like Stanford's linear accelerators. They were selling broadly. We were probably buying vacuum equipment from their salesman. But we were doing all of the quad work in one room a little bigger than this room here. I think it was twenty feet by twenty feet or twenty by twenty-five. We called it the "secret room." [laughter] I tell stories about it, even recently at Mike Story's retirement party. We kept the doors locked all the time. My boss had said, "Whatever you do, I don't want Chris (EAI's president) learning about this from you guys." He said, "It's my job to

tell him and I'm not ready yet. I would like you to keep it quiet because we might kill it any time as well, but for sure we'd have to kill it if Chris found out about it." I said, "Well, you know, I can work with that." So we kept the doors locked, and only certain people could go in that room. Everyone wondered what the hell was going on in the secret room out there. Sooner or later, they found out—not sooner in most cases. But the person from Ultek, Bob [Robert] Yarbrough was his name, we had to show him certain things because he was providing us equipment like ion pumps, Bayard-Alpert gauges, and stuff that we needed. He was really savvy. He said, "I know what that is that you have in there." "Is that a residual gas analyzer?" was what he asked. Ultimately, I said, "Yes, it is. And it's just a prototype." He said, "I could sell a hundred of those right now. You know, I really mean it." I said, "Well, who would you sell them to?" I quizzed him. I pretty soon became aware he was an unusual guy and he could sell a hundred of them. He was, I think, their only salesman at the time. But he told his boss, who was the sales vice president, Chuck [Charles] Piercey, who told Bob [Robert M.] Ward, who was the president of Ultek. Ultimately, they came over and said, "How about working together when this product is ready to go. We think we could become an OEM [original equipment manufacturer] for you, sell this to our customers, and really help both of our businesses." They were a company doing about two million in sales at that time, which was pretty small.

So I said, "Well, you know, let's stay in touch." We were moving reasonably fast. I asked them how they would do this, who was going to sell our instrument with only a few salesmen. I learned a lot about them. In a period of three or four months we had put together an agreement that we thought was fair, would provide a profit for us, and would give us a sales outlet. We insisted on having the right to sell the quad ourselves, because we already had a lot of interest from Livermore, people like that, and the University of California at Berkeley. We had another company approach us, Jarrell-Ash [Corporation], an instrument manufacturer that also wanted to represent us.

BROCK: Right.

FINNIGAN: At that point, we got the vice president of sales and marketing from EAI involved and my boss, Ro Favreau. They were offering advice and wanted a bigger mark-up on the price. Somewhere during that period, the president of the company learned about it. By the time he had learned about it, we already had a large number of orders, and a lot more on the come. With Ultek, they were willing to commit to fifty instruments the first year at a price of eight or nine thousand dollars per instrument which was what our original thoughts were on price.

I think the president of EAI—when he got over the shock of what had been going on without his approval—wanted to fire us and get rid of the whole thing. But he became aware that there were a couple million dollars in orders stirring out there, very profitable orders. I have always said he fired us all and hired us back the same day. But he did see that this would probably be a profitable business for the company. Then he got involved in the pricing and he really wanted to hoist the price, which was probably the right thing. By the time we really got

agreements settled with Ultek and Jarrell-Ash we had commitments for more than fifty instruments over a year's time. By that time, we were estimating ten or eleven thousand dollars for the electronics and the probe. This price would include a flange that would be compatible with their vacuum system. Ultek made sure we were industry compatible in every respect.

BROCK: Right.

FINNIGAN: So we wound up offering a commercial product. It was mid or late 1964 when we really introduced it. It didn't take a lot of introducing because we had people beating our doors down. We delivered the first unit to the University of California's (Berkeley) nuclear engineering department and the person's name was Professor D. Smith. We were quickly accepted. I mean it did things that nothing else around could do. At that time if you were trying to measure compounds in a vacuum, time of flight—Bendix time-of-flight—was probably one alternative. The GE magnetic residual gas analyzer (RGA) was probably the most often one used—Veeco [Instruments] Corporation were also selling a magnetic instrument.

BROCK: Those were magnetic sector?

FINNIGAN: Yes. They were magnetic sector. They were pretty low performance. Our sensitivity was at least a hundred times better. We could scan very quickly (milliseconds), which they couldn't do. They were doing fixed mass analysis with Faraday cup collectors. So they could only measure certain masses. The idea of scanning was not really possible with most gas analyzers. We were scanning an entire mass range in fifty milliseconds. We were able to do a lot of things they couldn't do. Right from the beginning, the quadrupole was very attractive to the customer. Best of all, it was a workhorse instrument. It just ran all the time. It didn't necessarily always give the best data around, but you could have almost everything not working very well and it would still give a reasonably good spectrum. We called it, almost from the beginning, a "workhorse instrument." It really, to this day, is that and more. With real advantages in scan speed, sensitivity, ease of use, a linear output, and workhorse capability it really caught on in a hurry. And Ultek sold their whole year's commitment in a few months time. I think they signed up for twenty-five instruments and they sold two hundred during the first year.

BROCK: In the <u>first</u> year?

FINNIGAN: Yes, so we had a very good reception.

BROCK: How were you keeping up with the demand? Did you ramp up the production really fast?

FINNIGAN: Well, we did. The manufacturing fellow that we brought in, Dick Greenan, was pretty experienced in production and was a very good person. He set up a group in a hurry both to build quadrupole electronics and assemble quad heads. We had a company in Redwood City that actually did the manufacturing, the grinding of the quad heads including the quadrupole filter, but we did the assembly. The company building the quad filters was called R&W Associates. We really learned in a hurry. And it helped that Greenan had brought in quality people. We were able to ramp up the manufacturing reasonably fast—not fast enough for the orders. But it wasn't all that difficult. So, EAI at Long Branch made only that one prototype. Then we modified and copied that which was mostly like the SRI stuff. So, we had basically the in-house capability of doing everything needed. All of a sudden we had a real business going. At that point, even though I was the head of all of the EAI operations at Palo Alto, I was spending most of my time in the quadrupole area helping quad sales, making sure we made our commitments to Ultek, met customers, learned what they were doing with our instrument, and so on. Mike Uthe was very helpful, in that he was a physicist by training, recognized the technical problems, and was very capable technically. So we had a year of immense growth. I would say it was as fast as we could handle and basically really more than we could handle.

We kept raising the price after that until it got to about fifteen thousand dollars, and with sales going up the whole time. The competitive instrument, the Bendix time-of-flight, was the only one that could compete. They weren't so aware of this RGA (residual gas analysis) market at Bendix. And they were at twenty-five thousand dollars, or a significant increment above us. So we had this product launched and really going well. We built a solid-state version of it, the Quad 150, we called it, starting in mid-1965. We could sell it for eight thousand dollars and make a good profit. We became the most profitable part of EAI by 1966, at which time we were going full bore. I don't really remember anymore the exact numbers, but during 1965 and 1966, I think we sold more than a thousand of these instruments, which was pretty amazing for a new instrument product. And with probably three salesmen, selling it only in the United States.

BROCK: That was for the solid state?

FINNIGAN: For both. The Quad 200 became the Quad 250 when we made some improvements to it, and we sold more of those than we did of the Quad 150. But they both were doing well. The instruments worked well. We provided service from Finnigan. There was also a person named Frank Davis working at Ultek who was very interested in servicing them—he was a very competent person, and conscientious. We had a serviceman, Tim [Timothy] Tooey at EAI—between him and Frank Davis we were able to train other people and basically have a service capability so that people were able to keep the instruments running. Most of the customers were physicists or equivalent, so they really could keep them running themselves. They preferred to, if the truth be known. In this period, especially in 1966, I used to go in to the

company every Saturday morning. It was located about three miles from my home. The reason was because every Saturday we got a big order from Livermore (Lawrence Livermore Laboratory). I just wanted to open the order, you know. They were ordering like one quad a week. It was a house account. We didn't keep too many organizations in the house account, but it was a good thing we kept Livermore. I think we kept LLRL and Berkeley as well. We had staked out certain customers, and Livermore was our largest customer.

BROCK: Was Mike Story working on product development at that time and pushing the technology?

FINNIGAN: Yes. Mike spent his whole career with EAI and Finnigan later on, designing and improving the quadrupole ceramic spacers, the quadrupole structure, the ion source, et cetera, and what we called the "head," the ion source. Mike had that, among other things, for the life of the product, I guess, until he retired recently. He was a vacuum chemist. We had Loren Wright doing the electronics. They were basically the R&D staff. Loren was equally capable in the electronics area. There were a lot of electronics problems, which we could talk about it for a long time. For example, you could scan the mass spec measuring a particular compound or just looking at the background peaks at vacuum, and then scan again a minute later or even a few seconds later and it wouldn't be the same. We call it peak hopping. It was due to instabilities in the RF/DC [radio frequency/direct current] generator, which has to control the DC from the RF voltage and keep the ratio constant to one part in a million (at that time). We had things like heating of components, rectifiers, capacitors, various things that would change the RF/DC ratio—hence change the amplitudes of the masses that you were looking at.

BROCK: I see.

FINNIGAN: It was a long time before we really had solved these problems. Fortunately, most of our users didn't really care that much about that when they were using it as an RGA. But we had some like a scientist at Livermore, Norm [Norman] Milleron, who would do in-depth tests on it. He could have sunk us, if he had wanted to, with his evaluations. Fortunately, we chose to work with him and to solve the problems, which we needed to solve anyway. We befriended him. He was on our side as a result, and because he was such a guru in the vacuum instrumentation area his approval really meant a lot, and his disapproval similarly. Anyway, Mike Story wanted to build a mass spec from this RGA—I guess we both did. So we ultimately set up a project for a product called the Quad 300, which would be a mostly solid state version of the Quad 250—the high end RGA, and would be a prototype mass spectrometer but without inlets. We were looking towards markets like GC-MS [gas chromatograph/mass spectrometer], which was really an unknown and non-existent market at the time. We saw that as a step in that direction. We reported in some of our papers, Hewlett-Packard [Company, HP] ordered one from us. They had just bought F&M Scientific [Corporation], a gas chromatograph company in Avondale, Pennsylvania.

So they, including Bill [William R.] Hewlett personally, saw GC and GC-MS as one business, you know, one product area. He recognized early on that mass spec was just another form of detector for gas chromatograph, and that the two were one and the same business. I would just say it was amazing, you know, how early he recognized that and saw the need for mass spec as an important detector for the gas chromatograph. On the other hand, we saw GC as one of the many sample introduction systems (inlets) for the mass spectrometer. I think that at the time Hewlett was more correct than us.

BROCK: Yes, at that time. So they bought F&M right around 1966 or 1967?

FINNIGAN: Yes. I think they bought them in 1966. It was in 1966 that we were first aware of it.

BROCK: And they quickly made the order to you for the spectrometer?

FINNIGAN: Yes. The F&M people ordered it from us. But Hewlett and [David] Packard themselves were aware of this market and had been involved in the acquisition of F&M. There was a scientist, Gene [C. Eugene] Bennett, at F&M, who really saw mass spec as the detector that he needed. And Bill Hewlett really glommed onto in a hurry—he had a very good feel.

BROCK: Really? Eugene Bennett was one of the three founders of F&M, right?

FINNIGAN: Right. Aaron Martin and Gene Bennett.

BROCK: Right. And Frank Martinez was the third. At that time, in terms of GC-MS, what was out there in sort of 1965, 1966—I mean was it just Klaus Biemann's group who was doing that?

FINNIGAN: Well, there was a fellow named Roland Gohlke who had done it with Fred [W.] McLafferty at Dow [Chemical Company]. They built a GC-MS using a time-of-flight mass spectrometer some time in the late 1950s I think. I can't remember the exact date. But it was really a breadboard instrument, a jerry-rig, one-of-a-kind sort of a deal. But they had run an instrument early on.

At this point, in 1966, Syntex Corporation became interested in EAI's scientific instruments division, probably because Carl Djerassi was one of the key people there, vice

president of research. Alex [Alejandro] Zaffaroni was president of Syntex Research [Laboratories]. They had both come up from Mexico, and along with [George] Rosenkranz had been the three who had really done the work that led to the birth-control pill.

BROCK: Right.

FINNIGAN: So Carl Djerassi was interested in this operation and they approached us. They had a guy named Ken [Kenneth] Hansen, who was the vice president of development. He came over to express interest in what we were doing and wondered, you know, if we would like to be part of Syntex, and ultimately brought Roger Sant to review our financial numbers. We said that we would be interested in some relationship, partnership, or whatever. It wasn't ours to decide; we were owned by EAI. We were not a separate entity, but we were a separate department.

So they really expressed strong interest, which was relayed to EAI management. Initially, I met with them alone. I might add in this time period, (it was probably before Syntex approached us) a friend of mine at HP—Don [Donald Hammond]—who had worked with Bill Hewlett personally for quite a few years, was asked by HP to look over the mass spec field—this was about the time that they acquired F&M. Don worked in HP Labs as head of the physics group.

BROCK: Right.

FINNIGAN: HP realized pretty early on that they would probably need a mass spectrometer. They were looking closely at CEC [Consolidated Electrodynamics Corporation], which was a magnetic mass spectrometer manufacturer. Don Hammond and I had worked on the board at the University of Santa Clara together for quite a few years. (First in the physics department and, ultimately, the College of Arts and Sciences.) He came over to visit, so we talked about MS. I wound up showing him a little bit about the product. Not too much, but he was already somewhat aware of it from SRI, and he was also aware that we were working with SRI. Anyway, he subsequently talked to Bill Hewlett and Dave Packard about the approach we were taking with the quadrupole, and I think recognized that it was a better approach for what they wanted to do at HP than the magnetic MS. But around this time Syntex put the full-court press on us. They looked over the operation and ultimately made an offer to EAI for the scientific instruments operation, to acquire it.

BROCK: This was all happening with Hewlett-Packard? I mean, that was the fast pace of things happening in 1966.

FINNIGAN: Right.

BROCK: Hewlett-Packard ordered one of the quad?

FINNIGAN: Our Quad 300 mass spectrometer.

BROCK: For what? Evaluate, to see how it would work with one of their new F&M chromatographs?

FINNIGAN: Right. Exactly.

BROCK: Was Hewlett Packard thinking both of acquiring—

FINNIGAN: Yes, the quadrupole. That was it.

[END OF TAPE, SIDE 8]

FINNIGAN: HP probably wouldn't have needed us as badly as Syntex, you know, if EAI were to sell the operation. If it had been sold to Hewlett-Packard, I didn't know what the status would have been for many of the people in the company, because we were now a fairly significant number of people.

BROCK: About how many would you approximate?

FINNIGAN: I'd say, probably fifty or something like that. HP would have cherry-picked our people—in any area where they had other people who could take over those jobs. I had already followed other companies that had been acquired by them and seen the outcome. HP was always a fair company, but if they had better capability in-house, they would use those people.

So it appeared to us that if we went to Syntex, we would be kept as a unit and really be able to control our own destiny quite a bit more, because Syntex didn't have an analytical instrument division. Then, in dealing with Syntex—Roger Sant, who was chief of finance for Syntex Research, and I formed an immediate friendship. I trusted him right from the beginning. We would probably be working together, if we were acquired by Syntex. Roger and I would

probably be the two that they would look to to direct it. At that time, we were looking to move our operation across the street from Syntex in Stanford Industrial Park.

We were considering a building there because we were doing quite well financially and needed more space. In 1966, we had over two and a half million in sales and we made more money than all of EAI put together. EAI had around fifty million in sales at that time, maybe a little more than that. But they were doing very poorly on the bottom line (loss). I think that probably was one of the reasons why they were interested in selling the operation. They weren't really all that committed to doing analytical instruments and they could sell it for a premium, because if they got twenty or thirty times profits, it would have been a very profitable sale for them.

BROCK: Now as you were talking to Roger Sant, Syntex was getting very forthright about wanting to acquire the division. EAI seemed favorably disposed to do a deal like that. Did Hewlett-Packard know that that was in the offing? Did they make an offer to acquire them?

FINNIGAN: No, they hadn't. At this point, they were looking at the product waiting for delivery. We delivered, actually, on the very last day of 1966. So they didn't have the product. I would say that they were just sniffing around—I guess that's the right word. One day I had the corporate vice president of marketing and sales (Noel Eldred) visit. The next day, it was the corporate vice president of manufacturing (Ed Porter). I was ultimately meeting just about all the top echelon at HP. At that time, they were sort of functionally organized, where Hewlett and Packard had these key guys under them. All of those people came over, but neither Bill nor Dave came over to visit at that point. We would talk about a possible OEM arrangement or other things. I don't think HP stated forthrightly that they wanted to acquire the division. They wanted to work with us, with the product, but they were also being careful not to compromise themselves, so that if they wanted to build their own product they would be able to do that. Don Hammond was one of the principals at HP Labs, and he started a project there on the quadrupole in this very same time period.

BROCK: After visiting? So they probably wanted to see what they could do with it.

FINNIGAN: Right. Exactly.

BROCK: You talked about this immediate rapport and trust with Roger Sant. Did you also have meetings with Djerassi and Zaffaroni?

FINNIGAN: Yes. And with Dr. George Rosenkranz, who was CEO of Syntex Corporation. I met all of them. We met for lunch and spent some time talking about it. They left it to Roger

and Ken Hansen, their VP of corporate development, to recommend what they should do and how much it was worth. There was a lot of effort spent looking at the patent situation on the quadrupole. They, being a pharmaceutical firm, placed a lot of value on patents. And there was a patent situation where Professor Wolfgang Paul had invented the quadrupole in Germany. But he had published too soon in the U.S. before filing for the patent. So it was not felt to be patentable in the U.S.

BROCK: Because of that prior publication?

FINNIGAN: Yes. It was only a matter of a few days. But, nevertheless, it made their patent in the U.S., which they filed for, appear to be invalid. It was never really tested, as far as I know. Siemens [Company], I think, filed it, and they didn't really ever claim that it was valid.

BROCK: So did the Syntex folks find that to be favorable or unfavorable in terms of looking at the Quad?

FINNIGAN: Well, they obviously found it favorable that it wasn't patented by a company in Germany.

BROCK: Right, but, on the other hand, you didn't have a lock on it, the exclusivity.

FINNIGAN: Right. So that, probably for them, was unfavorable from that end. Actually we had no patents at EAI that we had filed for. It was all SRI know how. It was in the public domain, so there was no use in filing patents. We were really building the SRI quadrupole, so we weren't sensitive at that point to the whole patent idea. I think we realized that it was probably not worth a lot of effort for us at that time as opposed to just taking the football and running.

BROCK: Right.

FINNIGAN: In this period, we were doing extremely well at selling the quads. As fast as we could build them, we were selling them, making a really good profit. We had this strong interest from Syntex, which was somewhat de-stabilizing in the organization, in that people didn't know what would happen—some people didn't want to work for Syntex or didn't know exactly what it would mean for them. We didn't publicize the potential acquisition. It was pretty late in the game, during October 1966, before I got the whole organization together and

told them what was going on, because we had people working on analog computer contracts and other people working in other areas.

During this period Mike Uthe, Dick Greenan, and Loren Wright—all three were key people—left to form a company called Uthe Technology Incorporated (UTI), in order to build solid state electronics for the semiconductor industry. Some of their motivation was the feeling that they didn't want to be a subsidiary of Syntex, or whatever we would become. Part of it was just unrest.

BROCK: For those three?

FINNIGAN: For those three. Wanting to do their own thing, I guess you'd call it, and not feeling that they could under Syntex. It was something that was pretty unusual in the Valley, more so than it is now. Today it wouldn't even raise any eyebrows. But it was very disturbing to me, because Uthe was my right hand. We had been together a long time (ten years), and he basically did it without giving any advance warning. To take, as well, our manufacturing manager and the key engineering guy with him was really almost an act of war. It was worrisome. But I had brought in other people, in fact, who had worked for me or with me at Livermore to become part of the scientific instruments division. It turned out that a fellow named Bob [Robert C.] Marshall became a far better manufacturing manager than Dick Greenan. He was working in engineering at the time Greenan left, and I quickly saw that he would really be an outstanding manager. He later became manufacturing manager at Tandem Corporation and several other firms. He became a very successful executive in the Valley. I had another electrical engineer, Gordon [G.] Nelson, who worked for me at Livermore, became the chief electronics guy and we had other people as well. We recovered, but losing Mike Uthe was a big blow for me.

BROCK: Right, and that was in late 1966? Since they were key people, they knew about the talks with Syntex.

FINNIGAN: Right. Yes. I think the destabilization of the organization started in May. It went on for about eight or nine months, you know. It was the sort of thing most acquiring companies would have done in a couple of months' time. They would have either said yes or no, because you can't keep it secret after a certain period of time. Our employees saw all these people coming in, looking over the operation, and so on. So those three formed UTI. Ultimately, they made a quadrupole mass spectrometer, for the semiconductor industry, to be used in looking at cleanliness in semiconductor manufacturing systems, and so on. They built a good quadrupole. It was during that period that I recognized that there was a limit to the RGA market at some two or three million dollars a year.

I was carefully looking for the growth in the mass spectrometer market, where was it going to come from, but I didn't really see where residual gas analysis was going to grow that much more. There were competitors like Varian [Inc.] coming in. I became aware from Carl Djerassi of the GC-MS market, then I started doing my own looking and began to suspect that that could become a sizeable market. I guess dealing with HP as well, you know, and seeing their interest in the mass spec for their GC—all of those sort of added up, even though it was a zero market then. There was one company, LKB [LKB-Produkter AB, now Amersham Biosciences], selling a GC-MS at that time. They had just introduced it.

BROCK: And that was the only one out there?

FINNIGAN: The only one around. Right.

BROCK: So Syntex's interest was to acquire the division, keep the very profitable RGA business going, but look to develop the GC-MS. That was their idea?

FINNIGAN: Yes. We hadn't talked so much about it, but that was my goal. I think they agreed with that. They really wanted it. Anyway, Carl Djerassi wanted an analytical instrument operation within Syntex, so they called it Djerassi's "electric train." I think there was some truth to that. I guess through all of this it became somewhat of a self-fulfilling prophecy. In that, I pushed EAI to let us develop the GC-MS market, saying that was what we wanted to do and this was what it was going to cost. The cost wasn't going to be all that much. But we needed to have collaboration with a GC company or we needed to make our own GC, which probably wasn't in our thinking. We needed money to develop the joint product. EAI as a company was really hurting. They had been working on hybrid digital-analog computers and were becoming very unprofitable, and we were the only part of the company making money. They never said you can't do it, but they never approved of us doing that. They would not take on a sales force that was different from their computer sales force; they were against our hiring salesmen. They knew how expensive it was going to be, so EAI was quietly sort of sitting and hoping that they could sell it off.

But it was not lost on me or on others in the organization that their interest in this had diminished, and along the way, I personally had to decide if I wanted to be with Syntex or EAI. EAI had earlier offered me an opportunity in their computer operation in Long Branch. They were considering me for vice president, operations. I told them I wasn't interested in it, and I didn't go back to interview for the job. They sent the chief of personnel (Dick Donavan) out here to talk to me, and I ultimately told him that I didn't want to leave the Bay Area and that that was that—which was not a very good thing to say when the president of the company had sent him out here. I said, "I just don't want to waste your time because I'm not interested, you know. I wouldn't leave here." I had family ties and so on. I liked the company and all that, but I didn't want the job and I didn't want to move. And that was not taken very kindly. [laughter]

But I felt like I might as well be honest and not go through all the rigmarole and have them make me a job offer for me to say I really didn't want it. I probably wasn't very shrewd in how I went about it. But I was at a point where I just didn't want to do it. I'd been going back and forth enough that I really knew the lay of the land. That also took place during this period. So it wound up with me out on a limb that I was quickly sawing off. [laughter] Ultimately, Syntex decided not to go through with the purchase of our part of EAI. And it wasn't Djerassi or Zaffaroni. They had three lawyers on their board. The company that owned them was a firm back east, Allen and Company, who was the primary shareholder and held three positions on the board and controlled the board. They didn't see that it made any sense at all. They were the ones that called it Djerassi's "electric train." So they all really killed it just by dragging it out and dragging it out and, you know, by asking for more information, and this and that. So sometime in late October, early November 1966 it died.

BROCK: November 1966.

FINNIGAN: Right, 1966. At that point, I had sawed the limb off to the point where I really felt that I couldn't continue to work there. I didn't see any future for the GC-MS product at EAI and that they really needed the profits we were making to keep the company going. As I saw it, it was not a retrievable situation. The last day of 1966, I resigned from the company. Before that I had tried to buy this operation through a leveraged buyout (LBO). I had relations with the Stanford Bank and various individuals who were willing to put up money, so I proposed to the board at EAI that they sell to us part of the company, the people in the company, and the new investors. I proposed that we buy half, and EAI would continue to own half of it, or something of that order. They really didn't appreciate that. I mean, at that time, it was a conservative eastern company and they interpreted it as a gun to their head, which it wasn't really meant to be at all. But it ultimately would have probably been to their great benefit if they would have done it. But they didn't see it that way, so they just said no, they weren't interested in doing that, and they were going to initiate the sale of the operation to either Varian or HP. So I resigned in the end of 1966 and left to form Finnigan.

Roger Sant had asked me what I would do should the acquisition fall through, before he told me what the outcome was going to be at Syntex. I could see it sort of coming. "What will you do if it falls through?" I said, "Well, I would like to start my own company, if I could find backing." He said he would like to be involved with me, help to raise money, and become involved on the financial end as well as the overall business. As it turned out, there were several venture capitalists that came to me and wanted to fund Finnigan, but I had decided that I really wanted to work with Roger and T. Z. Chu, who was willing to put up some money and offered a great prospect as a president, CEO, general manager person. I resigned and turned the thing over to EAI. They sent somebody out from Long Branch who was going to be the overall manager. Then the fellow that I had brought from Livermore, Gordon Nelson, became general manager.

BROCK: I have just a couple of quick questions on that. Did you talk with key people in the group about your thoughts before you resigned? I mean, when you resigned, if I'm understanding correctly, you did have the thought "I'll start a company—my next step will be to try to put together to deal with the GC-MS." Did you talk to key people in the group to try to get them on board?

FINNIGAN: Well, you know, it was sort of a touchy thing. I didn't feel it was ethical for me to be recruiting people for a new company while I was at EAI, so I didn't. I talked to the whole group about the outcome at Syntex. Of course, everyone wanted to know what was going to happen now that had fallen through, because a lot of them were counting on that as a good opportunity (including me).

BROCK: Right.

FINNIGAN: I felt—I cannot answer for the company—but I felt that they would probably want to find somebody else to step up to the plate, and that they had no ambitions in the analytical instrument area, for instance, beyond RGA. I was it, you know. I didn't talk to anybody. I resigned. I wrote it out longhand and sent it to the person I was working for, Fred L. Martinson, who was still a good friend. I sent it to him and said I would be gone in two weeks and if they wanted, I'd be gone sooner. Then a number of the people approached me as soon as I was gone. They also recognized that it was sort of a touchy thing. Mike Story was the first and a guy named Dick [Richard L.] Hein, who was a close associate of Mike Story and was a chemist as well. I had already decided that I would go back to SRI and try to get Bill [William] Fies. I had talked to him before I left to see if he would be interested, and he said he would be. He was the key electrical engineer at SRI on the quadrupole project—really a genius. I had realized that while I was at SRI, you know, and I was sort of saving him in case we ever had to do something different. I was glad he hadn't been the one selected to do the work at EAI building the quads. Bill was a key guy. I felt that Bill, Mike, and I could do it with Roger providing the financial initiative and the business acumen.

Within maybe a week of leaving EAI both Bill and Mike had signed up. I took Dick Hein as well, because Mike felt that we needed him and he wanted to leave. I thought that EAI was probably planning to be rid of Mike and Dick—maybe not so much Mike. The person in charge, Gordon Nelson, who had been my number-two man at Livermore for four years or so, didn't get along very well with either of them. Gordon, I think, wanted to join us at Finnigan, but I really didn't feel he was the right person. You could quickly get into the business where EAI was sending letters saying, "If you take anybody else we're going to meet you in court." They sent me a letter more or less expressing those sentiments and saying, "If we were going to build quadrupoles, we're going to meet in court." I wrote them a letter back, which we had a lawyer read, but he didn't change a single word, in which I said, " I brought the quadrupole from SRI to EAI, not the other way around." They learned of it from me.

BROCK: Right.

FINNIGAN: I had worked on it at SRI, and most of what I knew I had learned at SRI. There was nothing proprietary. We were building the SRI quadrupole and we made no bones about it. We were telling the world that. We didn't have a single patent. We used the same electronics and quadrupole that SRI had designed, and so on. I pointed this out in a letter and never did hear again from them one way or another. They never said, "You have our blessing." And we really didn't intend to do the same things as we had done there.

BROCK: Yes. Two questions just to tie off some ends. One question, and feel free not to answer this if you don't want to, is you had this close collaborative relationship with Mike Uthe, the rupture in 1966, and his departure—was that a break in your professional relationship or a break in your personal relationship too?

FINNIGAN: It isn't like I never forgave him. The fact that he was working behind my back, so-to-speak—people were telling me, so that I was aware of it. Then he made this rupture without any warning at all. We'd had some problems earlier, but I had kept him on at his request as a full-time consultant and partner. I really saw that his help was needed, so it wasn't all because I was a great guy. I was really generous in accommodating him, you know, and asking him not to work behind my back and sort of undermine the things that we were both trying to do as part of a company. There were some events prior to that, but I would say at the point where he left, I didn't see where there was a lot of room for continuation of a friendship, because it was a very close friendship, at least, from my standpoint.

BROCK: Right.

[END OF TAPE, SIDE 9]

FINNIGAN: With regard to Uthe, when I was asked by EAI to be director of this operation, I asked that he be named co-director. They didn't want to do that. Mike said that was fine, he would be associate director of the operation. When he showed uneasiness with this setup I said, "Well, why don't you be director and I'll work for you. I don't have any problems with that. Really." I really meant it. But it was not acceptable to EAI. It was too far-out—I guess you would say—from their conservative mode.

I don't know all the reasons. I'm sure there were a lot of things that were hard to discern, for me at least. But we had a good relationship right up to the last day, as far as I was

concerned. I don't guess it was quite the same as far as he was concerned. I think he probably wanted to do his own thing. I respect him for that.

BROCK: Was he eventually with UTI? Was that a successful concern?

FINNIGAN: I would say, no. The two people he took with him became absolute enemies, Dick Greenan and Loren Wright. They had to put a wall down the middle of the company with one of them on either side of it—I mean, literally. They made power supplies for the semiconductor industry as their first product, and about three years later made quadrupoles to be used in the semiconductor industry. In fact, I think it was some time in the late 1980s that Mike came to me and wanted to sell the company to us. UTI never went public, whereas, we went public within about four years of start-up. They had financial problems and wound up selling a large part of the company to one of the companies that produced gas, oxygen, nitrogen, et cetera.

BROCK: Like Air Products [and Chemicals, Inc.] or something?

FINNIGAN: Yes. I think it was Air Products. But they didn't do so well. They probably grew to five million dollars over twenty years or something of that order. I think they built a good product, but it was not a very big market.

BROCK: Right.

FINNIGAN: Mike died an early death in his fifties. At the time we meet, when he was interested in selling UTI to Finnigan in the late 1980s he said he had pancreatic cancer. He died shortly after. I don't know if he ever got any money out of it or not. I really don't know. At that point UTI was of no interest at all to us because we were in a different business.

BROCK: Right. What was the fate of the EAI division? How did that play out?

FINNIGAN: Well, after I left, they continued making RGAs, selling them through Ultek. The company tried to sell the quad operation to Hewlett-Packard. Once HP realized I was gone, they expressed zero interest. They just said that they wouldn't consider buying it without me. They didn't know Mike Story, but they felt, probably, the key people had gone with me. Hewlett and Packard themselves ran the product. As soon as it was delivered at F&M they appeared there the following week. Each of them was involved. They spent a good part of the day running the Quad 300. It certainly influenced their own entry into mass spectrometry. So EAI tried to sell it to Varian. And Varian looked at it for a while.

By that time Ultek was part of PerkinElmer. Then EAI decided to sell the RGA product through Varian and do the OEM'ing that we had done through Ultek through Varian. I think that worked okay for a while. But over a period, they tried to build the Quad 300 that we had developed, and delivered one to Professor Klaus Biemann at MIT. He was the guru in the mass spectrometer community. It was a pretty big failure. But anything done here without Mike Story would have been a failure, so they wound up maybe going on for about five years, and then they decided to move the operation from Palo Alto to Houston. I don't know exactly why; I don't remember. At that point I had enough problems of my own. It closed down. Gordon Nelson went off to do other things and I don't think anyone moved with the quad operation.

EAI, as a company, became a huge disaster and really—lost the only thing they had, the lead in analog computers. Analog computers became sort of like buggy whips, I guess. They tried to make the change to digital computers through the hybrid door but it never panned out. EAI has become an obscure company that does service. I think the only thing they do is provide service to other companies in manufacturing and maybe in field service of products. Within eight to ten years of my departure, it became a company in deep trouble. I don't think I left any enemies at this part of EAI in Palo Alto, and I hope not at the mother company. I think that was because we went after the GC-MS market, which they had openly turned down, and said they not only had no interest in, but strongly forbade me to pursue (while I was at SRI). That became our major market area.

BROCK: So in setting up the Finnigan Corporation you were working with Sant on financing and Chu, who was at Varian managing chromatography?

FINNIGAN: Right. He was head of the GC division. Aerograph they called it.

BROCK: He was an investor?

FINNIGAN: Right.

BROCK: Was he sort of advising, then, about the chromatography side of things?

FINNIGAN: No. I wrote a business plan. It was on a yellow paper pad, which I was going to get typed. I didn't start writing it till I left EAI. I really didn't do anything in that direction because I didn't feel like doing it. I wrote on yellow pad paper a whole outline of our business plan and went through it with Roger Sant. He calculated his own numbers and figured out from there how much money we would need and this and that. Roger is a very bright person. He's a very successful businessman today and has been on the *Forbes* [*Magazine*] 400 list. I

recognized him as a very bright person. He was not a billionaire then. He had made a little money on Aerograph and he had a good job at Syntex. He was very respected by Alex Zaffaroni especially, who's still a close friend of his, and still a good friend of mine too.

He was the person who had arranged the financing of Aerograph while working with T. Z., and had talked Keene [P.] Dimick, the founder, into using loans rather than selling equity. Keene wound up maybe making a large amount of money, twenty million or more, which was a lot of money back then, when he sold Aerograph to Varian. He was the principal owner of Aerograph. Wilkins Research [Services] they called it. But Roger was the financial whiz. He had worked, I think, for one of the investment bankers (Morgan Stanley Dean Witter and Company) and was helping to raise money for Keene Dimick. Then Keene offered him a job and that was how he wound up there. So Roger was proposing to me that he, I, and T. Z. put up the money for Finnigan and that they (Roger and T. Z.) put up guaranteed loans that were good for three years.

BROCK: Who would put up the guaranteed loans?

FINNIGAN: Roger, T. Z., and everyone, except the founding people. We had no money. I think I had eight thousand dollars to my name that I got from EAI from selling my stock at the time I left. So I was borrowing money to buy equity. Roger, T. Z. and I put up seventy-five thousand dollars amongst us. Then we raised another twenty-four thousand dollars—a thousand from Story and Fies, then the lawyers, and Jon [Jonathan W.] Amy, who was a professor at Purdue [University]. Anyway, we wound up with a total of ninety-nine thousand in equity. Then everybody except Mike Story, Bill, and I put up four dollars in guaranteed loans for every dollar of equity. This was Roger's idea because it had worked at Aerograph. Basically the three of us (Roger, T. Z., and I) would own the company, most of it. I might mention that my wife and I had seven children ranging between 15 and 1 years old at the time of the founding, so we were really in a high-risk situation.

I had planned on T. Z. coming in at the end of the second year. I thought we could afford to bring him on board and pay him—he would come in as the CEO and general manager, which was a job that neither Roger nor I coveted. We saw our own roles somewhat differently. That was the plan. So we raised this money. I might comment that the person who had owned Ultek and sold it to PerkinElmer—Reed Dennis, who became a very famous venture capitalist in this area—wanted to fund us. There was only one venture capital firm in the peninsula at that time, Sutter Hill [Ventures]. I knew one of the partners in Sutter Hill and he was willing to put up two to four million, whatever it would take. You never know, looking back, but I felt that we probably did the right thing. I never had any regrets, even though how we financed it was a mistake. At the time we started the company the interest rate we were paying was four percent. So it made sense to borrow money to start the company. But within six months, it had gone to six percent and went up from there. If we had raised more equity at the beginning we would have been able to survive without selling our souls, so-to-speak, in subsequent financing.

BROCK: Initially, you were acting as the general manager.

FINNIGAN: I was president and general manager. Roger was still working at Syntex, but he was VP of finance for the company. We brought in Alex [Alexander D.] Cross from Syntex who was director of the Institute of Steroid Chemistry under Djerassi. We also brought Hal Eyring (Henry B. Eyring, Jr.) of Stanford University onto the board. His is a fairly famous name. Hal Eyring is now one of the Elders in the Church of [Jesus Christ of] Latter-day Saints.

BROCK: Right.

FINNIGAN: Yes. He was a professor at the business school at Stanford at that time. So we were the board members. We had the help of Jon Amy, professor of chemistry at Purdue, who was an advisor, father, confessor—whatever. He had helped Aerograph through some really tough times.

BROCK: And Mike Story—what was his role?

FINNIGAN: Mike was vice president of engineering. Bill Fies and Dick Hein were engineers or chemists working for Mike. Bill never had any desire for a big title. I would say he was probably chief scientist from the day he arrived till the day he retired, without anybody ever wanting to compete or come in. Nobody could compete. We started up in Stanford Industrial Park, so Roger could sort of be down there part of the time without having to travel too far. (Syntex was only a few blocks away.)

BROCK: Right.

FINNIGAN: We found there a back end of a building, literally a garage. We rented that and it was relatively expensive but had a couple of offices too. So we were in the building on Hanover Street, across the street from HP and Varian. We were there for two and a half years or something like that, until 1970. We moved into a bigger building behind our original garage that our landlord built about two years later. We really couldn't afford that so we moved to Sunnyvale.

BROCK: Once you were in there, established, and really financed in your space, was the number one goal to develop GC-MS?

FINNIGAN: Right.

BROCK: That was the number one thing to do because that was going to be the product.

FINNIGAN: We had two number ones. One number one was to get some dollars coming in the door. We built a residual gas analyzer that was different. It was a higher specification unit than what we had done at EAI. We did this for a company called Granville-Phillips [Company] in Boulder, Colorado. We built it for them to sell. Part of the agreement was that we would teach them how to build them, so they could do it themselves, something they never took us up on. We also did that for a company in France that had been selling for us at EAI, they were going to build their own product anyway. So we made the same agreement with each of them, and I think we got fifty thousand dollars up front from each of them for doing this.

We re-engineered the RGA—we were not really that competitive with EAI/Varian. Those companies were going to do it with somebody else or do their own RGA anyway. But it was a smaller project, and we were out of that business (by plan) within two years. We really didn't pursue it beyond those two, developing that product for Granville-Phillips particularly.

BROCK: You saw that as the transitional business while you were doing the developing?

FINNIGAN: Yes. The electronics that we put in that product were the electronics we were building for the GC-MS. It had the same mass range to 750 atomic mass units, and so on. It was really just taking that part of it and selling it separately, selling the electronics and head. But really from day one, Mike Story was project engineer for the GC-MS 1015; we ultimately called the product Finnigan Model 1015.

BROCK: The 1015 was introduced in 1968? Is that correct?

FINNIGAN: Actually in late 1967. We prepared a brochure in which we introduced that product as a laboratory mass spectrometer. We showed models that were made of cardboard in the brochure because the product was not really ready to show. We got an order for that product from Stanford School of Medical genetics department in the fall of 1967, to be delivered in 1968. It was delivered on 1 January 1968.

BROCK: Did I hear you correctly, it was just a laboratory analytical mass spec with no GC?

FINNIGAN: It didn't have a GC because they wanted to interface it themselves to their own GC, which they had there in the lab. A lot of people at that time had their own GCs and would ask you to hook it up to that or one like it made by either HP or Varian.

BROCK: That was [Joshua] Lederberg's group that wanted it?

FINNIGAN: Right. They wanted to computerize it. They wanted to hook it up to the Stanford ACME system and to a LINC-8 mini-computer, which was the predecessor to the PDP-8. Walt [Walter E.] Reynolds was the person doing that project.

BROCK: Their desire to link that instrument into their computer system—did you have to do something with the electronics for the instrument that you delivered to allow for that?

FINNIGAN: We had to work with them. The quadrupole has a linear mass to voltage ratio, so that it was a very straightforward thing. We provided output 0 to 10 volts, which was over a mass range of ten to five hundred or so. So that was straightforward. They were the ones principally doing the interfacing, but we worked with them and would make sure that they had a place to grab onto if they plugged their computer in to both run the mass spectrometer, control it, as well as record data.

BROCK: The first one that would control it. So at that very early period you were thinking about the data system dimension.

FINNIGAN: Right.

BROCK: There was an early acquisition that you did at Finnigan, which was Disc Instrument.

FINNIGAN: This discussion is more involved, I'd say, than I've ever had with anyone since the formation of the company.

BROCK: Just our review of the details?

FINNIGAN: Yes. It's very detailed.

BROCK: I thank you for indulging me in my love of detail.

FINNIGAN: I might just comment on one thing that's probably very key, and that is that it was my feeling at the very founding of the company that it was essential—and I must say it probably was based on previous work with Djerassi's people and Walt Reynolds, who was Lederberg's computer guy—that we had to computerize the quadrupole system. I mean, that was part of our objective, to find a way as early as possible, feeling that there was too much data, you know. Let's say that our bag was to provide a computerized GC-MS something that was not very easy to do with other mass spectrometers with the magnetic instruments namely. We had a hard time doing that.

BROCK: Right.

FINNIGAN: We felt our success was just absolutely tied to that. It was our objective from the time of the formation of the company. That didn't happen right away but it was on our mind. Obviously, our first delivered instrument being computerized immediately, you know, made that something that we saw could be done and we could realize the value of. I spent literally hours and hours every week at Lederberg's lab in genetics with the people there seeing what they were doing, watching and learning in great detail the value of the computerized system. And so that was really tops on our list as something we had to do.

BROCK: Right. Was that part of the motivation behind the early acquisition of Disc?

FINNIGAN: No. It had nothing to do with it. In late 1968, we became aware that the owner of Disc Instruments wanted to retire and get out of the business. There were digital integrators coming along and he saw the handwriting on the wall. T. Z. was the one who told Roger and I that Disc might become available. We were running out of money at a hell of a rate at that time. We realized we needed a cash cow to keep the doors open. Warren Pirine was the owner of Disc. He wanted a million dollars for the company. We didn't have a million dollars, but Roger was very shrewd and felt that he could use their assets to raise a million dollars once we owned them.

So we actually asked Alex Zaffaroni to join with Roger and I. We both put in twenty-five thousand, I think, Alex put in a million, and we bought them. Actually, Alex guaranteed a loan with the Wells Fargo Bank. So we used that money to purchase them, and then immediately turned around and paid off the loan by borrowing money against their assets. We now owned a profit-making company, were able to pay off the loan and relieve Alex Zaffaroni of his obligation, and Wells Fargo were able to get their money back.

So Disc was making disk integrators for GCs. They were making digital shaft encoders as well. This was a product that we thought would have longer-term benefits, but Disc was always run as a separate company. So that gave us a balance sheet that looked better than it had with Finnigan alone, which was losing money at a pretty good rate.

BROCK: So you basically kept that organization just as it was—it was just an ownership change.

FINNIGAN: Right. I think we brought T. Z. on board as CEO shortly after that, on 1 September 1969. We hired a guy named George L. Jordan who had worked for T. Z. and Roger at Aerograph. We put him in charge of Disc. He was in charge for a number of years. But we ran it as a separate operation the whole time. We subsequently sold Disc to Honeywell for eight million dollars in 1980. It was a good investment and it made profit every year that we had it—made a good profit.

BROCK: When was that sale to Honeywell?

FINNIGAN: Sometime in 1980.

BROCK: So it was relatively long. I wasn't able to figure that out by looking around. So you got that in place. That was right around the time of—I guess, the GC-MS 1015.

FINNIGAN: Yes, sort of.

[END OF TAPE, SIDE 10]

FINNIGAN: The earliest mass spec brochure showed a laboratory mass spectrometer without GC. Then we put a gas chromatograph—it was a Varian GC—interfaced to it. That was our first GC-MS system. We purchased the GC's from Aerograph under an OEM agreement. We also offered HP.

BROCK: Did the customer choose between which GC?

FINNIGAN: Right, but we basically offered it with the Aerograph. We interfaced it. A big thing was the interface—a big problem area, a real challenge. We used a separator that I had

first seen when I was visiting a lab in Sweden. Einer Stenhagen was the person who ran the lab and invented the separator. [Ragnar] Ryhage developed the LKB interface—they called it the Ryhage interface, which was a metal jet separator. It also was actually co-invented by Stenhagen, when Ryhage worked for him. Stenhagen didn't take credit for anything. He was a very wonderful man. In visiting his lab, he showed me this single-stage all-glass jet separator. I asked them if that was something that was proprietary or what. He said, "No." He, in fact, gave me the drawings when I was there. I brought them back, and we started building some of them here with the help of some moonlighters from Varian—glass blowers, you know. So we made a glass jet separator, which was really for the medical and pharmaceutical markets. It was really needed—the metal separator (Ryhage's) tended to cause problems—reactivity with the samples being analyzed.

BROCK: Right.

FINNIGAN: So that was a critical area because everybody used open tubular columns at that time which could not be interfaced directly to the MS ion source because of the high flow rates of the carrier gas. It was a long time before capillary columns really came into use. So we introduced that with the Varian GC. We called that the Gohlke separator because Roland Gohlke, who joined us for a period in 1968 from Dow Corning [Corporation], helped to make this interface work. He helped in many ways besides the interface—building a new solid-state version of the GC-MS, for example. We called it the Model 3000 GC Peak Identifier. So that was Gohlke's product. It was a less costly, solid-state version of the Model 1015, our first GC-MS product.

BROCK: That came out in 1969 or 1970?

FINNIGAN: Yes, 1969. By 1970, we were delivering them. This started the 3000 family of products—the 3100, 3200, and so on. We introduced it at twenty-nine thousand nine hundred dollars for the complete system. Ultimately, it turned out to be quite a good system and was the basis for a series of products that were pretty successful.

BROCK: In 1968 and 1969, what were the orders like? How was that going with the orders and the manufacturing?

FINNIGAN: It was very difficult. I mean, it was a very bad economic time period. Plus, we had a lot of competition from PerkinElmer, Hitachi, and LKB. Varian had a product they were just introducing—M-66, I think they called it—which was a cycloidal mass spec. They were trying to use it as a GC-MS. HP was introducing a quadrupole—dodecapole, they called it. It was a tough time period. We sold maybe thirty instruments from 1968 to 1970. T. Z. said,

when he arrived in September of 1969, that we had thirty instruments in the field, thirty 1015s, which were sold mostly in 1968 and the first part of 1969 (none of them working). I was involved, probably, in every one of those orders. Some of them were outside the U.S, in Sweden, for example, to the labs of Stenhagen (Goteborg Universitet), Ryhage (Karolinska Institutet), and so on.

BROCK: What was your role in that period? Obviously, you were doing sales and getting out there and seeing, you know, like what they used in Lederberg's lab.

FINNIGAN: Yes. I was president of the company, but I was also looking for help from Roger Sant. We brought in another fellow named, Hank [Henry] Taylor from HP, to be a general manager sort-of type. I was spending probably half my time away from Palo Alto, and I was giving seminars sort of all over on the quadrupole. In labs, typically, I'd try to get a group of people to get together to listen, and I was giving talks at technical meetings. I would say I was sort of the chief spokesman to the technical world for the company. I visited many people in the field whether they were potential customers or not.

A lot of them were people who were using magnetic mass specs, most of whom wanted nothing to do with us. I was basically trying to do a lot of what I did at SRI: trying to find out who were the key people, and trying to find a champion for us out there. I did find and correspond with one such champion, Evan [C.] Horning at Baylor [University], who was a very well known chromatographer in his era, maybe the best around. Had been head of a large part of NIH [National Institutes of Health], and had set up a group at Baylor that was funded by NIH.

I learned of him while visiting a scientist named Geoff Eglinton in England at the University of Bristol. In fact, Geoff asked me if I knew Evan Horning. I said, "No, I've never heard of Evan Horning." Geoff then said, "I don't think you should be in the GC-MS business, if you don't know Evan Horning. It's not impossible that you could be serious and not know him." The first thing I did when I got home was found out who he was and went to visit him. As a result of that visit, in mid-1968, Evan Horning bought a computerized GC-MS system from us, the 1015. We had no computer, but he knew of Walt Reynolds' work. I was talking about doing something in that area. Evan had enough faith in us that he actually placed a purchase order for a computerized system. He had enough money and power to do that. We were in a tough spot when we accepted the order.

BROCK: Right. Did you have a time limit to fill the order?

FINNIGAN: I don't think we were pinned down in terms of the computer part of it. We didn't have the money to start our computer effort. Even though we knew Walt Reynolds and what he was doing, Walt was not the right guy to bring in to do it. He was a scientist. He needed

somebody he could show him how to do it. He was willing to do that. So for a period, Evan Horning took that part of the order away from us. We agreed that he would give it to Picker Nuclear, who was selling for AEI [Associated Electrical Industries]. AEI was a major company in the mass spec business with the MS-9. So he gave that part of the order to them with our agreement.

It was in this period that I met Ed [Edward] Zschau, who was a professor at Stanford, working with Hal Eyring, who was on our board. He was also a friend of Roger Sant. He and Hal introduced him to me, and I went to some of his classes at Stanford Business School. He was looking to start a company and a product that he could start the company around. So I suggested, and I'm sure I had help in this from Tom Conklin, who was our sales manager, that they should build a computer for our Model 1015 GC-MS. We sort of scoped out that product, sent him to see Walt Reynolds, and to see what Stanford was doing. So he started a company, Systems Industries, inside of Finnigan.

We gave him a room that they worked in. He hired several guys. Pete [Peter] Olson and Ed [Edward] Markel were the two guys that he hired who were really crackerjacks. Ed himself is a very bright guy (he later became our congressman in Silicon Valley). So they proceeded to start building a computer for our system. T.Z joined us in September 1969. We wound up getting the order back from Picker Nuclear, because they couldn't fulfill it. I was very close to Professor Horning by this time. Picker decided that they would buy the system from us, the System 150, that we were about to start selling with our GC-MS systems, and fulfill their contract with Baylor by buying it from us at no profit to themselves with all the money going to Finnigan and System Industries. Thus, Baylor would get a System 150 to run their Finnigan GC-MS. We demonstrated that very system at the fall ACS [American Chemical Society] meeting in 1969. We delivered it in November, I believe, to Evan Horning. So that was, as far as I know, the first commercial computerized GC-MS. I know it was the first commercial one that worked. It was a prototype system in many ways.

BROCK: That relationship between Systems Industries and Finnigan was obviously close. You were talking about what you needed in terms of performance characteristics and what the computer had to do and all that, so it was very close. But they were separate companies.

FINNIGAN: Right.

BROCK: You had a partnership?

FINNIGAN: Yes. It was a partnership even though we were separate corporations.

BROCK: For how long did that partnership go on, and what was the trajectory of that like?

FINNIGAN: Well, it went on until 1973, when we bought Quantamatrix [Corporation] and started to develop our own data system. In fact, Quantamatrix already had a data system that we could adapt to our GC-MS. I think, importantly, the GC-MS computer that included the System 150 was very slow in getting started. We sold one to Battelle about half a year after Horning bought his system. Rodger L. Foltz at Battelle-Columbus, who was a real pioneer in this area, bought one. Those were the only two we had out there until the EPA [Environmental Protection Agency] came into being in 1970. Then we started working with the EPA labs, particularly at Athens, Georgia. It was that environmental research lab that was evaluating GC-MS systems. We were running samples for them, and we were using Model 1015/System 150 computer to do some of that. They had a computerized Hitachi RMU-6 GC-MS at that time that didn't work at all. They were very suspicious of whether this would ever really be a workable product.

They had very good reason to be suspicious. [laughter] We were running samples for them. Ultimately, at the time of formation of EPA—I think it was 1 December 1970 that EPA was formally organized—several parts of the EPA were involved. Athens people—the people at Cincinnati Environmental Monitoring and Support Laboratory (EMSL) they called themselves—were involved in coming up with a recommendation for an instrument to be used in many of their environmental research labs and regional labs. They commissioned a group out of Battelle-Columbus to help in the evaluation.

This was where we had our system, fortunately, because even though the person that had our system (Rodger Foltz) wasn't on that committee, there were two people on the committee who worked with Rodger: Maynard Neher and Jim [James] Watt—both pretty smart guys. Jim Watt was a real computer whiz. They were aware of the success that Rodger Foltz was having with our system, and I think Jim Watt might have been in his group or associated with it. But there was a committee consisting of those two, another person from Battelle, and two EPA scientists: Bill [William L.] Budde and John McGuire. Both John McGuire and Bill Budde really preferred the CH-7 made by Varian, a magnetic instrument that really didn't have a data system yet. The people from Battelle thought our system was a better one, and ultimately they made their recommendations to the associate director of the EPA lab at Athens who was the decision-maker (11). This was in early 1971.

BROCK: Right.

FINNIGAN: There was a split vote—three out of five recommended Finnigan and the other two recommended Varian. They had evaluated HP, Hitachi, and all the others, but Varian was the only other company that wound up competing—HP really wasn't ready with a data system that worked at that time. We were actually running quite well by that time. But it was a real chancy thing for EPA to be doing, given our financial condition. We were not doing all that well. We had raised private money: two million dollars in 1969 at the time T. Z. joined us. We were running through that at a hell of a rate, and through a recession, as well, in 1970.

It was a very black period. I don't know I got you off track with all this. So it was a combined Model 1015/System 150 that EPA decided on. At that time we had been asked by several EPA labs if we would come and visit them to tell them about our system. We went out and gave a talk at every one of the EPA labs. I think there were twenty of them—we got twenty orders. I went in with the local salesmen in each area—I think we only had three salesmen at that time, but we would make a joint presentation. Basically each lab was allowed to make their own decision, but the EPA really wanted to have the same units in every lab. So we were visiting regional labs, environmental research labs, like Duluth, et cetera.

BROCK: So was that after the Battelle recommendation that you made those visits?

FINNIGAN: Yes, right after it. In fact, we already had a purchase order for three units including Cincinnati, Athens, and Alameda. The San Francisco Regional Lab was a station at Alameda. I think those three had already ordered. And we were not aware of how much control EPA headquarters would exercise, you know. Bill [William] Donaldson of EPA-Athens and Dwight G. Ballinger, director of EPA-EMSL at Cincinnati, were two people heading up EPA's decision on GC-MS. We weren't aware of how much their recommendation really meant or didn't mean. So what they told me was, "You'd better go out there and sell to these people so that they make their own decision, and hopefully in your favor." At that time there wasn't anybody else there trying to get their order. So it was really a pretty nice situation, so we had twenty orders almost immediately.

BROCK: Those systems were selling for how much?

FINNIGAN: I think we wound up selling them to them for eighty-two thousand five hundred dollars. You know, that was like twenty each system. We were probably making a little money on that but not a lot.

BROCK: That was a big order.

FINNIGAN: That was a big order, and in that period, we sold other systems because of EPA's interest. We had been selling units to Shell, Monsanto, Dow, and people like that.

BROCK: The people who were going to be regulated.

FINNIGAN: Right. As soon as EPA really put the stamp of approval on our systems, they were writing their protocols around the Model 1015/System 150. Then the industry just lined up to buy them.

BROCK: That was later in the 1970s?

FINNIGAN: That started in the early 1970s with the leading people buying, then it continued through the 1970s and even into the 1980s.

BROCK: The standard measurement procedures—the EPA started that in 1972?

FINNIGAN: No, actually they bought these, and they were probably starting to use them in 1972. Then Cincinnati, the analytical methods people, whose job was to propose and mandate methods for measurement of pollutants in waters, in drinking water, wastewater, et cetera, started their evaluation. They served this role for EPA as part of the Office of Research and Development [ORD] of EPA. There were various players in EPA. The Office of Water, for example, was separate from ORD. The Office of Water and the original labs were separate. So there were really a lot of players. I guess we never knew and I don't know to this day whether EPA said, "Buy Finnigan and that's all you can buy."

We were on a GSA [General Services Administration] contract, and they were buying off GSA, which was government procurement. It was a contract where they basically, on one page, gave you a purchase order. Sometimes it was two pages because they'd have to buy a computer separate because a different part of EPA had cognizance over the computer part, but we were going sort of on the basis that we'd better sell to all of the EPA labs and let them think that they were making this decision. For all we knew, they could decide what they really wanted. We never tried to ram anything down anyone's throat. I don't think it would have worked if we had, both for industry and for EPA. But Cincinnati started writing the methods around our system in the early 1970s.

Then when the Consent Degree Program came around in 1976, EPA was ordered to start setting standards and enforcing them. Bill [William A.] Telliard from the Office of Water and a scientist named [Lawrence H.] Keith, who had been at EPA-Athens most of his career and was later at Radian Corporation wrote an article for *Environmental Science and Technology* (12). In which they published the priority pollutant list for the first time.

BROCK: Right.

FINNIGAN: That was the first broad coverage that the priority pollutant list got, but they had methods to measure each of those. The methods had been mostly GC before we came along. Suddenly Cincinnati and other parts of EPA were doing that using our computerized GC-MS. The methods were being written around the Finnigan Model 1015/System 150. The consent degree, as far as I know, used our systems completely.

Then there were contract labs that were asked to run samples like California Analytical Laboratory in Sacramento and a number of others. Most of them already had our instruments, but the ones that didn't, got them. EPA wanted to have the same instruments being used in the contract labs, and the industrial companies also were buying them because they wanted to have equal power to EPA, if you will, and not have a different kind of instrument, which might be producing a different kind of data. So it became sort of a situation where everyone was on the same frequency. They were all buying our instruments and really creating a really good business for us.

BROCK: Was there a pretty even acceleration of orders over the 1970s, or what was there a curve?

FINNIGAN: Well, there was a big hump with twenty instruments in 1971, then there were no large orders for a while. There was a long rise in sales from there. Our total sales at Finnigan were going up throughout the 1970s—from less than two million dollars in 1970 to over forty million by 1980. We had a significant problem mixed in with this growth, and that was because Dwight Ballinger, who was the person making methods recommendations formally for EPA as head of the EMSL group at Cincinnati (which was part of the Office of Research and Development) had a problem on his hands mandating GC-MS as the required method, because, by the time all this was taking place, our instruments were selling for like one hundred fifty thousand dollars, which was a more realistic figure than what EPA had paid for their GC-MS instruments.

The competitive instruments were also pretty expensive. Ballinger made the statement to me when we were in Moscow together teaching courses to the Russian environmental scientists and chemists that "the day I required GC-MS as a mandated EPA method—the only or the approved method for measuring pollutants and organics in water, drinking water, and so on," he said, "will be my last day at EPA." Those were the very words he used. When we were teaching he was a good friend. He was being honest. He just said, "Bob, you don't have a chance," in terms of the overall mandate. In other words, "I cannot mandate GC-MS," was really what he was telling me.

BROCK: Because it was too expensive?

FINNIGAN: Absolutely. You know, he just said, "We've never mandated anything over twenty thousand dollars—probably not even that." GC was probably eight thousand dollars at that time or so he told me that, while we were at a hotel in Moscow having a beer. He obviously was telling me the truth. He was the one who had to recommend it, if it was going to become mandated—in other words, chosen as an EPA method. I was teaching courses part of the time with him, and some of the time separate from him where we talked about GC-MS, all the power of it, and so on. [laughter] He knew what it could do, and there wasn't any question in his mind about that. But, nevertheless, that was what he said. That stimulated our study that was published in *Environmental Science and Technology* (13).

BROCK: Right.

FINNIGAN: Which became known as "Priority Pollutants II - Cost Effective Analysis," which we dedicated to Dwight Ballinger. I think we acknowledged that he had created the need for the study that we did.

[END OF TAPE, SIDE 11]

FINNIGAN: I cannot say that GC-MS was not expensive, because it was, but it provided an unambiguous identification, which GC couldn't. Nevertheless, Ballinger was the boss and what he said went. As I saw it, the job was to find some way to prove that it was really worth the money that it was costing. Nobody ever talked about cost per analysis, you know. It's easy to look back with 20/20 hindsight and say, "That was obvious," but it wasn't obvious at all. People just talked about the cost of the equipment. We went out and started visiting customers. This was with Dave [David W.] Hoyt, who was working with me as a summer intern from Stanford Business School.

We were visiting various customers, both EPA, contract labs, Shell, Monsanto, and so on—industrial users—and asking them, "Why do you use GC-MS to do your analysis?" They would say, "Oh, it's the cheapest way to do it." I would say, "Well, God knows it costs you a bundle for it. How can it be the cheapest?" And they would just say, "I don't know. But it's cheaper, we get the right answer, and we believe we can defend the answer that we get. Furthermore, EPA and the labs look over our shoulder—they also have the same instrument." So we went out to learn more. It was a long time before we really got the concept that we should be looking at cost per sample, not total cost.

So our study dealt with that. We visited about sixty organizations—maybe thirty-five were supporting labs working for EPA—and wound up with numbers. We did in-depth surveys, *a la* SRI, getting a handle on cost and ultimately what was the cost per sample. Most of the labs used both GC and GC-MS, so they had data that they could provide us. We wound up putting all our numbers together and writing a paper. It was a presentation initially. I went around

giving it to various parts of EPA, including Cincinnati, Athens, and in Washington to the EPA enforcement people, Office of Permits on a Saturday morning.

We had a large number of people from EPA and industry that critiqued this before we ever published it. They provided some pretty interesting inputs and questions. The bottom line was that the data held up in court, and *Environmental Science and Technology* published it as a feature article in 1979. Our data was fairly well accepted by that time. I guess first of all it gave Dwight Ballinger the ammunition to say "Well, this is the cheapest way to do it." He could point to a hundred contract service labs that could do GC-MS analysis for you for four hundred dollars a sample or less.

BROCK: Right.

FINNIGAN: So he could promulgate a method that required GC-MS and not lose his job, [laughter] which, certainly, he was right to be worried about. EPA wound up deciding they should also do their own study and not accept our numbers unsubstantiated. They assigned a guy named Ed [Edward] Branch to do this; he was an economist for EPA at their headquarters. They specifically asked him to conduct, with some of his colleagues, a parallel study to find out costs for themselves. He had been a prime person we interviewed and worked with in coming up with our numbers. He did his own study, but he came up with the same results, more or less. That was when EPA submitted it to the Office of Management and Budget.

BROCK: That was in the later part of 1979?

FINNIGAN: Yes, it was formally proposed in late 1979. I think it was formally approved and became the law in 1984 some time. That was quite a long time. There was a change in administration—[President Ronald W.] Reagan came in and practically killed off the EPA. But, nevertheless, industry and environmental folks almost worldwide accepted it when EPA actually proposed it as the method. They proposed GC along with GC-MS. If you were to call any EPA office they would tell you, "We're using GC-MS. You use whatever you want. Either method is acceptable." The most important thing was what were you using. I might add that we did one very fortuitous thing in our study—we published reliability of data, which meant how accurate was this data. The lawyers at EPA really caught on to this the first time I presented to them. They didn't care a lot about the costs of performing an analysis. They were really interested in reliability of the data, something we had gathered from experts as we went along. We had a lot of backup for that. It showed GC to be a fairly unreliable technique for a majority of the organics being analyzed. So I kept trying to find out from EPA why they were so interested in reusability of data. They said, "Well, it costs us a thousand dollars or fifteen hundred dollars to collect the sample. Costs us, let's say, four hundred or maybe five hundred to run it by GC-MS. To defend the results, it costs us thousands of dollars, and ten thousand is not an unusual number just to defend data in court." So if a user or a petrochemical company presented data

and EPA didn't have GC-MS or vice versa, one of those parties was going to wind up spending a lot of money trying to defend unreliable data. That became the killer part of it in terms of really requiring GC-MS rather than just recommending GC-MS. Nobody wanted to go to court with data that was indefensible. So it became the way to do it.

BROCK: Was there a step function in the orders after EPA had the mandated method?

FINNIGAN: I would say so. Yes. It became a really good-sized business. Through the 1980s, it represented anywhere from 60 to 75 percent of our business at Finnigan, which was a lot. We were dominating the market a good part of that time. Then Hewlett-Packard came in and really started sharing with us, and it became a significant part of their business too.

BROCK: To backtrack just a tiny bit. Systems Industries was supplying the data system component of the overall system through 1973. Was that in fulfilling that first order of twenty from the EPA or was that with the System Industries?

FINNIGAN: Right. I'd say we probably delivered four hundred systems or something of that order with System 150—a large number.

BROCK: So here are some events I see coming together in time. You got that initial order from the EPA labs for twenty. You took the company public in 1972, and then Quantamatrix happened in 1973.

FINNIGAN: Right.

BROCK: Was the decision to take Finnigan public in 1972 tied up with the order that you got from the EPA? What were the implications of that? Did that give you the capital that you needed to do an acquisition like for—

FINNIGAN: Yes. I would say we needed that money in order to even be able to continue to build more and more instruments. I think we needed to have a public stock in order to acquire somebody, such as Quantamatrix. So, we raised money in order to have more flexibility. We didn't raise all that much. It was a very tough time to raise money, so we didn't get a very good price for our stock. I think it was pretty rock bottom. We had another public offering in 1974, because we needed more money by then. This, again, was to buy parts to make instruments and to expand our marketing and sales force, and to do the things to sort of take advantage of the lead position that we were in: If we didn't move fast—HP was always ready to move in. It was

sort of like a giant steamroller right behind you, you know. It would just flatten you if you ever stopped running. I don't think they were as entrepreneurial as we were, but they were pretty quick to pick up on it, you know. Here was a market opportunity HP felt that we ought to also be in, share the market, get a good part of it, and, in fact, one in which they felt that they ought to dominate at some point.

BROCK: Did it bring a lot of changes to how the company was operating? Did it bring a lot of changes when going public?

FINNIGAN: Oh, yes. We were living in a fishbowl, so to speak. You had analysts following you. H&Q [Hambrecht & Quist Capital Management, LLC] was our investment banker, we were the second company they took public—Spectra Physics [Inc.] being the first—so they were very aware that they had to follow us closely and help make sure that we did well for their reputation as well as ours. They had analysts visiting us quite often and kept close track: doing analyst write-ups for their public, the customers out there. It required us to report a lot more rigorously than before what we were doing—basically to give our shareholders information on the company—why they should invest in us and continue to hold our stock. It changed our life a lot.

BROCK: Right. Did you encounter any problems with that financial community or the stock-analyst community? Often times when I'm talking to various people, you know, they decry near termism amongst analysts as really being inimical to the time lines that you need for some really forward looking development project. Did you?

FINNIGAN: Initially, we were growing pretty fast both in revenues and profits. They were happy to see that. It was only when things turned down that they became nervous. But I'd say that they were watching us pretty closely the whole way. We had to prove ourselves and be able to present in annual reports or whatever—present a story that really hung together. I think Hambrecht & Quist was a lot of help. At that time, they were oriented towards small companies. We had spent a lot of time personally with George Quist and Bill [William R.] Hambrecht, that is, Roger Sant, T. Z., and I. They really thought long and hard and visited us for a whole year before agreeing to take us public.

It was a very difficult offering because we were not all that established and didn't have all that great a track record. But for us it was essential that we do that in order to really grow at a rate where we could keep our market share. HP had internal money and they didn't have that problem. All our competitors were big companies like HP, Varian, PerkinElmer, and AEI. All of them were a lot larger than us. We were taking market share away in some cases or creating new markets, but they were all ready to join in with us, and some of them, like HP, in a very solid way.

BROCK: Just a quick question. We talked about the changes of going public. I want to ask you if you had some words to say about the sort of organization that you built at Finnigan with your colleagues in terms of the culture, thoughts about R&D, and the relationship between R&D and production, which is oftentimes a problematic interface.

FINNIGAN: Well, that's a series of questions. We were research oriented from the beginning, and, obviously, at the very beginning, spending more on research than we were on anything else. As we went through this phase of getting EPA orders and industry orders following that, we continued to invest heavily in R&D. Mike Story was the person in charge of that during most of that period. We probably were investing 15 to 20 percent in many of those years in R&D, which was pretty excessive, but needed.

BROCK: Pretty high.

FINNIGAN: And we were working on new products, like the ion trap. We probably invested three or four years with the fellow who's in charge of research right now—George [C.] Stafford—working on that, when there didn't seem to be anything coming out the end of the funnel. So we were research oriented and we were also developing markets where we were actually bootstrapping our markets. In other words, there was no market before we came in, and they didn't know that they needed GC-MS. You had to show them what it could do. The environmental market is certainly an example. But other markets in drug testing, in law enforcement, and so on, all of those required a lot of spadework.

We were fortunate, I think, in many ways in having someone like Mike Story, who got along well with everybody and was heading R&D. He also had worked in manufacturing and was able to work with the people, so there was not a lot of jealousies or competition. We were fortunate in having T. Z., who had come from Aerograph where he had been the general manager. Really, my reason for wanting him was that I, first of all, didn't have interest or experience in setting up the systems, procedures, and all of the folderol, I would call it, in running things day to day, running a company, making all the minute decisions that have to be made and looked into before you made decisions, and getting a bunch of disparate parts of the company working together and keeping them working together. I felt that that wasn't my bag. I didn't want to do it and had, probably, gone out of my way to avoid it several times in my career. But it allowed me to go out there and find the markets for the products we had and help define most often, with Mike Story, the products for the markets.

BROCK: All right.

FINNIGAN: With customers, I would say, we were always very close. We didn't have a lot of formality in the company, and we had a lot of trust. Nobody would second-guess anyone else in helping to find an environmental market or second-guess other people in defining the product—what the product had to be for that market. I had no great pride of ownership. If I said, "This is what I think," then anybody could say, "I disagree like hell with you," and I would not take offense to that. I would want to know well, what's your better idea and let's work to figure it out. There was such a dialogue going all the time amongst key members that we didn't have—especially in the early days—problems. We relied on T. Z. to make it happen and, you know, get manufacturing cranked up to produce what we needed. When, during our market survey, we were defining a new product such as the organics in water analyzor, it was like trying to turn the Queen Mary, you know, a few degrees to come up with the product that was needed. T. Z. and I were very close. We would talk almost every day or two on the phone when I was on the road. I was in the field a good bit of the time, defining what was needed, talking about problems that we had, and ultimately making sure we were both on the same frequency. It helped that we were partners and Roger was the third partner. Then Roger took off in 1974 to become the deputy secretary of energy for conservation in the [President Gerald R.] Ford administration. That was a loss. He was chairman of the board, as well as the third partner in the company. I really missed him a lot, in that he had to leave the board while he was in Washington.

BROCK: Right.

FINNIGAN: Then he came back on the board after he left the Department of Energy. Anyway, Roger was always a hell of an idea man. He was the most innovative person I've ever run across in my whole life—in being able to come up with lots of ideas, and helping to find better ways and different ways, especially when you were in a box.

Roger is sorely missed, especially by me. We had to find ways to get around that loss. So, I stayed in touch with him and I visited him when he was working. He would come up with names of people that I could visit—like people from EPA. One of them became my lobbyist in Washington. Roger always had ideas that were just good ideas. He was a great guy to have on your side. We weren't real formal then, and especially through about 1980.

BROCK: Right.

FINNIGAN: We had a lot of outstanding people, and it was sort of keeping them all working in the same direction.

BROCK: Obviously, all of those travels, steering the company through those difficult times, thinking about new technologies, and having a large family kept you off the streets. Did you have any time to develop personal hobbies and interests over? Did you keep up with golf?

FINNIGAN: I didn't have time for golf, but I played handball three days a week through thick and thin, when I was in town. I played reasonably good handball, I'd say an A or B player, depending on whom I was playing with. Even through the busiest and worst times, I would take off during the day. At noontime, instead of having lunch, I'd go over and play handball. Then later, I converted over to squash, which became my favorite sport when I reached fifty-nine. So that was one hobby. Wine was another hobby—learning about wines and collecting wines. I ski. My wife and I learned together, starting around 1969, and we skied until I got sick a year or so ago. We have hiked together in the Sierra [Nevada Mountains] for many years, often with the children.

BROCK: Right.

FINNIGAN: We also did snow shoeing in the Sierra. So yes, I had hobbies. Tennis was another sport I really enjoyed. The family, obviously, had many demands on me. I don't think I did as well in that regard. Being gone, a lot of the responsibility fell on my wife's shoulders. That wasn't a good thing. But there wasn't any choice. She really was a partner through the whole thing. It couldn't have happened without her being willing to shoulder a lot of what should have been my job. We lived in this house in Los Alamos through the whole time; we bought this house while I was at EAI and have lived in it ever since. We raised our family of seven children here. She has been the best partner a man could have.

BROCK: Wow.

FINNIGAN: So we had a good home base of operation. Somewhere in there you asked the question, "Did I have extracurricular activities as well?" I worked for the American Electronics Association [AEA], a trade association. I was on their board of directors for three or four years and helped started their Environmental and Occupational Health activity. I co-founded it with Dean O. Morton, the COO [chief operating officer] of HP. I was very involved in that activity, working as a director of AEA, which represented four thousand electronics computer-instrument companies.

We wrote a large part of leaking underground storage tank legislation (LUST)—that whole set of regulations that governed the storage and disposal of various chemicals that are used in the semiconductor industry, gasoline stations, et cetera. AEA helped write the regulations that were first adopted by the counties here in the Bay Area, then by the State of California, and almost without change by EPA as part of resource conservation and recovery at CRCRA.

BROCK: What year or years was this?

FINNIGAN: It was in the early 1980s. I was involved with AEA from 1980 through 1987.

[END OF TAPE, SIDE 12]

FINNIGAN: The AEA job was somewhat compatible with Finnigan in that we were both trying to do the same thing: clean up the environment. In the same period, I was co-founder and chairman of the OIML, Organization Internationale Metrology Legale. It's legal metrology. In the U.S., it was an activity that was sponsored and guided by the National Bureau of Standards [NBS] (now National Institute of Standards and Technology). We had coordination by NBS. We wound up writing the protocols—proposing and ultimately getting passed regulations, internationally, for fifty-one nations in the world. They were really the EPA regulations, if you really get right down to it. In instrumentation, in particular, we wound up proposing to fifty-one nations that, for instance, they use GC and GC-MS for organic pollutant analysis—IR [infrared], other methods, and UV [ultra-violet] for other measurements. We wound up getting all the nations in the western world to adopt the same standards as we had in the U.S.

BROCK: Right.

FINNIGAN: We didn't put it that way, but it was the way it worked out. I spent seven years, I think, as chairman of that group, making trips to Europe and various places to get other countries to go along with what we were doing here.

BROCK: Well, your interactions with the EPA and working hard to create this environmental market through GC-MS—was environmental science something that you had had exposure to prior to that? Did that experience change or develop your thoughts about the environment or things of that nature? I mean, you were heavy into this early EPA world pretty intensely.

FINNIGAN: Yes, it did. For instance, at AEA, my job was to convince other companies—electronics companies, semi-conductor companies—to become good citizens. Some of them, like Hewlett-Packard and IBM, were already good citizens and were helping and providing people in every way they could—helping to clean up the Valley or wherever they happened to be. Both those companies were included among those that were contaminating the environment, but were not aware of it. But once they became aware of it—using modern instrumentation, they could become aware of it—they were eager to correct the damage.

I would say, wearing the AEA hat, I was not interested *per se* in trying to sell anybody instruments. I was very interested in helping them stop polluting. I had already worked with a lot of petrochemical companies who generally had a good conscience—Monsanto would be a good example—a real leader in being a good citizen and showing leadership in their industry in cleaning up the environment and stopping the pollution. So I was really imbued with doing that with our companies in AEA.

We established regional AEA offices that we would visit and try to get across the same spirit of "let's clean up our act." We did a pretty good job with it. We often had EPA administrators, local or national, as guests to help get the message through. I think AEA wound up really doing that job very well. My own sort of goals were to help as much as I could in that area and helping to write regulations for EPA that didn't have any bearing, at least any direct bearing on instruments we sold, but wound up with companies using double wall tanks that wouldn't pollute the drinking water in this valley or any other valley. It was fun to do that.

BROCK: Yes. Let me see. There was a big acquisition by Finnigan of MAT [GmbH] in 1981. Was it in 1982 that you had a position change for yourself personally?

FINNIGAN: Not really. I think I was always doing the same thing, basically.

BROCK: Right.

FINNIGAN: Whatever it was called—I held every job in the company at one time or another. But we acquired MAT in 1981 from Varian. They were a real drag on us. I mean, in terms of not doing well financially. It was a real burden for us. We had been a company with profits going up. MAT had a huge loss at the time we bought them and they continued to have it for a number of years. In fact, I don't think they ever made a profit before Thermo [Instrument Systems, Inc.] bought Finnigan.

BROCK: All right.

FINNIGAN: And it probably had a lot to do with the ultimate financial outcome for Finnigan that we couldn't show a good profit after we acquired MAT. They were a very good technical group. Today they're making a good profit, probably own, by far, the lion's share of the isotope ratio mass-spec market, and are doing very well. But during the period we had them, VG [Elemental] was a major competitor to them. And VG was eating their lunch—almost all the salesmen from Varian joined VG before we bought MAT or about the time we bought them.

So that was a huge drag on the company in terms of management time, trying to extend our culture to them and have them become more flexible—very often the German way is different than the U.S. way in terms of entrepreneurism, and so on. That was a difficult period. In that period we brought in John Hearn as the vice president first of manufacturing, and then vice president of operations for our San Jose operations. Then, somewhere in the mid-1980s we made John VP of operations for both Bremen and San Jose.

T. Z. gave up some of his own general manager role to John. For a while that worked out. Then, as I said, it subsequently didn't come out so well. I would say that we inherited a lot of problems. I think as opposed to having a straightforward goal of dominating the environmental market, which we did—at the time we bought Bremen we probably owned 85 percent of the environmental market; we now had many other MS goals. I would say by 1990, when we sold to Thermo, we had a lot less market share, and probably were at the same level as HP. Even though our horizons included magnetic and isotope ratio, those were markets dominated at that time by VG, both in terms of sales and profits. My own role, I would say, didn't change all that much. I had various titles.

BROCK: I guess, 1982 there was a title change.

FINNIGAN: Yes. I can't remember all the times, but I was president of Finnigan instruments division at one point. I was president or chairman of the board of international operations later on. Essentially, I was doing the same job that I always did. I was the chief strategic officer for the company from the beginning until the end. I shared that job sometimes. I shared it with Roger Sant at times, with T. Z. at times. But ultimately I was the person accountable for our strategic vision. T. Z. was chief executive officer from the day he joined us until we were acquired. We had various other people in other roles. We had titles that varied along the way.

BROCK: Right. Was the acquisition by Thermo in 1990? Was that sort of the end of your real active involvement with the company?

FINNIGAN: So intensely, yes. At that point I was working for Arvin Smith, who was the president of Thermo Instruments, which was the acquiring company. I did work some for George [N.] Hatsopolous, the president of Thermo Electron [Corporation], who was also CEO of Thermo Electron. In terms of day-to-day, it did sort of terminate my huge involvement day-to-day, minute-to-minute. But I stayed involved, I think, for about a year. I never formally retired—haven't yet. I actually defined a job for me as a consultant for Thermo Instruments and Finnigan in 1991. I think that probably took effect around January 1992. I worked basically one day a week for Thermo, from that time on for Arvin, and some of it with Finnigan, but not all that much.

So my involvement was usually more on a individual consulting basis. For instance, the person who is currently president, Ian Jardine, is a person I helped to bring in. He was marketing manager for some time. We would argue some days—I mean we would discuss marketing issues at some length. I would get involved in certain things, but try not to interfere in the operations of the company. Rick [Richard W. K.] Chapman was brought in as president of the Finnigan subsidiary, which became ThermoQuest [Corporation] for a while, and then was changed back to Thermo Finnigan. I was there occasionally. I moved my office into the middle of R&D, basically at the invitation of the R&D people. That was where I sat when I went into the company. I interacted with them. I tried not to get involved in the operational end of things.

BROCK: Right. That consulting that you were doing for Thermo Instruments and Thermo Electron, was that in the vein of strategic consulting for them?

FINNIGAN: Yes. I was on things—like they had a detector for explosives. It used chemiluminescent detection. Thermo Detection, they call it now—I think it was called Thermo Detection then. They (Thermo Detection) were predicting billion-dollar markets for this instrument. The president of the company, George Hatsopolous, asked me if I would look at it independently. I think they were predicting fifteen hundred units a year of this product, which sold for about two hundred thousand dollars. I spent about two months looking at that market and visiting some of the people, doing a mini-survey. I came in with a number of fifteen units over three years, whereas they were talking probably five thousand units over three years. They actually sold twelve.

George was looking at whether he should make a huge investment. I think he did not make that investment, but they did continue to develop the product, but no one ever bought it. So I helped out on things like that, some of them in-house and some of them involving companies that they were looking at or that Arvin was considering buying. I was very peripheral and not really challenged all that much. They were not here, and I was not interested in moving. Hatsopolous asked me to consider moving to Boston or the East Coast and take over a part of his job that he actually enjoyed very much but didn't have time to do anymore—and that was sort of working with politicians in Washington and working on new product kind of things, helping figure out where to build it at Thermo. A lot of it was just plain rubbing elbows with the politicians. In 1988, I had really done a lot of that representing AEA. I had, I think, visited a number of senators and many congressmen—one of them, Norm [Norman Y.] Maneta, was a very good friend, who was writing the legislation for water regulation. We were basically providing him, from AEA, much of the material that he ultimately incorporated into the EPA legislation. So, Hatsopolous wanted me to consider doing that sort of work for Thermo and work directly for him, but I didn't want to move. I mean, I just knew it wasn't in the cards for me.

BROCK: The early 1990s—was that the period of time or did it even begin earlier—when you began to sit on the boards of other companies and continue entrepreneurial stuff? I don't know how to describe it.

FINNIGAN: You did very well. I went to Hambrecht & Quist, the investment banker, starting in 1988. They were starting up an Environmental Technology Fund and asked me to be an advisor to it, which was quite an honor. They were also our investment banker at Finnigan, so there was a potential conflict. I basically asked our board if I could do it, if the board approved of it. I said, "If there's a single dissenting vote, I'm not going to do it." There wasn't. So I did that starting in 1988 through 2000. I worked with various people at H&Q in San Francisco. I was asked onto various boards, and some of them, like Dionex [Corporation], I turned down because it was a potential competitor to Thermo. If it was anything competitive with Finnigan, I didn't want any part of it.

BROCK: Right.

FINNIGAN: Whether I was going to be active at Finnigan or not, I was not interested in working for a competitor. So I went on the board of small companies that were entrepreneurial where I thought my skills maybe were more *a propos*, you know. I think I was on five boards at the peak. I'm on two right now, and one of them is here in the Valley. It's a group from the Stanford University chemistry department. It's a fun thing.

BROCK: Have you enjoyed serving on these boards?

FINNIGAN: Yes. Where there are really good people it's really enjoyable. Some of them are not as entrepreneurial as others. East Coast people—I don't want to generalize—but the board I'm on back there—Strategic Diagnostics [Inc.] is the name of the company—are more conservative. I've been very helpful to them, because I'm such a radical by comparison to other board members. They were originally doing immunoassay analysis of pollutants in water and soil, an inexpensive test, and currently they are doing a lot of analysis for GMOs [genetically modified organism] in corn, soybean, et cetera, using an immunoassay test.

Occasionally, my experience with EPA has been helpful to them. I introduced them to people at EPA, who were the right people to work with, and I could also help them to avoid some of the pitfalls that we have encountered. I would say it's been fun for the most part. The people on the boards have been real fun guys, like Ricardo Pigluicci and Dave [David R.] Nelson, who's getting the instrumentation hall of fame award this year.

BROCK: Right.

FINNIGAN: So over all that's been fun. We sold one of the companies I served on the board of to Hewlett-Packard. They were making a portable process GC. I think, ultimately, they were not going very far on their own, so we sold the company at a good price to Hewlett-Packard (Agilent Technologies), who I think are doing well with it.

BROCK: Well, I believe I have come to the end of my long list of questions. So if there's anything that you have the energy left to mention that I didn't ask or any other thoughts you wanted to include, we can talk about those now.

FINNIGAN: Yes. One thing that might be of interest—the whole concept of being acquired. When you have a company you've grown from zero to say one hundred thirty million and with prospects of going further, when do you give it up?

BROCK: Yes. That's right. I didn't ask about that whole sequence leading up to 1990.

FINNIGAN: I think that we were running more and more into a situation where we saw a lot of companies in the analytical instrument business being acquired—Thermo doing a lot of it—and where it was becoming harder and harder to compete when you were a one-product company. We were basically a one-product company. So your margin for error becomes less and less and as you become bigger, you need a different management than when you were smaller.

For instance, I think the management that we had was very capable of taking the company to a hundred million dollars or maybe a little over, but we needed a different type of experience to take the company from a hundred million to five hundred million or a billion. It was a pretty big step and the stakes got higher the whole time. You can't make very many mistakes and survive. I felt that we needed partnerships and it might be an acquisition—and we needed some person, I won't say a Jack [John F.] Welch, but somebody who was a bigger dimension than either T. Z. or I, to take it the next step. T. Z. may or may not have agreed with me. I think he agreed that we needed somebody but he didn't feel the same urgency that I did. In such areas, you never know whether you're right or wrong.

The value currently assigned to Thermo Finnigan is many times what it was when we sold it to Thermo. It's hard to say whether we would have done better or would have done worse. I don't know. But, I guess, my feeling was we needed a whole new management structure and person to make that change. So that was my reason for feeling that it was time to either be acquired or find that outstanding person to lead us. But whether you can do it with one product is hard to say. I just don't know if anyone could ever say for sure. But there's been a lot of consolidation in the industry.

BROCK: Sure has.

FINNIGAN: Not just our industry, but certainly in general. And profits are the name of the game. The first thing Thermo did, when they acquired it, was cut back on research from 12 percent to 6 percent, or something like that, which gave an immediate bounce to the profits, but in the long term, it didn't help. So products, for instance, like the triple quad suffered. At the time I left the company we owned 90 percent of the market for triple-stage quadrupoles—MS-MS [multi-stage mass spectrometry]. Today Finnigan owns about 5 percent of that market, which is huge. That turned out to be an opening for PerkinElmer/MDS Sciex, and Waters [Corporation]/Micromass [Inc., a subsidiary of Waters Corporation] who came in and ate Finnigan's lunch. When we left, it was our most profitable product. Some things have not done so well. But the whole thing of "when do you call it quits" is sort of a hard thing to sense—

BROCK: Yes. To get some distance on that decision and be somewhat analytic about that—that must be very difficult, too, because you've really poured your heart and soul into this thing and it's been your—

FINNIGAN: Your baby. But I have to say, in all honesty—and maybe it's a good place to end—that I've had more fun in the last eleven years than I've ever had, because working with small companies I've been able to help a lot. I'm good at that, and, I think, I'm helpful to the companies. I've had enough financial freedom and investment success to be able to do whatever I want to do and I get to spend more time with my wife and family doing things we want to do. So, I think there comes a time when it's better to turn it over to somebody else. I wanted to turn it over to Ian Jardine, at the time T. Z. and I were leaving. I spent a lot of hours with Arvin Smith suggesting that Ian, who had a good scientific background, a Ph.D. chemist who worked at the Mayo Clinic before coming to Finnigan, was the right choice.

[END OF TAPE, SIDE 13]

FINNIGAN: Arvin felt that he didn't have enough business experience. We had several excellent people that were business types—Dave Hoyt, at Stanford Business, worked with us for many years at Finnigan in various business roles, and our chief financial officer, Steve [Steven] Wade. Both were willing to stay on in a business role serving someone like Ian. I argued for that, but Arvin didn't see fit to go that way.

Instead, he put somebody in charge that was a Ph.D. chemist, but was not creative. He was a person who was motivated to make the most profit, which probably was what Arvin wanted at that point, but he gave much of the futures of the company in the process. Then ultimately, ten years later, the new management at Thermo chose Ian to take over.

He's doing a terrific job. I'm more active today, than I was a couple years ago, in that we talk frequently. I go in to visit with him and others every month or two, I'm pretty aware of everything going on, welcomed into all parts of the company again, and feel like I'm still part of the company. I don't know that I have that much to contribute, but I actually helped close an order for Finnigan for a million dollars within the last month at a local university and for a very large system. It was the marine sciences division of a nearby university. Anyway, when you see somebody like Ian doing it the way you would do it or even better, that's a good feeling.

BROCK: Yes.

FINNIGAN: Then you say, "Well, I feel good about that." I can be excited about what Thermo Finnigan is doing today, but there was a period of ten years where I wasn't at all excited about what they were doing. So the acquisition by Thermo for a lot of the people that I helped bring in the company meant a loss. They were attached to either me personally, T. Z., both of us, Mike Story, or whomever. Many saw the change as a big loss. When I'd go in there, I would get a crowd gathering around me pretty quickly, you know. I'd be asked to go speak when somebody was retiring, leaving, or whatever, and I always did it without any exception. Often I would wind up with a lot of the people imploring me to return. I would tell them, "No way. I'm having too good a time outside." But if Ian had been selected as CEO, when I had suggested it in 1991, I think, I probably would have been a lot more active there in the interim ten to twelve years. But, you know, things work out the way they work out.

BROCK: Yes. Well, now it must be nice for you to feel that it's on good footing and in good hands.

FINNIGAN: My wife tells me that I shouldn't worry. We had a going away party for Mike Story about two months ago. It was a big party that Ian held at a place in downtown San Jose. He had invited many people, including consultants to the company, people like Jon Amy and Graham Cooks from Purdue and Rick [Richard A.] Yost from the University of Florida—people, whom Mike and I had both worked with over the years, some of them more than twenty or twenty-five years. T. Z. and I were there, and some of the other people who were at Finnigan earlier in the company's life. We had a very nice party. We talked after dinner. We all got up and had a few words to say—it was quite a good feeling. It was sort of the feeling like Finnigan of old.

T. Z. made the statement he was happy to see they re-named the company Finnigan. I would say we all felt a lot better about the future of the company with Ian there in charge, a very creative person, who knows the history, is proud of the history of the company, and wants to keep the personality and the culture that was Finnigan, as part of Thermo Finnigan.

BROCK: Right.

FINNIGAN: Which, I think, is the way it should be done. And it's probably the way that it's going to be most successful too, hopefully.

BROCK: Well, thank you so much.

[END OF TAPE, SIDE 14]

[END OF INTERVIEW]

NOTES

1. Jonathan Swift, *Gulliver's Travels*, edited with an introduction and notes by Paul Turner (New York: Oxford University Press, 1998).

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8. Robert E. Finnigan, P. Michael Uthe, Jr., and Allan E. Lee, "Modern Process Control," *International Science and Technology* (April, 1963).

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9. See Note 6.

10. See Note 6.

11. Maynard B. Neher, *Summary Report on Evaluation of Gas Chromatography/Mass Spectrometer/Computer Systems to Water Quality Office, EPA* (Columbus, Ohio: Battele Columbus Laboratories, 1971).

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13. Robert E. Finnigan, David W. Hoyt, and David E. Smith, "Priority Pollutants II – Cost Effective Analysis," *Environmental Science and Technology* 13 (1979): 534-541.

INDEX

Z