DAVID H. BRANDIN INTERVIEW: AUGUST 2, 3, 4, 2008
LOS ALTOS, CA

Session 1, Tape 1, Side A

BRANDIN: Do you want me to do this like a deposition, where I answer the question I'm asked, or do you want me to ramble?

NORBERG: No, whatever comes to mind; this is an association game, really, rather than a deposition. I would like you to start with some general information from you about your parents, your education from grammar school on up, and something about your interests while you were, say, a teenager. One of the things I've found over the years is that a number of the electronic people back in the 1920s and '30s were ham operators, and that comes through very clearly in a whole series of interviews.

BRANDIN: David Henry Brandin, born January 27th, 1939—Mozart's birthday, by the way—in Chicago. Parents were Max and Libby Brandin, and Jewish. Max was a laundry truck driver/route foreman/teamster. Libby, at the time I was born, wasn't working, but prior to that, she had been an organizer for the International Lady Garment Workers Union, and I suspect—I believe—that she used to carry a card in the Communist Party in the '30s, and quit the party in the year they assassinated Trotsky, which I think was '43, because she was more a Trotskyite than she was a Stalinist. This led to interesting issues when I got my first security clearances later on. And then after I was older, she worked in a factory in Chicago part time. So I grew up in a neighborhood in Chicago called Humboldt Park, which is the near northwest, sort of, of Chicago. I went to an elementary school called Lafayette, and then went to Roosevelt High School, which is in a neighborhood in Chicago that was called Albany Park, about four miles from my house.

NORBERG: What was the program like in that high school?

BRANDIN: Well, to start off, Roosevelt, the year I started, won the Chicago basketball championship, and then never won another anything. They had a very, I would say they had a very good program in terms of education in the sciences. I don't know how good they were in the liberal arts, but I took the standard curriculum, which I think is two or three years of English, four years of math, four years of science. I took the science because on the first day of high school, the teacher asked for a volunteer in a physical sciences class. I went up; she poured water in one hand and alcohol in the other hand and asked me which hand is cooler, at which point I learned that evaporation was a cooling process, and I decided right then and there, I liked science. Actually, at that point, I wanted physics. They had a very good science program, and the math program was pretty good. I got lousy grades throughout high school, but I took all the math.

NORBERG: What does "lousy" mean?

BRANDIN: Oh, C—the equivalent of C's. In those days, we used S's and E's, G's—things like that. In fact, I almost didn't graduate from high school because the college algebra teacher said I

was flunking, and I had to stay after school for six weeks to do extra work, and then I graduated and went on to major in math, which was sort of interesting. We just celebrated our 50th anniversary of the class of January '57.

Now in '56, I had joined the Illinois Air National Guard with a bunch of other high school buddies, and I was a jet engine mechanic. This was to beat the draft, of course. And one of the other fellows and I had applied to the Air Force to go attend jet engine school upon graduation from high school. The jet engine school was regular Air Force training, and it was in Texas. Six weeks before we graduated from high school, the Air Force called and said, "Sorry, but the classes are filled up with regular Air Force people, so you can't take this course. You can either wait for six months or cancel." So my father looked at me and said, "Well, you can either go to work or go to school or get out." I said, "I'll go to school," at which point, I went to a junior college for two years in Chicago, Wright Junior College, which was known for having Kim Novak as a graduate. Wright Junior College had an excellent chemistry department and an excellent set of math teachers. I took two years of math there; I took a year of physics; I took a year of chemistry. I was president of the chemistry club. I don't understand how that happened, but I worked in the physical science department. I was the acid man; I poured acids and made up unknowns. I originally wanted to major in physics, but in one of the physics classes, I had a professors—Hedenberg, I think his name was, like Heisenberg, but not quite—and he said, "Brandin, every class has its horse's ass, and you're ours. If I give you a B, they'll think I'm crazy at IIT," because I was transferring to IIT, and he gave me a B anyway. But when I heard that--

And you know, the physics, the physics at Wright wasn't that sophisticated because we hadn't done calculus, it was still kind of analytical geometry-driven physics. But the physics itself was hard. I found the electricity and magnetism really difficult, so I decided, oh, I'll switch to math; I'll major in math. So when I transferred to IIT, at the end of two years at Wright, I became a math major, and the math classes, they were easy. I was way ahead of the IIT students because Wright had had a very good algebra, advanced algebra, college algebra, analytical geometry, functional analysis—really good basic underpinning of math. So by the time I got to IIT, I was way ahead in math, and I majored in math and I took more math classes in undergraduate school than most masters do in a full master's program. I probably graduated with somewhere around a C+ grade point average, because I got lousy grades in everything else. But I did minor in physics, and took a lot of English and a lot of psychology as well.

I graduated from IIT in January of '62. It took me five years to get the bachelor's degree; I and worked full time in the process and got married in 1960 to Ellen. So Ellen and I have been married now for almost 47 years. She had also gone to Roosevelt High, but was a semester ahead of me in high school, and we did not date in high school. So we knew each other, but dating started after high school. I was in the Air National Guard until June of '62. I put in my six years, completed my military obligation, and had no further responsibilities. I really didn't have to do that, because with my math degree and then being married and then having a baby, and then also working in national defense, I never would have been drafted in the first place.

NORBERG: Yes, I wasn't either, for the same reason.

BRANDIN: But the Guard was actually a lot of fun, and I had a good time at the Guard. At the time my six years were up, and I was getting my degree. I was asked if I would consider reenlisting. I had three stripes at the time. They would send me to navigator's school because I had a degree in math. I wore glasses, so I couldn't be a pilot. They would give me another stripe

while I was in officer's training and all that stuff. But they wanted me to reenlist for six years, and I thought that was a non-starter, so I didn't reenlist, and I just got out.

In '59, when I was at IIT, in my first year majoring in math, I insisted with the chairman of the math department—a very nice guy named Hiam Reingold, a very good mathematician—to take a two-hour programming class. He didn't want me to take programming; he wanted me to take more and more math. But I insisted on taking the two-hour programming class, which was an introduction to the 650, taught by some IBM guy. At the time, IIT did not have a computer, so the class was strictly paperwork. And I learned how the 650 operated the fundamentals of the machines. The programming language at the time was called SOAP—symbolic optimum assembly package. I think Bell Labs had developed it. But we learned machine language and all that stuff, and the 650 was a 2000-word rotating drum machine. We used Dan McCracken's book. So let's see. Where were we?

NORBERG: I'm going to ask you a couple questions. Dropping back a little bit, what was the attraction of IIT at the time to you?

BRANDIN: Well, it was an engineering school, and I had a cousin who was kind of a role model six months ahead of me who was at IIT majoring in nuclear physics. And it had a math department. I felt that I couldn't get into Northwestern. I felt there would be Jewish problems with Northwestern. And I thought the University of Chicago was too tough. Now later, after I completed my junior year worth of hours; I tried to get into UC, but they wrote back and said that they wouldn't take anybody who was only going to take one year of classes for a degree.

So there I am; it's '59; I'm at IIT taking classes. My cousin is there, and most of the classes I'm taking, he's in as well, because we're taking a lot of math—everything, complex variables, differential equations, partial differential equations, Laplace transforms—you name it, we were taking it. Game theory, analysis, and so forth. He had a job working at what at the time was called Armor Research Foundation, which is now the IIT research institute, which is to IIT the way Stanford Research Institute was to Stanford. He got me an interview with a physicist over there in the solid-state physics department called Bob Pohl at the time. He changed his name to Stewart and became an active guy in the IEEE computer society—a little nuts, too, but... Anyway, Bob was doing work in super-powered cathodes for the Air Force, and I became an assistant technician working for Bob.

While I went to school, I was working at IITRI or Armor Research, and Armor, in 1960, got a UNIVAC 1105, which was one of three manufactured for the 1960 census. It was government money. And then there was some other guy in the physics department, solid-state physics group who was doing a thesis, and he needed some computations. They had a programming language for the 1105, I think it was BASIC or something like BASIC, ALGOL-ish like. And I heard about it; I volunteered to write the program. So I did the computations for him on the 1105. And that was the first operational, real program I ever ran, on the UNIVAC 1105 in this little algebraic language. I did some other math for them; I derived an integral in complex space on that, and after I had derived it, we found it in the tables. But they needed it for some paper. So I worked in solid-state physics; then I worked at optics as a technician. I was an assistant. I should go through all the titles. Between ITRI and SRI, I went from assistant technician, technician, technical assistant, assistant mathematician, associate mathematician, research mathematician, senior research mathematician, department head, laboratory director, executive director, to vice president. I went through every single title in those two organizations.

NORBERG: It sounds to me like IIT was a fairly large institution at that time.

BRANDIN: Well, I think the undergraduate school probably had about, I don't know, 6,000, 8,000 students. And this was 1960. The campus still was an oasis in the middle of a really impoverished jungle. I mean, even the police dogs didn't want to come out of the guard shacks at night. It was really dangerous. But Chicago was in the process of completely renovating everything between IIT and the lake. It sits about four miles south of the loop, and about a mile west of the lake.

NORBERG: Pretty close to downtown.

BRANDIN: Yes, it wasn't that far. But at the time, after we got married, I lived on the north side of Chicago, so it was a commute. Let's see. I was going to IIT, and so I graduated with a bachelor's in math. I was working in the solid state and optics department. One of the guys I worked for in optics, most of the projects were military, and one of the guys I worked for was an Indian...not a Hindu, but the guys who wear the turbans.

NORBERG: Sikh?

BRANDIN: Sikh. Narinder S. Kapany. And Kapany's name was very well known in fiber optics. In fact, he gave a major contribution to the Asian Art Museum of San Francisco, where my wife is a docent. Kapany had some interesting work we were doing in satellite optics. So you can imagine who was paying for the research. And when he went out and formed his own startup company, Optics Technology, I ended up driving his car to California. My cousin drove one of the other guy's cars, so we drove out in tandem.

While I was working for Kapany, ITTRI had a little computer department, and they had the 1105, and I went up, and I told these guys that I was getting a degree in math. I took a senior-level math class, which was scientific programming. It was called scientific programming, but it was numerical analysis and programming in FORTRAN. And I said, "I'm interested in becoming a programmer." I transferred into their department, and when I got my degree, they made me an associate mathematician. I started programming, and my first application was for an Air Force project out of Wright-Patterson Air Force Base on simulating six-degree-of-freedom satellite interceptors. The idea was that the vehicles were sitting in orbit, and when a platform would detect an ICBM launch, they would launch an interceptor that would come down out of orbit and get these guys as they were coming up into the exo-atmospheric region. The Air Force at Wright-Patterson at the time had a 709, which was a hell of a great machine. ITTRI still only had an 1105. So I wrote the program in machine language; well, SAP I think it was, Assembly Language. And we thought would go down to St. Louis to McDonnell-Douglas where we could buy time on the Air Force's bill from McDonnell-Douglas to use their 7090. And there are all kinds of stories related to those trips, but the idea was that we would debug the program on the 7090. And the big, technical, complex problem I had at the time was, I didn't have a good numerical integration program, and we were simulating simultaneous parallel linear systems. We were simulating all six degrees of freedom—the control systems, the trackers, pitch roll and yaw, along with integrating this sucker in inertial space. So I borrowed a numerical integration package from McDonnell-Douglas, and we took that, and went to Wright-Patterson. The first time I tried to run the program, we couldn't even get it to load. We went home with our tails between our legs. This was a JCL-equivalent problem—job-control

language problems that were strictly unrelated to the program, but we just couldn't get past the operating system, which at the time was called SOS, the Share Operating System. We fixed that problem, went back to Wright-Patterson, got the program running, and eventually, according to a former data automation officer there, we became the single largest user of computer time at Wright-Patterson Air Force Base.

NORBERG: Around 1960, '62?

BRANDIN: Well, '60s. In fact, one night, I remember the Chinese were bombing Kimoy and Matsu with artillery, and we arrived at Wright-Pat too late to get a car pass. The car passes were pieces of white cardboard, and so we took a piece of toilet paper and stuck it on the windshield, and drove it right onto the base. I was waved right past the guard shack! Yes, we would generate reams and reams of paper—I mean piles ten feet high sometimes.

NORBERG: Yes, I remember those.

BRANDIN: We were only simulating six seconds of real flight time, but it would take six hours of computer time on a 7090 dedicated machine. And we would operate the machine ourselves. In those days, the price was \$600 an hour during the midnight shift for CPU time—well, for the machine. The application was a very popular program. The Air Force used it; the Navy used it; Hughes used it; LTV used it. I have no idea who else. It was used in studying tracker behavior, vehicle performance because we could simulate basically everything. The navigation law was fixed; it was biased-proportional navigation, but you could change time constants and screw around with the mass and the engines. The warheads, we did a lot of warhead simulation.

NORBERG: So who was funding this?

BRANDIN: Mostly the Air Force. But the money came primarily from a guy who was interested in trackers and navigation. That was his part.

Now one interesting thing we did at the time was, there was a hardware system called Stromberg-Carlson 4020, which was a CRT camera system, which took CRT pictures high-speed. The picture taken generated a screen; and a high speed camera would photograph the display, like with a Polaroid, but fast. We didn't have an SC-4020 at ITTRI, but we did have a UNIVAC 1105 and a CRT-- a Hughes memoscope. You could put the picture on the screen and it would stay there; you didn't have to keep refreshing it. So we built a simulator of the SC-4020, on the UNIVAC 1105 and Hughes memoscope. And then we had to debug it. The drivers had to run on the 7090. So here we were, basically building a hybrid system in like '64. And we did it. They had a 4020 at Wright-Patterson, so we did all the debugging at ITTRI, and then we eventually ran the thing on the Air Force computer, and generated thousands of photographs of, you know, missile performance and stuff like that. So that was through almost '65 or '66. We eventually converted the program to FORTRAN-II, and then eventually converted the program to FORTRAN-IV. I'll give you an interesting technical problem we had, and then I'll go back to some teaching. In '64, IBM released the 360 series, I think, and Naval Ordnance Labs in Corona had a 360/40. The IBM 7090 was a 36-bit machine. The 360/40 was a 32-bit, four-byte, word. So I took this six-degree-of-freedom monster from 36 bits to 32 bits. This vehicle was flying at 22 million feet above the center of the Earth. The equations of motion were simple and based on the center of the Earth— $F=MA$ stuff. After we converted this thing to the single precision on the

360/40, we ran it and discovered a weird universal constant, at 22,970,000 feet. At that position, the vehicle decided to stop moving, but it accelerated, and its velocity increased, but it didn't change position. And it took forever to finally find out that we had a round-off error that was greater than the truncation error in the algorithms.

NORBERG: I've done that.

BRANDIN: The only solution was to go to double-precision numerical integration. So we changed just the integration to double-precision numerical integration to solve the problem. That was one of my first really interesting computational problems. I mean, there were all kinds of other stupid diagnostic debugging problems a little further back. But the UNIVAC 1105 is a paper tape machine, and it had 90 column cards—a very strange machine. And with the 709, once you got into the 709, 7090, 7094, 7094 mod II, 7094 mod II-7040, you had very-- these were really nice machines, and we would program them in cards, and you would sit at the keypunch and program at the keypunch. You didn't even bother programming, you know. In the old days with the SOS decks, the object decks, which were called squeeze decks, you could patch those cards blindfolded, once you learned the format. So sometime around '64, IIT got a 360/40, I think. It might have had a 650, too, for a while. Peter Lycos, who was the Chairman of the Chemistry Department was the guy who got IIT into computing, not the math department. I had taken a three-hour senior-level math programming and scientific computation analysis class, and Peter came to me and asked me if I would be willing to teach a three-hour course basically in FORTRAN and numerical analysis. I don't even remember whose books were out at the time, but we probably used all of them. And I said, "Sure." So I started teaching in I think at the time, it was the Industrial Engineering Department. No, I guess he would have called it Information Science.

I started teaching the scientific programming class, and it was an introduction to computing. I would teach basically how the machine works. I would teach assembly language on a theoretical machine. It was like using McCracken's book, like TYDAC, the typical digital automatic computer. And I would use that; I would create my own structures, and I would make them write little assemblers in FORTRAN and stuff like that. And then I got into numerical analysis and computational analysis and all the stuff that goes with numerical integration—telescoping series and stuff like that. I taught a three-hour class in information science, and when the industrial engineering people asked me to teach the class, I taught the class. It was basically the same class. The Math Department asked me to teach the class; they wanted a two-hour class. I basically taught the same class, but just didn't get as far into the analysis. And then Lycos came to me and asked me-- I'm going to have to tell you about graduate school.

After I got my degree and I was working at ITTRI full time, I decided to take some graduate classes. So I started taking graduate math classes. I could take senior-level classes for graduate credit, and graduate level classes. One semester I took 12 hours of math. I took mathematical statistics, which was undergraduate, and something else, special functions, and modern calculus. Modern calculus was taught by Karl Menger, who was a quite well known mathematician. Well, I got C's in the modern calculus and special functions class. The special functions class was the hardest class I had ever taken in my life, and I was a lousy student. I was working full-time, and I was lazy. I got A's in the math statistics and the other class. So I got disillusioned about going to graduate school, and that was about the time my wife said she wanted a baby, so I never went back to graduate school.

But at '66 or so, and Lycos asks me to teach a graduate class in numerical analysis. Now, I'm pretty good at computational analysis, but to me, the difference between computational analysis and numerical analysis is, numerical is theoretical. Computational analysis is practical. I said to Peter, "You know, I never took a class in numerical analysis, and to me, it's the little deltas; it's not the big deltas." He says, "Yes, there's no difference; just go teach the class." So I ended up teaching a graduate class in numerical analysis, and used the same material, except instead of starting with introduction to machines, I started with the computational analysis and just went further. It was a two-hour class.

NORBERG: How old was Peter at that time?

BRANDIN: It was the '60s. I was in my mid- to late-20s, so Peter, I would say, was around 40.

NORBERG: He contacted us when the Babbage opened and wanted to not so much donate papers, but to be involved somehow.

BRANDIN: Well, as I say, he's the guy who got them into computing. So it's '67 now, and I might have been taking some extension classes at UCLA on the simulation of aerospace vehicles from Barry Boehm. I took a computer graphics extension class. And I also went to Princeton—not the Princeton U, but the Princeton Electronic Associates, Inc., EAI, was teaching classes on hybrid simulation and analogue simulation. And EAI offered me a job in their Palo Alto office, basically in hybrid simulation and teaching classes on hybrid simulation.

NORBERG: To whom?

BRANDIN: To their customers. And one of their major customers was Martin in Denver, building the Titan 3-C. They had a big EAI 8900 system, which was a combination of the EIA 8900 analog machine, which had 100 integrators, and the EAI 8400, which was EAI's version of the IBM 7090. It was a 32-bit word. It was a nice, little machine, too. Anyway, I took the job, and we moved to California. We were living up in Belmont, which is near San Mateo, and I was working in Palo Alto, and I was spending almost all of my time in Denver. I think at the time--well, I know I was in Denver at the time of the '67 war in Israel, and there was an earthquake that day in Denver.

So I'm working on the EAI 8900. I've taken all the EAI courses on how to program analog machines and build flip-flop circuits and all that stuff. Even though I thought of myself as a software man, I was really beginning to understand a lot of the analog hardware issues. I liked to program an analog machine and wire up a board. I never did learn, by the way, how to wire up the 80-80 board on the IBM 407 printer. Actually, it wasn't called an 8080 printer; it was a line printer, but the board, when it printed a card, an 80-column card was called an 80-80 board. So the first 80-80s were not microprocessors. And then one day, I was walking up the steps at Martin, and bounced off the chest of Werner Von Braun. I thought that was kind of cute. But I don't know, I wasn't ready to move to California or leave Chicago, or I didn't like the job, and their headquarters here on the West Coast were in El Segundo, so I was in El Segundo a lot; I was up here; I was at Martin in Denver.

So I called up IITRI and said, "Hey, I want my old job back." They said, "Come back." So we moved back after something like eight months in California. This was the end of '67. I went back to IITRI. This time, I went back as a senior research mathematician, and they

assigned me to the APT long-range program—automatic program tools, long-range program—which IITRI had inherited from MIT. It was funded by the Aerospace Industries Association or something like that. Do you know anything about APT?

NORBERG: No.

BRANDIN: Do you want to know?

NORBERG: Yes. [Chuckles] As long as it's relevant.

BRANDIN: Well, it's an interesting technology. It's numerically controlled machine tool technology. So in the '50s, the Air Force funded, and MIT developed, the original APT, Automatic Program Tools.

NORBERG: Is that the thing that Doug Ross worked on at MIT?

BRANDIN: I believe so.

NORBERG: Okay. Then I do know something about it.

BRANDIN: IITRI had that program when I was there earlier, but I didn't work on it. It allowed you to take the design of a part and program it in an algebraic language, and then APT, the software processor, would run on a big machine and generate a control tape for a numerically controlled machine tool. And that control tape would machine the part. So instead of having to inventory an engine block, Detroit Diesel just had to inventory the paper tape. It became very significant during the Korean War. It really made a big difference in manufacturing costs during the Korean War. So IITRI, in a competitive bid, won the project, and was being funded by the Aerospace Association. And a lot of corporations were pumping money and paying—GM, Boeing, Ford, Northrop—everybody was in the program. We even allowed the Europeans and the Asians in with a two-year technology lag.

So I came back to Chicago end of '67, and in 1968 I was working on this program, and I was working in the-- I was doing technical work; I was primarily in the arithmetic element unit of APT. I was doing maintenance. The main thing I remember was a program called Tinker, which tilted the tool axis. It allowed for a lead or lag in the cutter. It was kind of a five-degree-of-freedom geometry problem, computational problem. The APT surfaces are quite complex—very complex surface. And the cutter is also a very complex surface, so you have an interesting geometry problem. And then some time in '68, the guys at Informatics... would be Walt Bauer who was the president, and Bob Rector. I don't remember the other guy.

NORBERG: Werner Frank?

BRANDIN: Yes, Werner Frank was an interesting guy. A really interesting guy. And Werner was good buddies with Ben Mittman. I don't know if you know Ben Mittman, but you should get to know Ben Mittman. Ben was my first boss at IITRI. He was the UNIVAC 1105 salesman, and Ben ended up running the APT long-range program in Europe, and then went to Northwestern after that and ran the Vogelback Computing Center. It was his Control Data 6400

that won the first computer chess tournament ever, and he's very well known in the computer chess community.

NORBERG: Was he involved with the ACM project?

BRANDIN: I don't know. I assume so. But I can give you all the contact information. He was here a month ago for lunch, and we went to visit-- we were renting his apartment in Paris every year for nine years in a row. Anyway, back to Werner, somehow, I got to meet the Informatics guys, and they offered me a job. And this time, I was based in Chicago working as a manager of Mark IV—Mark IV file management system. The customer was the State of Michigan in Lansing—East Lansing. I accepted the job; it was based in Chicago, and I was working in Lansing building some social welfare system with Mark IV, keeping track of data. They were trying to correlate garbage cans and deserted automobiles on the street with poverty, income and childbirths and deaths—all kinds of interesting statistics. Then I went to an Informatics meeting somewhere, and Wilson Cooper, who was the director of the Palo Alto office said, "Hey, Brandin, why don't you come to California, and you can run my NASA project?" They had an umbrella contract at NASA/Ames for so many man-years a year of scientific programming. So I did. We moved back, and this time, we moved to Mountain View instead of Belmont. I took the job with Informatics, and I was running, I don't know, 20, 30 programmers out of Ames. And we were doing stuff for the various satellite programs. It was all computational simulation. And this is now '68. We moved back to California in '68. So I think we were in Chicago for about a year and a half, and then back to California for good. We've been here almost 40 years.

NORBERG: In this same house?

BRANDIN: Well, no, we were in Mountain View for two years, Palo Alto for a year, Los Altos Hills for five years. We had a slide problem on the property, so three years later, got rid of that house and moved here, and we've been in this house for 22 years. Now 67 was about the time I got active with ACM, too, so we maybe want to segue one way or the other.

NORBERG: We'll come back to that.

BRANDIN: The ACM? Okay. So I'm running this NASA program, and this friend of mine who is living out here in Palo Alto, working for Control Data, Arthur L Pfeiffer, had been the teacher at IIT that I took my scientific programming class from and had worked at ITTRI on APT. He was an APT arithmetic element expert. You know, there were maybe 20 APT experts in the world, and there were maybe eight of them who really understood the arithmetic element, and he was one in both categories. So Arthur said to me, "Hey, you know, why don't we start up a company?" Because it's late 1968 early 69; IBM is about to unbundle. APT III at the time was a piece of junk. It was an old FORTRAN-II architecture. It was slow and unreliable with limited capability, considering what you could do it if you started from scratch. FORTRAN-IV was available; the 370s were out. So he said, "Why don't we form a company?" So we scared up two other guys. This was in 1968. We found another guy who worked at IITRI, but not on APT, and we made him our token president. We found an IBM guy who was selling industrial process control systems to the chemical industry. We got him to be the vice president of sales and me vice president of engineering; Pfeiffer was the vice president of APT. And we went out to scare up money. Borrowed \$100,000 from the Bank of California, I think. That was the only

risky thing I ever did, really, in money. And we started scrounging around for money—we ended up finding guys in Chicago who said, “Yes, all right,” and put up money. the leader of the investment group was Irving Harris, who was a very wealthy guy. I don’t think he’s related to the Harris Trust and Savings. And he had a couple guys working him who knew Pfeiffer indirectly. Another investor was Edgar Grenebaum, who was an independent venture capitalist in Chicago at the time, who I knew, and between them, and the University of Chicago’s endowment fund, they put up \$900,000 in 1969. That’s a lot of money, and it was for, I don’t know, 45% of the company.

Then, on June the 23rd, 1969, a day of infamy: IBM unbundled. We had announced that we were going to build APT 70. APT 70 was going to be released in 1970, and it was going to be a FORTRAN-IV architecture, modular layers, interpreter-driven, new capabilities, more reliable, and so forth. We were going to use less memory, and we were going to be faster. In those days, the 360/60s were charging \$400 an hour for a partition, and we were going to be 40% smaller and 40% faster. Boeing at the time was spending \$60,000 a month on CPU time to run APT. So it was a big market.

Well, IBM unbundled and said, “We’re going to unbundle all our application packages.” Well, they did, except for APT. At the time, their APT was called NC-360, Numerical Control 360. So we struggled through ‘69 and built APT 70, started field-testing it. Everybody would field-test it—GM, Rohr, Grumman, these guys, they all field-tested APT 70, and it performed better. If you read the files from the subsequent litigation, you’ll find IBM attacking all the time, but it performed better, and it was smaller, and it was faster. And we even added a capability that I put in called Parametric Surfaces, where you could program a surface parametrically, and they hadn’t done that in NC-360. Meanwhile, IBM was releasing updates to NC-360; they called it cleanup. And the cleanup was always making it faster in the areas where we were outperforming it. It was very interesting. And IBM was running their user group meetings, and it was still free; it remained completely bundled. There was an IBM group in Los Angeles and Century City that was their NC maintenance group—I can show you a document from them—they talked about how they were maintaining their hardware. I think it was \$180 million a year in hardware drag-along—the hardware that went with running APT. The drag was protected with a \$300,000-a-year investment in the software.

NORBERG: By the user?

BRANDIN: By IBM. And nobody would buy APT/70. We started out with a pricing system which said we wanted to charge you based on how much we would save you. I had developed an algorithm and calibrated it to a 360/60 running what we called APT production units, and it was calibrated to deliver us 40% of the 40% savings. Every APT production unit was worth a quarter. But these guys, you know, “Why should we pay you? We get it for nothing from IBM.” And finally, NASA Ames-- and these were not guys I had worked for at Informatics, finally NASA Ames said, “Well, we’ll buy it for \$1,000 a month.” This is ‘71 or ‘70, “Because you can generate parametric surfaces, and we need that capability,” which IBM didn’t have, “for our wind nozzles for the wind tunnel.” We had a unique capability; NASA bought it, and they generated miles of paper tape happily. But we got nowhere with Apt/70 elsewhere, so finally, in November of ‘71, we filed a civil antitrust action in the northern district against IBM charging, you know, classic...

NORBERG: Restraint of trade?

BRANDIN: Well, no, it was a classic antitrust action for predatory pricing, illegal tie, and I don't know the rest of the stuff. But we were basically saying that we can't compete because these guys are giving it away, and furthermore, they're improving it. So November 18th, 1971 is when we filed. Now, do you want the history of that case?

NORBERG: Go ahead, please.

BRANDIN: Okay. We were in discovery for four years. I went to White Plains, and they handed me and Pfeiffer 1,600,000 documents. Many of them were punch cards. And we spent a week in there kind of poking around and picked out 30,000 documents to be microfilmed. There wasn't a lot of money. At this point in time, we had retained the firm of Frederick P. Furth in San Francisco as plaintiff's counsel, and IBM had retained Pillsbury, Madison, and Sutro as defense counsel. And they also had Cravath, Swaine and Moore people running around. At one point in time, when I went in for a deposition one day, I said, "We should be holding this in the Oakland Coliseum," there were so many people in that room. This was the first time that IBM had ever been sued exclusively on software-- well, maybe not the first time they were sued, but it was the first time they were ever going to go to trial on exclusively software antitrust action. The justice case was under way at the time, but it wasn't exclusively software, and we weren't related to it in any way.

NORBERG: Had they settled the CDC suit by that time?

BRANDIN: I don't know if they had settled. I think they had.

NORBERG: My recollection is around 1971.

BRANDIN: I think they might have settled CDC. The Telex case wasn't settled yet, wasn't resolved yet. And the Greyhound case I don't think was resolved yet. I don't know.

NORBERG: I don't know that case.

BRANDIN: That was a leasing case. So we've got these documents. Both Sides started taking depositions. Our lawyer, Furth took the case on a contingency with the understanding that we would pay expenses, and after we ran out of money, he just kept going. He kept going, which was very nice, although the investors came up with some more money. Eventually I think they came up with \$1.7 million total. But they were basically funding the antitrust case at that point. And I left the company in '72 to go to work for SRI; I would work on the lawsuit on leaves of absence and on the weekends and at nights, but I was basically working for SRI from '72 on.

So let's do the litigation. There were 80 depositions. We microfilmed 30,000 documents. IBM microfilmed everything of ours—absolutely everything of ours. I went to some of the depositions. My deposition lasted four days. We deposed people from Rohr, and from McDonnell-Douglas and GM, and they were all saying the same thing: "Yes, we tested APT 70; it was okay, but we were getting it for free from IBM." And that was essentially their transcripts. In 1975, or at the end of '74, IBM moved for a summary judgment. The judge was Zirpoli, a senior judge in the northern district. They argued about the motion for a while, and IBM said, "Look, your honor, we released this stuff to the public domain in 1967." I think

Learson had said in his deposition, “We could price this stuff until Hell freezes over, and nobody would give us a dime for it.” And that was their argument. And then further, said IBM’s lawyers, according to *Buckeye Powder v. E. I. Dupont* of 1914 or 1917, “We cannot be held liable for antenatal acts.” So the judge said, “Well, I’m not going to throw the case out, but let’s bifurcate it. Let’s assume that IBM violated the law and go to trial on the question of injury. Did any act or practice of IBM injure the plaintiff? And while we’re at it, since you did release it to the public domain in 1967, and *Buckeye Powder* blah, blah, they can’t introduce any evidence on price.”

NORBERG: Who can’t?

BRANDIN: We can’t, or the plaintiffs can’t. Well, that’s an interesting ruling. So eventually we go to trial, and it became a technical argument, because we couldn’t argue price. So we improvise and argue, “Well, they’re improving it, and they’re giving it away.” So what is maintenance? Is maintenance the same as changing documentation? Is cleanup something you should charge for? Is there value associated with cleanup? We were litigating very interesting questions that were really secondary to our complaint. We were in trial in ‘75 for six weeks. We presented our case. IBM moved for dismissal. The judge went into hibernation, and at the end of ‘75, he said, “Case dismissed.”

So meanwhile, we had accumulated some more information that IBM had been-- I shouldn’t use the word sabotaging, but let’s say that IBM had been allegedly interfering in some of our beta tests. So we tried to get a new trial, and we failed at getting a new trial in ‘76, and in 1977, we filed an appeal with the Ninth Circuit Court of Appeals. Our lawyer had made one objection during the trial, and it had been to the ruling that we couldn’t introduce price during the summary judgment hearing, and we appealed basically on the grounds that *Buckeye Powder* was wrong. The Ninth Circuit took it in ‘77, and in 1978—now, this was seven years after we filed the lawsuit, we had the oral arguments. The judge sitting on the oral arguments with Townsend, who was the Chief Justice of the Ninth Circuit, who had reversed the *Greyhound* case, Kennedy, who is now on the Supreme Court—a great liberal, by the way—and some visiting judge, I think, from the East. So they heard oral arguments, and then, in 1980, the Ninth Circuit reversed the case. They send it back to Zirpoli, and they said, “The *Buckeye Powder* ruling is wrong,” I can’t find the quote, but this is almost verbatim. “Although price may not have played any role in the decision not to purchase the plaintiff’s products, since it is the central nervous system of most economies with which we are familiar, the judge should at least hear the evidence,” and sent it all back to Zirpoli.

So in 1981, we got back in front of Judge Zirpoli, and he said, “Well, they didn’t like my bifurcation order, but they didn’t say I can’t bifurcate, so let’s bifurcate again, only this time, we’ll assume the plaintiff was injured. Let’s go to trial and see if IBM violated any laws.” The Ninth Circuit also ruled that *Buckeye* didn’t apply to antenatal acts if they were illegal or if they resulted in damage, you could sue to recover. So that was our strong case. And as everybody was pouring out of the courtroom that day, the judge said, “Why don’t you guys settle?” That led to about eight months in which I was a serious participant of negotiations with the deputy general consul of IBM, Dan Evangelista. And Dan and I, and Art Pfeiffer, and a couple of the other Cravath guys, I’m sure, finally negotiated a peaceful, amicable settlement. And the case was settled, I think it was settled in ‘81. No, it was settled in ‘82.

NORBERG: What was the benefit of that case?

BRANDIN: There was no benefit.

NORBERG: To Buckeye at that time?

BRANDIN: Well, there was none. Basically, I mean, the terms of the settlement were confidential. There was no real benefit. The alleged damages were \$63 million, trebled. So in order to make Symbolic whole, they would have had to give us \$189 million plus attorney's fees. We didn't get \$189 million. The investors basically were made whole, and the lawyers were happy. But if this had been the time of the Bill Gates software industry, it would have been an entirely different kind of story. But we were ten years ahead of our time. We were ten years ahead of our time in the venture capital software business and software pricing, and we were litigating fundamental issues: what is software maintenance, how do you price it. None of these things ever got resolved.

NORBERG: Wasn't there a case around the same time with APT? It was one of the first software cases, anyway, brought by Martin Goetz?

BRANDIN: Herb?

NORBERG: No, we'll get to Herb later. I'm sorry, I shouldn't have put that in.

BRANDIN: Well, there was a software case. There was a software antitrust action that was filed...I don't know by who, but it was like '66 or '68.

NORBERG: Marty Goetz.

BRANDIN: Yes. But that got settled. That one never went to trial. Our's was the only exclusive software antitrust case that ever went to trial prior to the guys in Texas who took a case in, like, somewhere in the '80s. So anyway, the case got settled, and that was the end of that. All the documents got destroyed and returned, and I assume I got my name on a list in Armonk, because I had more opportunities to meet with Dan Evangelista in the future.

NORBERG: Yes, I gathered that from the list.

BRANDIN: And by the way, the general counsel of IBM, the former attorney general of the United States, he showed up in court one day; he showed up the day the Telex case got thrown out, beaming, and told the judge, "Your honor, you need to know that the Telex case has been thrown out of court," and the judge smiled, and he looked at him, and he said, "Well, these things happen from time to time."

So anyway, that was the end of the litigation. But it gave me a fundamental education in litigation. I was writing factual briefs; I was doing a lot of analysis on what IBM was doing to APT, and we were filing not briefs, but 300-page documents showing how the performance of the NC-360 was specifically improving in these areas where they weren't competitive. And we have a document that said they were waiting for the price program product to be competitive, and they didn't release a priced version of APT until 1975, and we were basically out of business by then.

You want to go back to SRI now?

NORBERG: Yes, I do. I have a number of questions to ask you about SRI. How was SRI structured when you became part of the staff in 1972? Did it have all the divisions that it had, say, ten years later?

BRANDIN: Yes, more or less. In 1971, I believe, Stanford Research Institute had a problem with students on the campus—the war in Vietnam. They had a building on Page Mill Road that got, I don't know if it was attacked or assaulted, but there was a big problem there—unrest. So at that point in time, the Stanford University Board of Regents or Directors decided that Stanford Research Institute was going to no longer be directly controlled by Stanford University, and it was going to change its name to SRI, International. Now, this is interesting. I worked at Armor Research Foundation, which changed its name back to IIT Research Institute, and then I showed up at SRI, at Stanford Research Institute, right after it's changed its name to SRI-International. Now, of course, you know, when you talk to the clients, you always would say Stanford Research Institute, because nobody knew who SRI International was in those days. There were divisions, and there were groups—groups of divisions. When I arrived in October of '72, there were three groups. One group was the physical sciences group, which had things like chemistry, physics, life sciences. There was the engineering research group, which had... Were they called divisions? I'm not sure they were called divisions. Yes, they were called divisions. And so they had the equivalent of four divisions. There was a systems group, which did things like operations research military work. There was an electronics group which did things like radar and electronic warfare. There was an advanced development group, which did things like Ricoh copiers, because they had an office copier technology. And there was a computer group, a computer division. And then there was an economics and management group, which did management consulting, and had the long-range planning service, which was the management consulting stuff, values and life styles, statistics. I don't know if they existed in '72. They did later on.

So I joined the computer group. I was in something called an information science department, and we were sisters to a computer science department, both managed by a guy named Dave Brown, who ran the information science laboratory. We were part of the information science laboratory. And Dave Brown had worked on the original air defense control system. I can't even remember the acronym for it now—PAGE or PACE or something like that. Most of the money was military or National Science Foundation money.

Short Break.

BRANDIN: Before we get to SRI, let me go back into the '60s. I published my first paper-- Well, publish is a strong word. I presented my first paper in 1963 at the ACM conference in Denver, I think it was. That was the first time I went to a conference, the first time I ever presented a paper, and I still remember that when I stood up to talk and started talking, nothing came out of my mouth. That's how nervous I was. Since then, I've managed to overcome that problem. But that paper was on the simulation of an interceptor missile. And I ended up giving a bunch of papers on that at an IEEE meeting and at a meeting in Lausanne. Well, that wasn't on the missile. But we can go through a paper history if you want later on, but I wanted to... That paper should be in the archives of the ACM, the '63 paper, because I mailed it to them when they asked for copies for their archives.

NORBERG: We'll have to worry about that later, because all of that material is tied up in the move they just made out of New York City into New Jersey.

BRANDIN: They just left New York?

NORBERG: Almost. They took the whole publishing out to New Jersey. Obviously, it was a cost-saving measure.

BRANDIN: I don't know why; they're drowning in money.

NORBERG: You stated at one point that in 1972, you sold, managed, and participated in consulting projects worldwide.

BRANDIN: Starting in '72, yes.

NORBERG: What kind of studies do you remember?

BRANDIN: Well, in '72, the first project was, I was a senior research mathematician working for Dave Brown, and I think the first problem was HPRT—high-performance rapid transit—simulation of a monorail system where the trains would be circulating all the time, and they would respond on demand at a station. Now, that was quite a complex discrete-- that was a discrete simulation, as opposed to continuous dynamic simulation. That was the latest big deal.

Oh, we're going to have to get into my crazy uncle.

NORBERG: Where did the uncle come from?

BRANDIN: It's related, and it happened in '72. We'll have to come back to that as a segue. So let's see. There were simulations of...I don't remember, now, if these were at IITRI or Armor or SRI simulations, but tank simulations, optical calculations, and then in '72 a guy named Earle Jones, who ran the commercial stuff with copiers and stuff, became the acting division head, and I was working for Earle. Earle and I went to Tokyo, and we met with the guys who ran at the time, it was called the software division of Fujitsu. Now Fujitsu, at the time, was manufacturing a machine called the Facom, which was a knockoff of a 360. And they said that their big problem was maintaining compatibility with IBM, because all their customers wanted to be compatible with IBM, and they had an operating system called OSIS, and IBM's system was called OS-360. I think it was OSIS. And they wanted to purchase consulting that would help them maintain compatibility with 360s. So I'm nudging Earl with my elbow saying, "Please say yes. Please say yes." And finally, he said yes, and that began a contract that lasted 17 years with Fujitsu. And that man, the Japanese guy, went on to become a senior executive managing director of Fujitsu, who was one of three candidates to become president of Fujitsu, and when they didn't give him the job, he was forced to retire, because the man who they gave the job was younger. And this guy went on and then became the president of a Fujitsu subsidiary instead. I'm still friendly with this guy, and I did a lot of work for him over the years. But we started working for Fujitsu, consulting to them on software strategy. And then Hitachi found out about it, and they wanted consulting. SRI did have a lot of management consulting projects in Japan.

So that was the beginning of international stuff. We had tried to sell the APT long-range program in Europe, my company taking over the APT long-range program from IITRI. So that one had resulted in visiting Europe in '71—my first visit internationally. We didn't get that contract, but starting at SRI, I think I started in '72 or '73 also going to Europe. And we had a lot of business in Sweden. There was a big SRI office in Sweden with StanSAAB and Data SAAB. They were manufacturing machines and minicomputers, and I was working on those kinds of projects and consulting. I told StanSAAB to stop manufacturing minicomputers, because they couldn't make enough to make a buck. And I told Fujitsu, if you want to know how to make workstations, make workstations; stop distributing them from Sun. Stuff like that. We had business in France with banks. There was a lot of business in Scandinavia with the banks, too. And over the years, I ended up doing projects for BMFT in technology, the R&D ministry of Germany, and the City of Berlin. If we take a break, I can list projects.

NORBERG: Were you involved with Norway in any sense?

BRANDIN: I went to Norway on some sales trips, but I never had any projects involved. I remember SRI had projects there, and SRI had projects in Denmark, and I was crazy about Copenhagen, but I never had any real business there. But when I went to Northern Germany or Sweden, or even the sales trips to Norway, I would park myself in Copenhagen and commute. It was a great airport location.

So I started as a senior research mathematician working on projects, and then they broke Dave Brown's lab into two labs. Instead of the computer science department and information department, we became laboratories of our own. And I working for a guy named Norm Neilson, who was a discrete simulation expert, and Jack Goldberg, who was a very good computer scientist over in the computer science lab. And then, for reasons which history doesn't need to know, they took the lab away from Norm, and they came and asked me if I wanted to run the lab. And this was '74, maybe '76. I ended up taking over the laboratory. So I'm running the laboratory; Jack Goldberg is my peer, and we're both reporting, I believe, to Earle Jones. A lot of Japanese business, working in Europe. ACM is eating up a lot of my time, too, and there's a lot of travel that ACM is pumping into my system.

And then, in '76 or '78, Bill Miller, Professor William F. Miller, Provost of Stanford, Professor of Computer Science, and I think a professor in the graduate school of business, became president of SRI. And he decided to make the divisions that had executive directors, vice presidents, and their bosses senior vice presidents. And about the same time, SRI sold off the Doug Englebart Augmentation Research Center—ARC—which was a sister lab, essentially, in the computer group. Everything was time-shared. Tymshare bought them. So Miller called me and Earle Jones and a couple other guys into his office and said, "As of now, you're all vice presidents." Well, it didn't happen quite that way. He made the senior VPs, and this guy named Ray Leadabrand, who became a senior VP of the engineering group, and he said, "Okay, we're going to organize a computer science division. It's going to be called a division now. We're going to do a search for a vice president. And we're looking for a woman." So they looked for a woman, and the only women that they can find that are qualified for the job don't want the job because they had bigger jobs in Washington, like Ruth Davis. So after a while, they didn't have anybody. So I went to Ray, and I said, "Hey, I want the job" and he said, "Well, let me ask the other lab directors." He went to Charlie Rosen, who ran the AI center, I think it was Don Nielsen who had the telecommunication sciences center, Peter Hart ran AI and Charlie Rosen ran machine intelligence, and Jack Goldberg, and then some of my people, and everybody said,

“Yes, we’ll take Brandin.” Oh, right. There was one other little credential that crept in there. But anyway, Ray gave me the job, and I became vice president of computer science. Now, the little credential that crept in was called President Carter’s Federal ADP Reorganization Study. In 1978, I went to Earl Jones, and I said, “Earl, I’m tired of running the laboratory. Why don’t you make me the division marketing guy or sales guy? I want to go out and do more marketing and promotion,” because I was really good with the customers, especially the Japanese and the Europeans. I spoke enough German to be charming, and my wife is fluent in French. You need to know about my wife’s background, too. So Earl said, “Okay, let’s put you in marketing,” and about three days later, after I turned my lab over to Pat Whiting O’Keefe (who was a very good, PhD chemist), one of the other SRI staff VPs called me and said, “Hey, we’ve got a request from OMB. The President, Carter, is starting the Federal Automatic Data Processing Reorganization Study, and they want a volunteer. They need somebody for the national security team. Are you interested?” I said, “Yes, I’ll go.”

So I went to Washington. I think it was for three months, steady state, and then another three months of back and forth, and I worked on a national security team Air Force project. We spent three months-- oh, the White House cut orders saying that we were GS-18-equivalent—like major generals—and cut us travel orders, and we spent three months interviewing people in the Pentagon and the Office of Data Automation and the Secretary of Defense and the various intelligence community organizations. I went to SAC command posts in Omaha; it was down in the basement. I went to Cheyenne Mountain. I went to all the Air Force research laboratories in various commands, and came back. By the way, I’ve got copies of the reports. I got a thank you letter from Jimmy Carter signed by a machine, a certificate of appreciation signed by a machine. Bill Miller got a thank you letter, and when I came back to SRI, that was when they began the search for the VP.

So Leadabrand, who was the senior VP of engineering, was a fanatic about defense—you know, a real patriot kind of guy, a former fighter pilot in the Korean War. And Ray just took to my military background like a duck takes to water, so I got the job. And in the federal ADP reorganization study, we had three conclusions: one, stop studying the problem, two... the third one was, the only-- oh, the first one: the only thing that has changed in the last 13 years since the last study by the office of the assistant secretary is that the average age of the computing inventory has increased by 13 years. Two, stop studying the problem. And three, competition does not necessarily lead to the lowest cost to the taxpayer. Now, this was at a time when the Brooks Committee—government operations committee—controlled procurement, and the military was going crazy, because the rules in the Brooks Committee said you only consider hardware costs; you don’t consider software costs. And this was at a time when Barry Boehm had written his paper—you know, “The High Cost of Software.” So it was chaos, chaos buying computers in Washington. So I did that study. I have a copy of that report. I may even have the briefing too.

NORBERG: Who else was in this group?

BRANDIN: There wasn’t anybody that I would say was known. There was a guy named Larry Dreeman who was in charge of the national security team. He came from Coca-Cola. I’ll have to look at the reports. I don’t know.

NORBERG: Why from Coca-Cola?

BRANDIN: Because he was the automation guy at Coca-Cola. There were about 45 or so folks on the study. Half were feds, and half were called executive loan volunteers. I was paid my salary by SRI, and SRI paid my travel. Defense did not pay my travel. It was an expensive contribution for the Institute.

NORBERG: Didn't they get some of that back through contracts?

BRANDIN: I think the only way they would get it back would be through IR&D. They might be able to use it as a justification for IR&D.

NORBERG: When I went to Washington to the National Science Foundation for a few years, they wrote a contract with the University of California Berkeley, and I was paid through that contract, and I got all my expenses taken care of, and so on, and they got a piece of action, too; they got a little bit of overhead. It wasn't a lot, but it was something.

BRANDIN: I don't remember seeing a contract, and I would know, because I was paying attention to contracts. There was no contract, but I'm sure that it would have been used during the annual audit to justify IR&D expenses, because the charge number they gave me was an IR&D charge.

So that got me to be vice president of computer science. At the time, I headed artificial intelligence. There were about 70 people in the AI center. Machine intelligence, which is robotics and machine vision, were about, I don't know; there were 50 in AI, 20 in intelligence. Computer science had 20 guys. My old lab had 20, and telecommunications. Now, the projects at that time, we had AI projects that were in the area of expert systems and natural language. We had vision. We had a big industrial vision program that was sponsored by the NSF. There was a lot of NSF money there. In telecommunications, the biggest project was packet radio, which I worked on, by the way. Oh, that was my second big project at SRI was, I built the first portable packet radio terminals.

NORBERG: This is the Aloha system?

BRANDIN: Well, we used Aloha to check out the terminals, because the packet radios that were being built by Collins Radio were not ready at the time the terminals were. So we plugged them up to Aloha radios and took them to Maui, and ran the first portable packet radio terminal experiment, which actually started out in Frank Kuo's garage in Honolulu.

NORBERG: This is the fellow down in Cornell—Kuo??

BRANDIN: It's Kuo, but he's not at Cornell now; he's retired. He's here. We built the terminals, so there's a full terminal. It had 80 characters to display; it had a 20-column printer; it had a microprocessor—an IMP16 microprocessor in it. This was before the 8080s. They didn't want to use the 4004. Keyboard. And it was programmable. I mean, it had a user interface. We built it. And I designed the interface, and one of my guys programmed it. So we had this thing with an Aloha radio, and the first thing we did was run it in Frank Kuo's garage at the University of Hawaii, in his house. We were using his AC power, and we linked to the University of Hawaii, which then put us into a satellite link into the ARPAnet. Then we took it to Punch Bowl, and we linked to the University of Hawaii one hop, and it put us into the ARPAnet. Then

we went to Maui, and we went up to the top of Haleakala. So we were up here at the top of Haleakala, and the University of Hawaii had an Aloha repeater at the top of Diamondhead, and they had a station at the University of Hawaii. So we linked from Maui to Diamondhead to the University of Hawaii, up to satellite, down to NASA Ames, and then we were in the ARPAnet, and sent real-time email to ARPA. And for power, we used a 100-volt Honda motor generator with a gallon of gasoline at the top of Haleakala. We did that in 1974.

NORBERG: Aloha, I think, started in '72, didn't it?

BRANDIN: Yes. Now, we were very disappointed because they had a repeater at the top of the rim, but it wasn't working. We had made arrangements with US Forest Rangers to take an expedition down into the crater so that we could link to the repeater, and then link to the...

NORBERG: Oh, I see, yes.

BRANDIN: But so anyway, we did that. And Bob Kahn never gave us credit, never said a word about it. But we were trying to demonstrate that the stuff works in a rugged environment.

NORBERG: They had a test bed that they were trying to show would work in a robust environment, too.

BRANDIN: Well, yes, at SRI we ran the packet radio test bed at SRI, in my division, and that was a big project, and even in '74, we were driving-- I think it was '74, or maybe a year later, when Collins radios showed up, we were driving down the Bay Shore Freeway and linking 100KB/sec terminals to a 400KB repeater and going into the ARPAnet. This was '74. So packet radio was a big deal, and there were projects to put them on B-52s and put them on NOAA weather stations so that you can have a completely robust system that would self-configure, because there was no central station in packet radio—it was completely self-configured.

NORBERG: Can we go back for a minute? I'm having a little trouble in looking over some of the documents that you have produced as descriptions of your activities over the years. What exactly was your role at SRI at the start of '72? It seems to me that I think one of the documents says you became vice president for a division-- vice president director of the information systems division, international management and economics group.

BRANDIN: That came later.

NORBERG: That's what I think, too, but I'm trying to get it straight as to what came first, what came second, what came third.

BRANDIN: Okay. Well, first, I was an senior research engineer, and I was working on this transportation simulation problem, and then packet radio. And then somehow, I ended up running the department, and then I ended up running the laboratory, and then I became the marketing guy for three days, and then I went to Washington and then came back and took over the division. My real job running the division was, I was a bureaucrat. I mean, I shepherded the division; I argued for capital equipment; I maintained a little performance discipline; I fired if I had to fire somebody. But I provided the environment for all the guys to do their thing. I would

go to Washington, and I would knock on doors, and I would be nice in front of clients, and I would go get international customers like the Fujitsu guys to bring money in or the BMFT money for my guys. But I wasn't doing any specific research as the lead VP; I was basically an administrator, and I was keeping Bill Miller happy so that he could keep his board of directors happy. By the way, by the time the separation of the institute from the University started, the board of directors were the same, but over time, they translated and became different.

Session 1, Tape 1, Side 1

BRANDIN: I was doing consulting—plenty of consulting for various clients—because by then, you know, I had a lot of management experience. And in 1981, SRI sent me to Stanford for the eight-week graduate school of business executive program, where I basically got the MBA without the credential. And that was fantastic; that was a wonderful experience. They had also sent me in '78 to take a two-week course run by the engineering school. I don't remember what it was called, but it was management of engineering processes. That was great as well.

NORBERG: That's early, isn't it? A number of such programs didn't start until the '80s.

BRANDIN: This was '78. Yes, it was '78—a two-week course. That was called the Stanford Engineering Executive Program, and then the Stanford Executive Program, which was the GSB program. I would stop in at SRI a couple of times a week to make sure the computer science division was doing fine, but I had good laboratory directors, and my deal with them was that as long as they sign a document that they were comfortable with it, I would sign it. That was my basic understanding with them. And I actually thought they were better computer scientists than I was. At the time, I had Jack Goldberg running computer science. Charlie Rosen had retired. Peter Hart was running AI, and then Nils Nilsson, who is at Stanford now; and Don Nielsen, who was running telecommunication sciences, ended up taking over the computer science division when I left. And I don't remember who ran the other lab. But I had a great boss, Ray Leadabrand.

NORBERG: What do you think was the benefit of going through these programs at Stanford University?

BRANDIN: Oh, at Stanford, I learned how to be a businessman, I think. And in the engineering education program, I learned that if you really want to be a good engineering manager, you better support research. And how to deal with people; there was a lot of human interaction. There was Hal...

NORBERG: Varian?

BRANDIN: No, this was a psychologist guy in the B school program.

NORBERG: Hal Varian is over at Berkeley now.

BRANDIN: He did a lot of, you know, how to get along with people, and the classic problems. In fact, he had one problem which he brought up where you're the vice president, and you've got

a group working for you, and they don't want to do the work because it's not academic enough, and you can't convince them. And so I raised my hand, and I said, "This problem can't be solved. You cannot solve that problem." And nobody, of course, believed me. Of course I had an example. I had a perfect example. I won't use names, but I had a visitor from a highly classified organization come to visit us and tell us that he wanted us to do a very interesting problem that we were capable of doing, and I had the guy who ran the group stand up and say, "We don't want to do this work," in front of the customer. He said, "If we do this work, we can't publish or results; we can't be peer-reviewed, and we have people standing in line in Washington waiting to give us money, anyway." And the major general walked away in disgust. There was absolutely nothing I could do about it.

NORBERG: Before we leave this early part of your life and get on to volunteering for ACM, can you tell me a little something about Ellen's activity?

BRANDIN: Yes. Ellen may be more interesting than me. Ellen was born in 1938 in Warsaw, Poland, and in 1942, ended up-- I know a lot of this, because some of this is in my new novel. She ended up in the Piotrkoff Ghetto which was shrunk by the Nazis into the Bujai ghetto and slave labor camp. Approximately '42, and they were there until November of '44. Her father had a lot of money. Her family was wealthy, and that's how he managed to keep from being shipped out. In November '44, they were shipped to Ravensbrück Concentration Camp. The last thing, the Germans cleaned out the ghetto, then, and they were in Ravensbrück for two or three months before they were shipped to Bergen-Belsen. They were liberated by the British on, I don't know, April 10th, or something 1945. Her mother and her sister and she survived. They were all together. They were shipped to Ravensbrück and Bergen-Belsen together. Himmler at the time was negotiating with the allies behind Hitler's back, and he was using the Piotrkoff Jews as a pawn in some negotiations. So that probably got them a little coverage. Then they were liberated by the British.

An interesting story, there was a sergeant in the British army at the time who rolled into Belsen as a liberator, and he turned out to become a brigadier-general, Laury Lawrence, of the British Army, and ended up running the data automation operations for Midland Bank. I took him to dinner in San Francisco one night, and he and Ellen discovered that night that they had both been in Belsen on the same day, it was a very, very emotional scene on both their parts. Anyway, in '45, after they were liberated, the Red Cross sent them to Sweden, Malmo. Her mother was in a hospital for about a year, and the girls lived with some people, and then they found an uncle in Brussels and lived in Belgium until '50. So her first language—her first real fluent language—was French, which she still is fluent in. She came to the States in '50, Chicago. She went to the same high school I did, but was a semester ahead of me, and we started dating after high school, and got married in 1960. She got a degree in English, a minor in French, of course. Her major interest is traveling in French-speaking countries, and now Italian, and she's a docent at the Asian Art Museum. She's been a docent since 1993, I think, giving tours in the museum of all the Asian collections-- China, the Himalayas, Japan, Korea—that kind of stuff.

NORBERG: I'm impressed.

BRANDIN: We have one child, who was born in '66, so he's 41, and he has two boys, so I have two grandchildren.

NORBERG: You and I are only a year apart. You probably have guessed that. I was born in '38.

BRANDIN: Oh, well, you're Ellen's age. I'm 68.

NORBERG: I'm 69. Okay, let's turn to ACM.

BRANDIN: Well, let's finish SRI.

NORBERG: Oh, I'm sorry. Okay.

BRANDIN: In '83, I was talking to the guy who-- well, there were several senior VPs. I worked for Leadabrand, the engineering VP, and there was another senior VP named Donald Fiske, and he ran the management and economics group; I talked to him a lot, and we concocted a scheme to create an IT management consulting division out of some elements in his group and some of the laboratories in my group. So we went and talked to Leadabrand, and then we talked to Miller, and then we ended up creating a new division in the management and economics group. That was one of the results of me going to Stanford, wanting to do more management and consulting, and I created a new management consulting group, and I ran it from '83 to '86. And it was just like an Arthur Anderson kind of management group—statistics, and I had VALS, the values and lifestyle marketing data statistics program. I had all the computer security work. Donn Parker, the great bald eagle of American computer crime, worked for me. And by the way, I was the one who led the SRI team to give testimony to the Privacy Protection Commission. I've got copies of it.

NORBERG: We'll come back to that later.

BRANDIN: Okay. I ran that until '86. They'd brought in a new senior VP named Alan Elston in '85, and I put up with him for a year, and I couldn't take it, and I quit. I quit—a rare event at SRI for a vice president to not be fired. Bill asked me to keep going, and I said no. I said I had to finish my book I was writing *Technology War* with Mike Harrison then. And I knew then that if I didn't quit, I would never finish the book or quit. I would never leave SRI. It was a place where you could go to sleep. And I was restless and bored, because Stanford had taught me, you can never have enough money, and it seemed like there were ways to make money. So that then led to other stuff, and we can come back to '86 later on.

NORBERG: When did you become a member of ACM—member, now, not volunteer?

BRANDIN: Well, the first time I became a member was 1963. I was a member for one year, and then they raised the dues, and I quit. Then in 1966, I rejoined, and I've been a member since '66.

NORBERG: And a volunteer since '67?

BRANDIN: I mean, I was one of the group in Chicago that formed the Chicago chapter in '63. It was called Mid-Continent Computer Club, MC². Art Wachowsky was the guy's name who recommended we found a chapter, he worked at IITRT. Jim Adams, at the time, was working for

DPMA—the Data Processing Management Association, and Jim and I and Art and a few other people said, “All right, we’ll organize a chapter.” So we organized the Chicago chapter.

NORBERG: Was that very difficult to do?

BRANDIN: No, it was very easy to do. I mean, ACM was desperate for chapters. ACM was desperate for volunteers. I don’t know if they’re still desperate for volunteers, but they should be. Anyway, there was a problem at least in the main period of time that I was active with ACM. ACM had a set of volunteers, IEEE-Computer Society had a set of volunteers, and some of them were good and some of them were bad on both sides, and it would have been really nice if you could combine the good ones, but you couldn’t do that for lots of reasons.

Anyway, I was active in ‘63 as a volunteer, and then in ‘67 I was sitting in my office in EAI in Palo Alto reading the Communications of the ACM, and there was a letter to the editor written by Bernie Galler. I had given Bernie Galler, when he was alive, credit all the time for this. It was: “Why don’t you volunteer? If you’re sitting there wondering what to do, volunteer.” Well, I read it and I said, “Well, shoot, I’m working in simulation. I’ll join the special interest group on simulation.” Opened up Communications with I couldn’t find...there was no special interest group on simulation. I couldn’t believe it. I mean, at that time, simulation was one of the biggest users of computer time in America. So I wrote a letter to, I don’t know if I wrote it to Bernie or Oettinger. Who was president at the time? I think it was Oettinger. I said, “Hey, I would like to join the special interest group on simulation. I can’t find it. It must be a typo. Please forward my letter, and if not, tell me what to do.” So the next thing I knew, I got a letter saying, “Come to Washington at the ACM meeting in ‘67, and we’ll give you money.” \$200. So I don’t know who paid for that trip, whether it was IITRI or EAI, but I went to Washington, and went to a meeting with the Council. Maybe it was an executive committee; I don’t remember.

Galler was there, Oettinger was there, a couple of other people. They were talking about big things, and here I am, a little kid, 28 years old, and I said, “Yes, I want to found a special interest committee,” at the time, “And call it Simulation—SIC-SIM.” And they said, “Okay, here’s \$200.” So I went, and I created a newsletter and a mailing list, and I published something, a notice. I don’t remember how I got members, but I started getting members, and I was chairman of SIC-SIM. And that led to a proposal to Jim Adams, who now was at ACM, running the professional development seminars. Art Wachowsky and I wrote a proposal to deliver professional development seminars called “Digital Simulation of Continuous Systems”, and ACM published brochures and mailed them out, and we went around to different cities, and we gave these professional development seminars.

NORBERG: What was your view of ACM at the time you attended those meetings—either the executive committee or the council?

BRANDIN: Well, these were people who were academic theoretical people who had invented the field, sort of. I never really thought science was the right word for computer science, because to me, programming was not a science. Science was running physical experiments. But it ended up kind of getting that name, and these were the big names. Oettinger was a big name, a Harvard guy. And I was young still, and I had only given a couple of papers. Never published anything in a refereed journal at that point. Not even at this time. And I was a little in awe of them. But they gave me money, and I had good friends who were beginning to be associated with it.

Adams, I had known since '63 in Chicago. Wachowsky, I had worked with at IITRI for years. So we started these seminars, and that got me more active in ACM. I was chairman of SIC-SIM. I started going to meetings of the SIG-SIC board, which at the time was managed by Jean Sammet.

NORBERG: Jean Sammet was the chair.

BRANDIN: Chair of the SIG-SIC board, and Denning...

NORBERG: He was around.

BRANDIN: Yes, I don't know if he was on a-- for some reason, he was involved with the SIG-SIC board.

NORBERG: I had the idea that he was vice chairman at some point of the SIG-SIC board. But I don't know whether it was this early or not.

BRANDIN: That's possible. There is a little confusion in my timing, here. Anyway, I wasn't chairman of the SIGSIM-- was I chairman long enough to get it converted to a SIG or not? I don't know.

NORBERG: I have the history that you wrote here. That may tell us something.

BRANDIN: But anyway, in '72, approximately '72, Denning and I began a project for Jean Sammet to rewrite by-law seven. By-law seven of the ACM constitution was the controlling instrument in managing SIGs. I guess it would have to be '74. There was a terrible problem when I got elected chairman of the SIG Board, because Jean Sammet had just confiscated all of the money in the SIGs. This was an emotional, political issue. Some of the SIGs were actually making profits, and ACM was losing money. So Jean commandeered the money, and she now-- I don't know which is the chicken or the egg, here, but she now wanted the chairmen of the SIG Boards to sign over their signature authority for their checking accounts to headquarters, so that if they needed the money again, they could grab the money. Well, this led to quite the confrontation at a council meeting, and at that time, it was like 25 votes on council. I argued eloquently against, and the vote came down 12-12, at which point Sammet voted her way. So we had to sign the cards, but anyway, that was a very tough moment, but it came and it passed. Anyway, we got the controller to indicate that the SIG funds were reserved, and so it was harder to get at.

NORBERG: Well how does that happen? Because one of the management issues here has to do with the relationship between ACM Council and the board of the SIC-SIM.

BRANDIN: Well, I mean, it didn't go down very well. Everybody was pissed off, and I had to-- I think the only reason they didn't lynch me was because I had done an admirable job at getting a 12-12 vote out of that council, because prior to me, anybody who would have gone in there would have been completely in agreement with Sammet. And in retrospect, I am now in agreement with Sammet.

NORBERG: Why?

BRANDIN: Because I don't think the SIGs deserve the financial autonomy that they got, because the people who ran the SIGs were, for the most part, prima donnas, unskilled in managing, and had no business at all controlling those funds.

NORBERG: But isn't that the case with every professional society, no matter how large?

BRANDIN: Well, maybe so. Maybe so. But anyway, we can come back to this; this is an interesting topic.

But at that point, or somewhere close to it was when Peter and I got together and rewrote by-law seven, and the new by-law really got into management issues, requirements for SIG chair and how you created SICs. One of the nice things that by-law seven did was, we took away the requirement of council to give money to new special-interest committees, and we put it in the hands of the SIG Board, so that the SIG Board, which was managing the surplus that belonged in theory to the SIGs, could decide on its own to create a SIC and give them money out of the SIG reserves to finance themselves, and that took away having to beg and plead to the executive committee. So that was a very nice autonomous decision to give to the SIG Board.

NORBERG: Okay. Was this part of the Sammet reorganization attempt in 1975 that Bernie Galler was running for her? Do you remember that?

BRANDIN: I remember that Jean did a lot of things, because when she inherited the job, ACM was a little broke. She had to steal the money or get the money wherever she could. The publications board people were spending money left and right, and Galler was certainly in her camp.

NORBERG: Always was, until the day he died.

BRANDIN: And so, you know, it was a competition for resources thing. And meanwhile, Council was semi-bipartisan. There were vested interests; there were the SIGs, there were the chapters. The chapters had this problem that they wanted to get ACM money, and they wanted ACM recognition, but they had no technical credentials whatsoever, and they wanted to be treated like a SIG, more or less. So there was always this problem of, if you want to support the technical activities, you want to give the money to qualified technical people. Why give the money to these guys who have these meetings, and then after the meeting, they all sit around and get drunk? Which I'm pleased I participated in, too. But there wasn't really any good management structure in place for the chapters. It was like a dog hanging on your ankle, chewing every now and then on your ankle. The rest of the association, they wanted bigger chapters, but the chapters were a problem, and the council was divided, and there were people who were being elected by the regional reps. The regional reps were elected by, presumably, the political power of the chapters, and then there was everybody else. So it was a bonfire of vanities. It was. And of course, you had a spoiler in there; you had Herb Grosch. He was a real spoiler of a guy. He wouldn't put up with any bullshit from Jean. I don't think he had a problem with Galler, per se, but him and Jean were like fire and water.

NORBERG: She and I were just that way, too.

BRANDIN: Oh yes? Well, Jean doesn't like me, because-- well, she may like me, but she won't talk to me much, because she still has never forgiven me for the remark that I made about computing reviews being a rat-hole.

NORBERG: Was this made at an open council meeting?

BRANDIN: No, it wasn't at a council meeting, but it was quoted in Communications by somebody when they talked about the history of the SIG Booze.

NORBERG: All right. Let me get some of these things straight, but I want to ask an incidental question first. And that is, were you involved with any professional associations in the '60s and '70s besides ACM?

BRANDIN: Yes, I was a member of Simulation Councils, SCI, which was the simulation association. I was a founding member of the Internet Society until I discovered they dropped that membership category.

NORBERG: Did they drop it to get certain people off?

BRANDIN: No, they dropped it in order to get more dues. The Internet Society may have been after the '70s.

NORBERG: I was just looking at the dates, and that's a little early for the Internet Society.

BRANDIN: I don't think so. I don't think I belonged to anything else.

NORBERG: All right. Okay. So let me summarize for a moment here what roles you were playing in ACM in these early years. I'm talking about '67 to roughly the mid-'70s when Sammet was still president and before Grosch comes in, because that's a whole other can of worms we really have to open up, I think. But the roles that you served, you were a founding member of the Chicago chapter, which continued to be an important group through the next three or four decades. You were serving as some sort of member-at-large in the Council, were you?

BRANDIN: No, I was on Council as Chairman of the SIG Board, '74 to '78.

NORBERG: Okay. Did you participate in the various controversies involving SIGs in addition to the one you mentioned about the signature authority...

BRANDIN: Well, I mean, I was on the SIG-SIC Board, Sammet's SIG-SIC Board, for some number of years before I became Chairman of the SIG-SIC Board or SIG Board. And so I would have been involved in discussions at the SIG Board about problems with SIGs, or should new ones be chartered, stuff like that. But I don't remember any profound issues other than the money and by-law seven.

NORBERG: Okay, there was one.

BRANDIN: Well, you know, there were personality problems. SIG-Micro should it be called SIG-Micro or SIG-Mini—stuff like that, SIG-Soft.

NORBERG: But there was at least one of the SIGs in middle 1977 which was faltering, and the Board was giving its attention to this now, and the question for the Board was should we drop it, or should we strengthen it, and there was an argument to strengthen, again, and it went on. It was SIG-MAP.

BRANDIN: I was chairman of the SIG Board then, from '74-'78.

NORBERG: Okay. Now, would you be arguing before Council to allow--

BRANDIN: I would have been on Council. But that issue would not have been-- I don't remember that. Was that debated in Council? I don't believe so.

NORBERG: Well, you know, I'm reading between the lines.

BRANDIN: I don't think it was debated in Council. It would have been a SIG Board issue.

NORBERG: Well, maybe. I'll look when I go back at some of the documents.

BRANDIN: Well, the new by-law seven would have said that would have been a SIG Board issue. That issue would have been an interesting one, because it would have been the theoretical mathematician computer scientists who couldn't manage their way out of a real shoebox versus the guys who care about the money. And if I remember right, it was what's-his-name from Stanford who was the mathematical...the MAP guy. He didn't like me, either. I think he was a Council member, too.

NORBERG: Could be.

BRANDIN: I think he was a Council member and then he resigned, and I replaced him on Council later on. Well, anyway, you can check your notes.

But the main thing in the '60s was the Professional Development Seminar, the Chicago chapter, and then active on the SIC Board with SIC-SIM, and then bylaw seven, and Chairman of the SIG Board, which got me on Council. Now, during that period, I also may have been Chairman of the External Activities Board for a period of time. In fact, it was during the McCracken-Denning... I was Chairman of the External Activities Board in the beginning of the - no, I was secretary.

NORBERG: You weren't secretary while you were chairman of the External Activities Board.

BRANDIN: That's possible. Yes, I had to give it up, because Kathleen Wagner beat me in an election that was contributed to by the chapters, because she was a chapters advocate—she was from them, from that side. But when I became Chairman of the External Activities Board, I remember writing a letter. It was either to Denning or McCracken, or both, and it said, "Okay, I am now the equivalent of your secretary of state. I will be very happy to represent the interests of the Association if the Association will tell me what its interests are."

NORBERG: Chair of the External Activities Board, 1980-81.

BRANDIN: Yes, okay, so I was secretary from '78 to '80, and then I got thrown off when Kathleen beat me, and then somebody on Council resigned, and they put me up.

NORBERG: I think Peter Denning had to resign because he became vice president.

BRANDIN: No, he would have been on Council as vice president. But there was a Stanford guy who resigned during the '80-'82 period, and I then got back on council as a replacement for him.

NORBERG: Did you have anything to do with the DPMA?

BRANDIN: I had something to do with the DPMA only in the sense that it was in AFIPS.

NORBERG: Okay. I came across that at one point, and I didn't know how strong that association was.

BRANDIN: Well, I didn't have any personal relationship with DPMA. Jim Adams had worked for them, and I worked for ACM, but DPMA was a member of the AFIPS community, and they were a problem for us.

NORBERG: In what way?

BRANDIN: They were a problem in that the original guys who set up AFIPS—and this has got to go back to I think Oettinger or even earlier, and the IEEE computer society—set it up with a constitution and voting rights that gave big societies three votes and then little societies one vote, and depending on the total dues you had paid to AFIPS, it affected how much you got in distributions. So when DPMA came along, DPMA said, "Hey we want to join AFIPS, and we're a big society; we want three votes." But what they said at that time was, "Okay, but you're not going to get a bigger share of the distribution until you have accumulated the dues." At the time, AFIPS was making a bundle on the joint computer conferences. So the board of directors consisted of three ACM directors, three IEEE computer society directors, three DPMA directors, and nine little societies. My numbers may not be right, but the balance is what's important, because the president then casts a controlling vote. I don't remember the years I was on the board of Afips, but it was for more than four years. I was certainly on the board as president of ACM, and I was certainly on the board as past president because Adele would have kept me on the board, and I was on the board before I was president. So I was active in AFIPS for some time.

We encountered management problems during my term as president with AFIPS. Ralph Leatherman had been the president of AFIPS, and he was from DPMA, and now Sylvia Chorp was the president; she was from the American Educational Data Association, something like that. Ralph was either past president or something similar. The Afips leadership then cooked up a scheme to change the distributions. I can't prove it. Even if you get the minutes of the AFIPS meeting, I can't prove it, because they would do these things in secret votes, even though I would demand open votes. But they proposed a scheme to accelerate the distributions to the DPMA and, in the change of distribution, the little societies would get more, too, so that they could get

the votes of the little societies. So all of the sudden, we get this vote: we're going to change the distribution formula. And I look at Oscar Garcia, who's president of the IEEE-Computer Society. He looks at me. We go, "Where the hell is this from?" At the time, we were getting, I think, around six or \$800,000 a year each from AFIPS, so ACM needed the money. This was also about the time Shelly what's-his-name was starting COMDEX, but nobody knew that it was going to become a major competitor.

At the meeting, I introduced a motion to distribute a million dollars. And Mike Harrison seconded the motion. Wally Johnson, I don't know where he was from, but he was the treasurer, said "That's the stupidest motion I've ever heard." Mike asked, "Well, why is it stupid?" And then finally, the motion passed, and then they did a second meeting, at which the motion passed a second time. So they were going to distribute a million dollars, and I think ACM got 20%; IEEE got 20%. \$200,000 was going to show up in ACM accounts. Between the two meetings where the first and second motions passed to distribute the funds, they also introduced a resolution to change the formula. I asked for an open vote, and Sylvia said no. I objected and asked for an opinion of the parliamentarian, and the parliamentarian disagreed with me. I demanded that the minutes showed-- all six of us demanded that they record our votes. It was ACM and the IEEE, and there were three little societies and the DPMA. That's how the voting went. And they passed the resolution in secret and changed the formula. But we had passed the resolution for the one million dollars.

Then I get a letter in the mail saying Sylvia had decided that since we passed the resolution to change the formula, she was going to distribute the one million dollars according to the new formula. I asked Sydney Weinstein, who was my executive director, to get the lawyers to write a letter to AFIPS saying we were going to sue, and calling for an emergency board meeting. We called for an emergency board meeting, and they agreed to distribute the money according to the old formula. And then they went into the new formula after that. So DPMA basically got a free ride from that point forward, because of the scheme concocted by the officers. And the little societies, they were happy because they got more.

NORBERG: But now, how soon after that did AFIPS collapse?

BRANDIN: Pretty much... Let's see. When did we dedicate the building? Let me get the paperwork. By 1983 the whole thing was in the tank. It was shortly after they dedicated the building that NCCs wound down as Comdex wound up. After the collapse, I was interested in the dedication plaque. In this AFIPS building, on the wall in the entrance there's a bronze plaque with all the names of the directors that dedicated the building, and I wanted to get it and cut it up and make paperweights out of it.

NORBERG: I can see why they didn't like you very much!

BRANDIN: Well, after the thing collapsed. All right, so anyway, I was president from '82 to '84.

NORBERG: Well, let's not go quite that far now.

BRANDIN: But you were asking about AFIPS. And AFIPS, during Adele's-- who came after me, reign, the competition started to heat up, plus there were NCC problems. We had problems in Anaheim where there had been an air conditioning failure.

NORBERG: That was terrible. That was in Houston, wasn't it?

BRANDIN: It might have been Houston or it could have been Anaheim. But the fire marshal shut down that tent, and then nobody wanted to give the money back. I mean, it became very nasty. Meanwhile, Shelly was running COMDEX, and he had been treated like scum by AFIPS. The first time they had a personal computer exhibit, they wouldn't even let him on the show floor. This was in Anaheim. So they were in the Disneyland Hotel. And then after that, I guess Shelly launched COMDEX on PCs, and the competition was growing, and NCC was in deep trouble. Paul Raisig was the executive director at NCC, and he was an ex-Army colonel, and I don't think he really knew the trade show business. Politically it was in chaos. And as Adele took over, she started warning the ACM Council. The money was drying up; we've got to do something about that. Adele came in-- First of all, I left her a \$1.5 million surplus, which she spent. But she also started changing the economics of the association dramatically. And we had pushed through a dues increase during my term, I think.

NORBERG: Yes, several—your term, previous terms.

BRANDIN: It was very hard to get a dues increase. The problem with getting a dues increase wasn't getting the vote; it was getting a large enough quorum. And we had to advertise to get that. But she was good to work with. She could be a hard person to get along with, but she's very good to work with.

NORBERG: Well, that seems like Jean Sammet is the same way. If you're on her side, she works wonderfully well with you.

BRANDIN: She is a very smart woman. Okay, so AFIPS basically fell apart, I would say, in about '86, and I believe the ACM guy who would have been the AFIPS-- the one who I would have looked at for AFIPS information—he may have even been president of AFIPS—was Seymour Wolfson.

NORBERG: I remember the name. I don't know him.

BRANDIN: Seymour was enticed out of the ACM fold into the AFIPS world.

NORBERG: There are several things that have arisen now on what you had to say. One of them was the question of this by-law seven. Sammet started a reorganization group when she became president.

BRANDIN: Right.

NORBERG: And that group was led, I think, by Bernie Galler.

BRANDIN: I don't know. Actually, I don't know if she started a reorganization project; all I know is that they were reorganized.

NORBERG: Well, she did; she started it. And then later on, Adele Goldberg started another similar sort of group to look into long-range planning for the association.

BRANDIN: Yes, I went to one of their meetings.

NORBERG: I thought you were chair of the group.

BRANDIN: No, not of the long-range planning—not that one.

NORBERG: Maybe not that one. Board of the Long-Range Planning Committee, 1986.

BRANDIN: I was chair?

NORBERG: No, you were there.

BRANDIN: I don't even remember.

NORBERG: Well, I don't think you got much of a vote from the Council as to something they should...

BRANDIN: My God, it's in a refereed journal; it's another paper! I don't remember this. I remember going to a meeting later on, and I think it was during Paul's [Abrahams] term where I basically said, "You guys have got to think revolution, not evolution," because the Council was too big; the Council was out of control.

NORBERG: It was an important issue for Jean, and she got Galler to chair this group, and they brought in a 25-page proposal, I guess it was, on what sort of reorganization ought to be involved. And by-law seven, my recollection is, it wasn't even mentioned at that point.

BRANDIN: By-law seven was only related to the management of the SIGs, and the most significant thing that came out of it was better management, but it wasn't really-- She had already confiscated the money. She already controlled the money, so the biggest thing that came out of by-law seven was a more effective management, because a lot of the SIGS were going into a kind of hibernation mode, and they wouldn't publish a newsletter, and they wouldn't run a meeting. We kind of canonized, institutionalized the requirements. You know, if you wanted to be a chairman, you had to do these things: you had to publish a report; you had to send a report in to the SIG Board. And in return, she gave us, the SIG Board, the authority to set up our own SIGs. That was basically it. But I don't think that was related to reorganization of ACM.

NORBERG: Okay. Well, by-law seven comes up quite a lot in Council meetings during this period.

BRANDIN: Yes, I think there were lingering issues of the SIGs being unhappy with Jean. But they would have been unhappy with anybody. And then there was SIG-Graph, which was a special problem.

NORBERG: Yes, that's on my list of things to ask you about. The second issue that comes up repeatedly in the Council is publications, and where the money is going to come from, and how much money is going to come through, and who is going to manage these journals so that they're better.

BRANDIN: Or should there be new journals?

NORBERG: Yes, Herb Bright wrote a scathing review of *Computer Reviews*. I think it was directed at Jean, actually, because wasn't she editor-in-chief at that point?

BRANDIN: She could be. Herb Bright and I, we had a clash, too.

NORBERG: Not over this issue?

BRANDIN: No, we had a clashes over the honoraria paid to speakers who gave professional development seminars, and at the time I did it, there was a restriction; ACM would give you perhaps \$100 or \$200 for the whole day's work, and so there was a motion to start paying people who gave lectures on professional development—you know, tutorials—to pay them competitive rates. And Herb didn't want to do it. He said, "These are professionals. We're volunteers. You should be willing to do it for the science," and all that. So when the motion failed, this is when I introduced one of my most famous motions at ACM. I raised my hand, and Jean said, "What do you want, Dave?" I said, "I move we adjourn." Then the motion died for lack of a second. But if Herb was attacking *Computing Reviews*, then he and I would have been the same, because I thought reviews were a waste of money.

NORBERG: Well, I did, too, when I was looking through them in the '80s, but then they weren't doing much with them at all. But Bright wrote a very critical analysis of *Computing Reviews* in 1978. And Jean took umbrage with that, and then there were some public letters that were going around, including one from Herb Grosch.

BRANDIN: This was '78 you mentioned?

NORBERG: Yes, Herb Grosch, '78. He didn't propose that they get rid of it or do something; he said, "It needs serious attention. Let's do something about it. If it can't be made better, then it doesn't justify the cost."

BRANDIN: Herb, who was called and known as *l'enfant terrible*, always cared about the member benefits. At least he said he cared about the member benefit. He always was in favor of publishing the roster; he was always in favor of giving the members benefits, and he was willing to spend the money, even if the money wasn't there. That was Herb's attitude. Herb and I were very friendly. Herb would give me his proxy when he left the Council meeting, and I would give him my proxy, because most of the time, we voted the same way. Herb and Jean were at war, and had been at war since at least '74, perhaps sooner. And there had been a big eruption in '76 between Herb and Jean and Bernie, which was retroactively declared to be an executive session.

NORBERG: How do these people get away with this?

BRANDIN: Well, I made the motion to put it into executive session, and so they said, "Okay, we're making this a retroactive executive session," and they threw out all visitors. It was a very unpleasant moment. This was in Minneapolis, very unpleasant moment. But yes, Herb would have said, "Don't do anything that doesn't benefit the members, and do something that does." He would have been dealing in a clash with people who-- I don't know what their motivations were.

NORBERG: Well, it seems to me, looking at something like Computing Reviews, there's no great gain for membership. What do we need that for? We read the abstract, and we decide whether it's important to us or not, and then we might read it, or we might store it away for some future time?

BRANDIN: I don't know.

NORBERG: It's a very peculiar journal.

BRANDIN: I don't know. And when I got involved in ACM, it was already a well-institutionalized publication, and there was an employee, one of the guys at headquarters, who died probably sometime around '70, who had been heavily involved with introducing Computing Reviews, and I don't know, maybe in the '60s it was an important document. Maybe.

NORBERG: Maybe. I always thought that Computing Surveys were much better, because they had a real feel for what had changed in the profession or in the knowledge base over a period of time. But you don't get that from the reviews.

BRANDIN: No, and you don't need them anymore.

NORBERG: Let's go back to 1974. Jean Sammet gets elected president, having followed Ralston, I think. Or was it Oettinger? I don't know.

BRANDIN: Ralston.

NORBERG: Okay, so following Ralston, and apparently it was a pretty straightforward election. Was it not?

BRANDIN: I don't remember anything about it being contested. I don't think any of the elections were ever really contested. Do you mean in count?

NORBERG: Well, a certain group that might have objected. When Herb Grosch was elected president.

BRANDIN: I don't remember any unrest about that, other than, you know, "I don't like Herb."

NORBERG: No, there were a lot of people who felt that the whole business of nominations by petition should be taken care of differently, because they wouldn't get it otherwise.

BRANDIN: Okay, sure, there was always that. There was that in my election for secretary. In fact, because I was secretary and running for reelection, I was accused of manipulating the counting of the petition signatures. Yes, I think I was accused of manipulating the counting of the petition signatures for Kathleen Wagner.

NORBERG: Yes, looking to see who was still a member and eligible to vote.

BRANDIN: Well, there were always complaints from some group or other; they did that all the time, but the complaint was that I had personally told the counters something.

NORBERG: Yes. I did see that.

BRANDIN: There was that allegation, and I was astonished. I mean, I hadn't even known who the people were who counted the signatures. Or if I knew who they were, I didn't know they were doing it.

NORBERG: Was that a volunteer activity?

BRANDIN: I think so. I don't know.

NORBERG: But there was a period where they would hire somebody on the outside to do it. But I don't know how late that was. Now, Herb Grosch comes along and petitions, gets enough signatures.

BRANDIN: Yes, right. I signed his petition.

NORBERG: And Dan McCracken, the same thing, was the vice president on the same slate. And he and Herb both won that year.

BRANDIN: And who were they running against? It wasn't Sammet.

NORBERG: No, Sammet was the outgoing at that time. She was the past president in Grosch's term. I've only seen pictures of these people; I can't come up with the names.

BRANDIN: I don't remember.

NORBERG: But clearly, they weren't very well known in comparison to somebody like Grosch.

BRANDIN: Well, Grosch was a founding member of ACM. He's one of the few survivors of that original meeting. He's got to be pushing 90.

NORBERG: The last time I saw him, he was at the Smithsonian. They had given him an office. He wanted to come to the Babbage [Institute] and I said no.

BRANDIN: Hal Berghel was taking care of him in Las Vegas for a while.

NORBERG: Okay. But what about Grosch? He wrote these, I was going to say, biting letters in the Communications of the ACM, but it was probably worse than that for some of the people who they were launched against. But he always seemed to be at odds with everybody in the group.

BRANDIN: Yes, yes.

NORBERG: So how the hell did he get elected president?

BRANDIN: Well, the only thing it took to get elected president of ACM was name recognition.

NORBERG: Was his name that well known?

BRANDIN: Well, if he was in front of the voters enough, he would get elected. He had been active in ACM for a long time, and he was a founding member—a very important phrase. How did I get elected?

NORBERG: Well, I'm going to come to that. Also, I talked to Dan McCracken about why he went for petition to run for vice president in that same year, '76 through '78.

BRANDIN: I don't know why he wanted to be vice president.

NORBERG: Why did you want to be vice president?

BRANDIN: I didn't want to be vice president; I wanted to be president.

NORBERG: So it didn't necessarily follow that if one is vice president, they would be elevated to the higher--

BRANDIN: I was never vice president. Mike Harrison was. I wasn't.

NORBERG: That's interesting.

BRANDIN: I was secretary.

NORBERG: Yes, I recall that. I must have just missed that point when I was asking the question.

BRANDIN: Well, we'll get to why I ran for president. But I probably won't tell you the truth. I'll give you polite conversation which is an acceptable...

NORBERG: Wait a minute. I can't leave this issue, because I made a note to myself that you were running for vice president in 1980.

BRANDIN: I don't think so. I ran for secretary again.

NORBERG: It was in the minutes of a council meeting. Okay. If you ran for secretary, what sort of--

BRANDIN: I got beat by Kathleen Wagner.

NORBERG: For secretary? So what did you do during that period? Were you just a member of Council, or chairman of one of the boards?

BRANDIN: In '78 to '80, I was secretary, under McCracken-Denning, and then from '80 to '82, at the beginning of '80, I went off of Council, and then a councilor from Stanford left Council, and Denning and whoever his vice president was nominated me for Council again, so I got back on Council as a substitute from '80 to '82. I may have been doing external activities; I don't remember.

NORBERG: Well, yes, you were, between '80 and '81.

BRANDIN: Okay. And then in '82, I got elected president.

NORBERG: And you were nominated for that.

BRANDIN: I was nominated.

NORBERG: You did not petition?

BRANDIN: I did not petition; I had been nominated.

NORBERG: Why do you think you were elected—name recognition, again?

BRANDIN: Well, Mike Harrison, I thought, was a more distinguished computer scientist—probably a smarter guy, and I would think a better mathematician. But people didn't like Mike Harrison as much as me. And I campaigned. I went to chapter meetings all around the country, and I was giving talks on my-- well, this was '80. Oh, they had just published the Federal ADP Reorganization Study results about a year earlier. So I was giving those talks and Public Crypto talks. I don't know if I was on a lectureship series or not, but I was going to chapter meetings, and I was going to Baltimore to one, to Chicago, so I was out there. My name had been around in ACM for a long time. The SIGs knew me, and I guess I wrote a good campaign statement.

NORBERG: Was the campaign statement edited in any way by somebody else?

BRANDIN: Jim Adams helped.

NORBERG: Herb Grosch accused the nominating committee of censoring.

BRANDIN: Well, there was a lot of bullshit associated with campaign statements in terms of, like, number of words. You were restricted to a certain number of words, and then there would be the question of, well, who counts the words? Does the word processor count the words, or does a human being count the word? And do you count prepositions and articles and commas?

It really was petty. But nobody edited my statement, that I know of. And I don't know of anybody at headquarters ever asking me to do that for anybody else. That was all just paranoia. It was nothing but paranoia.

NORBERG: Well, Herb was the one who brought it up.

BRANDIN: Herb could have brought it up, but Herb used alleged paranoia just in order to drive people crazy. I remember, after Herb was president, he said that he thought that past presidents should get a budget of \$10,000 a year for travel and have a secretary and special stationery. We all thought he was crazy at the time, but now that I'm a former president, I agree with him. I think in the last four years, I've written one letter on my ACM former president stationery.

NORBERG: Do you remember any of the major issues that came up as president?

BRANDIN: Well, the main thing that we accomplished during my term was accreditation. That was done in conjunction with the IEEE-Computer Society, and my point man on that was David Kniefel from Princeton. I was very proud of that. I tried with Walter Carlson, who was another former president, to get the ACM Council to agree to let, I think it was Bell Labs at the time, increase the Turing Award from \$25,000 to \$100,000, but the Council said, "Oh, no, we can't have the Turing Award associated with a commercial name. Screw them. No, no, no, no, no, no," the opposition including Bob Ashenurst, who was one of the most vociferous opponents, from Chicago, and so we failed. I couldn't understand it. We were going to call it the Bell Labs Turing Award—the Turing Award, sponsored by Bell Labs. \$100,000. At the time, you were given a crummy \$25,000. And so it was another, what, ten years before they actually sold their commercial rights for the Turing Award. That was one issue. Tony Ralston wanted me to sign a letter. I think it was for Scharansky, but it might have been for some other Russian, to get out of prison—computer scientist. And I said, "You know, Tony, I'll sign the letter, but it really bothers me that this guy's dues were paid for by the *New York Times*." That bothered me. But Tony said, "Well, you ought to..."

NORBERG: Why did the *New York Times* pay them?

BRANDIN: Well, that's a good question, and my paranoia would say the *New York Times* paid it because it was political, and Tony would say, "Well, you know, the guy is a computer scientist, and he couldn't get out of jail. He didn't have the money, and so who cares who paid him?" That kind of thing.

NORBERG: It's kind of the argument your wife would give you. [Chuckles]

BRANDIN: So there's that issue. I don't remember any...

NORBERG: How about the establishment of the committee on scientist freedom and human rights? Was that during your years, or did that come before?

BRANDIN: I don't think that was during my time, but it could have come from the Computer Programmers for Social Responsibility, which I think was in Adele's time. I never went with those issues. I thought they were too liberal, too far to the left. Well, they are too far to the left,

but the issue of whether ACM should be involved with that kind of stuff, to me, it was always a delicate line of, were we there to support the technology, or were we there to make a social statement, and if given the choice, I would always come down to the side of we're here to support the technology; go make your social statements somewhere else. Like when I was on Council before I was president, one of the issues was, should we meet in Illinois because Illinois refused to ratify the Equal Rights Amendment? They cancelled the meeting in Chicago that was scheduled because of the ERA. I was opposed to that. I thought we should meet. I didn't see any reason why ACM should be taking a position on political issues like that.

NORBERG: Why not?

BRANDIN: To me, we were there to support the technology. That's all.

NORBERG: What was the organizational structure like then? We talked about Council, and we talked about some of the boards like the External Activities Board and chapters and whatever. There were those sorts of things around. Where did the office staff play into this at ACM headquarters?

BRANDIN: Well, there was the volunteer structure, and then there was headquarters. And now there's a new headquarters with a CEO, which I don't understand.

NORBERG: Today?

BRANDIN: Today. But in my day, in the golden years of computer science. We had a volunteer structure where the president was the decision-making authority, and the executive committee managed the budget, and the president appointed the chairmen of various activities, and then the chairman appointed their own, sometimes with the concurrence of the president. The job of headquarters was to follow the instructions of the volunteers. The chair of the Publications Board would review the publications budget, and then send it to the executive committee, which would integrate it into an EC budget, and then send it to Council. Then it would come back down, and then headquarters would get a budget, and they would execute to that budget. The only real money in the organization was in publications and SIGs. Everything else was peanuts. So the External Activities Board, the lectureship series, whatever else there was at the time, they basically got a travel budget, and the chairman of the board—External Activities Board—would meter out its money to people to come to meetings. That was it. There wasn't any money to speak of. So you had the chairman of the Publications Board, which was usually somebody like a Denning. He would work with, oh, is it Mark Mandelbaum who was head of publications at headquarters? And the two of them would get along. The SIG Board had one person at headquarters. But their job was to take the money from the SIGs, because after Sammet impounded the money, we came up with a toll, a dues, a tax. Every SIG member paid 20 cents a quarter out of his dues to headquarters to support the headquarters' expense for supporting SIGs. I remember once, we had a Council meeting where I had brought in someone to talk about cascading levels of overhead from Peat, Marwick & Mitchell. That was really funny. No one on council really wanted to deal with allocating overhead expenses. Anyway, yes, it was Raquel, and she was replaced by someone I can't remember his name, the runner, who would come to the SIG Board meetings, and he would make sure the newsletters got published, and the request for reports got mailed, and stuff like that. But the real control of the organization

was in headquarters, and the executive director was the one who really controlled the organization on a day-to-day basis.

NORBERG: The executive director?

BRANDIN: The real executive director. Yes, the president-- first of all, most of the volunteer presidents were volunteers, and they didn't spend that much time at the job. I was working full-time at SRI when I was president, and I put in 25% of my time in ACM, and I figured it should have been half of my time to do the job right. Sydney Weinstein was the guy who made the day-to-day decisions, and if he wanted to smuggle money one way or the other, he did. He's the one who approved the expense reports, or his office, and Mark spent the money on publications. SIGs were self-sustaining. In fact, they were generating surplus. SIGs and Pubs--Everything else was noise in the system.

NORBERG: Interesting. You mentioned in one of your emails to me that there were some tales to tell—I think that's the way that you put it—about hiring executive directors for ACM.

BRANDIN: Oh, sure. You want that one?

NORBERG: Yes.

BRANDIN: Oh, God. There may be some sour grapes in here. Well, Sydney was formerly-- I'm very close to Sydney Weinstein. He was the executive director when I was president, and he had been executive director since about '70 or so when he came from...I believe he came from GSA. He replaced Gordon Smith, who was a great guy, who had been at UNIVAC and Memorex.

NORBERG: Oh yes, I know Gordon, or knew Gordon.

BRANDIN: Anyway, when Adele became president, she wanted to change executive directors. She wanted me to do it, but I didn't want to do it. She basically fired Sydney, or told Sydney she was going to replace him. It's not as though there was an economic burden on Sydney. He had been at ACM forever, plus he had a GSA pension; the guy was swimming in money. So she puts together a search committee and made me chairman. It was my first search committee. I had three. So we went out and we hired this guy from Harvard. Who was the executive director after Sydney? Do you remember?

NORBERG: I don't know.

BRANDIN: He came on board in Adele's term, and I got him to agree to stay at least five years. I can't remember his name. And he stayed five years, but he couldn't stand Adele; she drove him crazy. So at the end of five years, he quit, and at the time the president of ACM was Paul, I think.

NORBERG: Paul followed Adele.

BRANDIN: Okay, then it would have been John White.

NORBERG: I want to say that it was Frailey.

BRANDIN: Denny Frailey? It wouldn't have been Frailey. So anyway, somewhere around '79, they put a huge search committee together, and then I chaired that search committee. I don't remember who was on that search committee, but I hired Joe DeBlasi, who was a good hire, from IBM. Joe came onboard and became a great executive director. He was a fantastic executive director. He cared about increasing membership. He was a manager. He was a real manager; he wasn't an academician. And he knew the technology, so he was a perfect guy for the job. And he was the executive director forever until he decided to retire.

So they put together another search committee, and made me chairman. So who was on my search committee? Bill Wulf from NSF. I don't remember the others, and I don't want to remember them. During most of the time I had been active, there was a guy on Council from '74 to '86-- no, it was longer than that. His name was Tom D'Auria, regional rep from New York City. Very close friend of mine. Tom has a bachelor's degree in mathematics; he's not a PhD. He speaks like a Brooklyn-easer. So the academicians never really liked him... Plus, Tom was a free spirit. But so was Grosch! Anyway, D'Auria was the longest-serving member of Council in the history of ACM. I think he replaced Bright. He applied for the job, and he was always a membership guy; he always had chapters, and he always was getting speakers to come to the New York chapters' meetings, and he was always asking for more money to increase the membership budget and give it to Lillian Israel, who is a very good person. Joe DeBlasi liked Tom D'Auria.

So anyway, I get the search committee, and we interview three people, and I say, "Okay, I like D'Auria." And the other two say, "Well, all right; that's fine with us." So I say, "Okay," and I go to the executive committee, and the executive committee says, "Okay, offer him a job." So DeBlasi writes a letter; we offer D'Auria a job; he accepts. Then DeBlasi and I took the acceptance, and went to the Council meeting—a special Council meeting. And Council said, "No." Actually it was more like "No, Hell No!"

NORBERG: On what grounds?

BRANDIN: I don't know. I was thrown out of the meeting. It was done in executive session. Joe and I had to leave the meeting. We came back, they said no. They said, "We received the report of the nominating committee. Thank you very much. The nominating committee is now discharged." Not the nominating committee, the search committee. And then the search committee was immediately reconstituted with the same members, except the new chairman was Fran Allen. And they went out, and hired John White. That was the end of my active involvement in ACM, except for I think I had one more year to go on the Awards Committee, no, I was chair of the Fellows Committee on my last year on the Fellows Committee, and then that was it.

Session 2, Tape 1, Side A

NORBERG: Today is August 3, 2007. I'm with David H. Brandin for Session 2 of our interview.

BRANDIN: First off, on the 40th anniversary, there was a big session in Dallas. I was a speaker there. They videotaped that whole session. Here's the program. I got a bunch of signatures on it. I don't know if it's good for history or not, but you can stick it into the archives.

NORBERG: I'll give it to Babbage.

BRANDIN: We haven't talked about SIG-Booze. We do have to talk about that. One of the things I did during my term was, me and Oscar Garcia, the president of the Computer Society, we got into some very serious thinking about merging. I mean, there were several issues. First of all, we both had inadequate sets of competent volunteers. We had adequate sets of incompetent volunteers and a small set of competent ones, and we thought that if we could merge, we could end up with a big set of competent ones. We had money, and the Computer Society didn't have money because the IEEE owned all of their money. But it just turned out to be politically too complex a situation to work it through the IEEE. I think ACM would have been receptive, but the IEEE wasn't. Anyway, Oscar wrote me this letter when I sent him a gift for being elected or reelected or something. That also can go in the archives.

NORBERG: While we're on the subject of the IEEE-Computer Society and ACM, what did you think were the benefits that could be gained from such a pairing after you get the competency issue taken care of?

BRANDIN: Well, first of all, ACM was a theoretical, computer science, software-oriented organization despite its name. The IEEE-Computer Society was a hardware engineering-oriented organization. Both organizations were failing to keep up with the growth of the IT industry. There were a million programmers and the ACM had the same number of members it had three years earlier, when there were half a million programmers. It just wasn't keeping up, and neither was the IEEE-Computer Society. So, it was kind of a natural objective. I wanted to call a new society after the merger the American Computing Society, just like the British Computing Society. And I hated AFIPS. I hated AFIPS because they'd tried to steal the money, and their leadership was even more incompetent. Their volunteers were more incompetent than ours. It turned out that ACM tended to send...I don't know. I don't know what the right word is; I don't want to be too critical, but it didn't send its most imaginative people to be representatives of other organizations. So, a lot of the outside organizations that had ACM representation were weak, they had representatives from other organizations who were weak in the sense that the organizations wanted to keep the sharp ones to do their important work.

NORBERG: Well, I have a list in front of me from 1984, in which all the representatives to other organizations are identified. You topped the list on three of them. AFIPS, because of your presidency.

BRANDIN: I was in the...

NORBERG: Obviously.

BRANDIN: Yes.

NORBERG: CSSP, you took over as well.

BRANDIN: Well, I didn't take it over. I became a member of it.

NORBERG: Okay. And CBMS. I don't know what CBMS is. You were the only representative from ACM.

BRANDIN: CBMS?

NORBERG: That's what it says.

BRANDIN: It must be the Conference Board for Mathematical Sciences, it sounds like. But I don't remember going to any meetings. Oh, yes, right here. Conference Board of Mathematical Sciences. I wonder why they sent me. I was automatically a member. That's why. It was *pro forma*, or whatever they call it.

NORBERG: What about some of the other people on that list? Would you say that they did not have the right set of skills? I'll say it politely: the right set of skills to represent either organization—ACM or the other one.

BRANDIN: Well, by the time I became president, we had effective directors because we were no longer taking anymore BS. I mean we were having it pushed down our throats, but we weren't smiling and accepting it the way we used to do it in the old days. I don't know the guy who went to AAAS Bentley, I assume he was good. Chambers was good. At NCC, Seymour Wolfson was okay. I liked Seymour Wolfson a lot. In fact, he's the one who was on a Nominating Committee that nominated me for president. But when Seymour got sucked into the NCC and AFIPS, he kind of became more... I felt his loyalty switched. Not loyalty. His agenda switched there. Walter Carlson was great. I'm crazy about Walter Carlson. The ICCP representation I was always a little skeptical of, although I don't know Sam Redwine. But there was someone else who was also the ICCP with George Luna [?].

NORBERG: Dan McCracken, I think, was on that group. Just before he was president, I think.

BRANDIN: Could be.

NORBERG: Because he signed a number of his presidential letters with the...

BRANDIN: He had the CDP, but I don't think he was a representative to the ICCP. There's a certificate that they gave. Let's see... I was going to say something, but I forgot it.

Also, some other things that happened during my term. As I said, I inherited a financial no-budget from Peter, so we got a continuing resolution. And the reason that he was unable to get a budget was because there was just too much contention on the Council. My feeling at the time was there was too many people there, and there were too many agendas, and the people who would be in charge would just kind of push their weight around. So I decided that I wasn't going to push my weight around; I was going to be fair. And if you ask the Council members during

my term, they will say that I was fair. So, I gave everybody their audience. I never locked anybody out of the debate, but I didn't force the rule that says that if it isn't on the agenda, it can't be discussed. I learned that from Sammet. But I allowed them to introduce stuff on the agenda before the meeting, and we could always amend the agenda at the beginning of the meeting. Everybody who wanted to speak got a chance to speak. They got into a queue.

There was one episode I remember, again, in a Disneyland hotel, where there was some contentious issue and the vote went 12-12. So I voted to make it my way, whatever that was. We had a break, and I came back and I said, "You know, I don't like the fact that this was contentious. So with the permission of the Council, I'm going to allow us to reintroduce this issue and debate it for another five minutes later." Oh, everybody thought that was wonderful. And then the vote came down, and it changed, so I didn't have to vote. And it went against my way. I tried to run a smooth, on time, unbiased meeting-- I had my own agenda, but everybody had the right to do whatever they wanted to do, and that was a change from the previous councils or from the previous administrations. Up until then, they had fought tooth and nail on the floor. Peter and Dan would, in the Executive Committee meeting sometimes, [chuckles] sometimes Dan would get so mad at Peter that he would turn his chair around and look at the wall. When we'd be in executive session, Peter would be fuming, so I'd have to kind of take over the meeting. It was strange.

I also want to tell you about my experience with Grosch. I also had problems with Herb. He was disruptive. He was in one of my Executive Committee meetings. We were in New York. The rule was you could speak if you were at the table and if you were a member of the Committee or an invitee. You know, a classic constraint. Herb is out there in the audience in the chairs along the side of the room. Raising his hand, talking without raising his hand, being disruptive as hell, and constantly wanting to butt into an issue. So I wrote a note to Herb "Herb, if you will raise your hand, I will acknowledge you, put you in the queue, and let you come to the table and say what you want to say. I don't care what it is, but you've got to do it according to the rules." And Herb shut up, raised his hand, got in the queue, came to the table, said whatever it was he wanted to say, and I never had another problem with Herb. I found that interesting.

NORBERG: Yes, I do too, considering some of the letters in the ACM Forum by him and then by successive officers between 1978 and about '84, I think.

BRANDIN: His most recent letter that I saw was one that said, "There'll never be another president like Dave Brandin." That was nice.

NORBERG: Was that a letter, or was that in the Forum?

BRANDIN: I think it was in the Forum. That was sent...I've got a copy of it. I think it was sent to the president. He was complaining about something, as usual. Probably the nominating process.

NORBERG: I want to come back to that.

BRANDIN: I've got Grosch; the EC meeting; IEEE; fairness, which I thought was very important; and then we have SIGGRAPH, which is a very special case. I had a lot of problems with SIGGRAPH. I felt that the volunteers abused the money. They were running these parties

that were-- They were spending *beaucoup* money on the receptions. Now, it's true that they were selling a lot of space and they were making money, but I thought they were living a little too high off the hog. Plus, they would not pay attention to the rules. They were basically a rogue operation.

NORBERG: Were they the only rogue operation?

BRANDIN: No. I mean, occasionally, some of the others would, but this was--

NORBERG: I wanted to hear about the SIGMAP controversy. 1976.

BRANDIN: Again, there's somebody from SIGMAP from Stanford, if I could only remember his name. But SIGGRAPH, just as my term was ending, wanted to sign a contract with IMAX in Minnesota, I think.

NORBERG: Science museum.

BRANDIN: They sent the contract or something, and it was a big financial commitment. I said no. I wrote them a letter and I said, "Do not sign this contract. You do not have the authority of the association." Well, they signed the contract. And then Adele took over. I told Adele that she should get the lawyers in and, you know, you should fix this problem and throw these guys out of office, but she just let it ride for whatever reason she had. Somebody can ask her about that.

NORBERG: Now, this is not the film that was about computing and personnel, was it? That McCracken was involved in and it played--

BRANDIN: I don't remember. No, I think it was a graphics thing. You know, just a big graphics thing. They wanted the theater to come in. They did do great stuff in graphics.

NORBERG: I don't think you'd need that big screen for that, though.

BRANDIN: But I always had trouble with the Tom DeFanti from UI. He was always pushing the envelope in terms of ACM. And of course, everyone pandered to SIGGRAPH because it was generating a lot of money, but I felt it was being poorly managed. So that's about all I could remember overnight.

NORBERG: Okay. Last time, and now for a bit this morning, we talked mostly about personalities in connection with various activities within ACM or with other kinds of activities involved in computers. Today, I'd like to start with some organizational issues where the people, aren't so important—to me, anyway. You may find it's necessary to put them in. But the structure of the organization-- and I brought a copy with me of a 1985 pair of charts: general organization of ACM, and then the next page is the headquarters' office staff. Was this a new definition of structure? Would this come out of the 1975, for example, reorganization? Or when you were president, you remember this as the set?

BRANDIN: This looks like the way we ran the organization from the time I was chairman of the SIG Board on, so that would've been roughly '74. Now, I don't know if Jean did things before that, but this was the ACM that I knew. One of my problems with this organizational was that this chapter organization was sitting at the same level as everything else, so it gave it a voice here that would have the same weight as other entities.

NORBERG: Two groups.

BRANDIN: So the SIGs, which were doing all the technical work, and the pubs, which were spending all the money, and the chapters had the same voice, which just didn't make sense to me. It was overrepresentation. But this was the structure. Let me look at the headquarters. [Examines paper] Yes, I guess so. You've got look at the-- I'd like to know their names.

NORBERG: Yes, I didn't find their names attached to them. I'd have to look in various places to assemble them, and I didn't think it was really worth it.

BRANDIN: Well, I think the director of the Office of Pubs was Mark Mandelbaum.

NORBERG: I remember the name, seeing it.

BRANDIN: Office of Membership Foundation and Corporate Services. I don't know. Maybe Jim Adams? Or maybe he was Professional and Technical Services. One or the other. Finance and Administration was John McKegney. The DP Operations...I don't remember. But by the way, the IBM computer that ACM got during Sidney Weinstein's reign was because of me and Tom D'Auria. Dan McCracken had arranged for ACM to get a, I think it was called a NCCS machine or some other little DP system. I think it was one of his clients, perhaps. And they had this little machine. It just couldn't keep up with the requirements, so Tom and I started working the Council, and we eventually got them to set aside enough money so we could buy an IBM 4300. An IBM computer, even though I was on their hate list. We were very happy with that machine.

NORBERG: Yes, I remember one of your letters. It might be the last one, when you were summing up what happened within your term, that you mentioned the restructuring of the office with a new IBM computer and so on. So, that did happen in those years.

BRANDIN: Yes. I'm not sure we get the machine-- I think the machine showed up before I was president. I'm not sure. I know on I was on Council at the time, but I don't remember when we got the machine. It would have been after '81, I suspect. Or maybe it was still during the litigation. I have my letters, too. Should I go get my Communications? Or do you have them all there?

NORBERG: Well, I have only some relevant ones or ones that I thought would be important.

BRANDIN: That was when I took over.

NORBERG: The association's accomplishments, and under that, there's the Turing Award. The National Computer Conference level of publication. The business aspects of it. Nothing is mentioned here about new computers, so that must've been earlier than that, as you just said.

BRANDIN: Has a date on that.

NORBERG: 1982.

BRANDIN: Oh, yes. This was not the first letter that I wrote. The first letter I wrote was called "Metamorphosis."

NORBERG: I left that back in the room. Yes, I do.

BRANDIN: That was when I took over from Peter. Adele, by the way, was doing all the graphics on these letters.

NORBERG: Yes, there was a short piece that she and somebody wrote in Communications.

BRANDIN: I gave her space to publish her own letter. [Break] Yes, I sat out there in the garage and shredded for two days. It's not in here. Here I say in this letter, "Our bureaucracy impedes us. Our family squabbles dissipate our ability to respond." And they did. But this was Adele's bitmapping. Pretty slick. Peter didn't like it.

NORBERG: For that time, it was pretty slick. Just coming into its own.

BRANDIN: Yes. So, anyway, we bought ACM a new computer, and everybody was happy.

NORBERG: Okay. I want to ask a question I asked you twice yesterday, but I want to see if I can get some more clarification about it. Were there any major changes being discussed when you were on the Council, when you were president, that came into being? Whether it's change to the organization, changes in distribution of funds...anything that was being talked about that would have changed the organization substantially.

BRANDIN: Well, there was the administrative control of the SIG money that occurred. There was a great debate about how long we could rely on AFIPS' money. There were spending issues that would periodically come up, like do we need someone to manage conferences for SIG? I mean create a new job. But those weren't of monumental proportion. The biggest things that were being discussed were being discussed at the AFIPS Board meeting, like should they launch a new magazine?

NORBERG: Yes, *Abacus*.

BRANDIN: *Abacus*. That was one of Tony Ralston's pet projects. Pretty sure it was Tony. They spent an enormous amount of money on that project, and it was a complete bust. Complete bust. We didn't start talking about competition from COMDEX at ACM. Those were discussed a little bit at the AFIPS Board meetings, but only near the end.

The structure of the Council. I used to talk about how I think the structure of the Council should be smaller. When I went to a Long-Range Planning Committee meeting, after I was no longer president, I said, "Revolution." It was basically unmanageable.

NORBERG: What's unmanageable? There were about 26 members, weren't there?

BRANDIN: There were 24 people. Oh, I'll give you an example. When we decided to buy a computer, we had 24 computer scientists that wanted to specify the machine. Yes, it was crazy. And then sometimes, they wanted to edit a document. A committee, as a whole, wanted to edit a document. [Chuckles]

NORBERG: Yes, that can lead to all sorts of problems. Well, what I was after here is, there was a discussion in the late '70s. I don't remember how late, but it was a discussion about the makeup of the membership in terms of their professional affiliations, whether there were too many academics in there or research people, or whether they were mostly industrial people and so on, there seemed to be two camps about this.

BRANDIN: With all due respect to the tape recorder, that's a lot of bullshit. [Laughter] First of all, there were people like D'Auria and DeBlasi later on. And Lilian Israel, and me, and Adele and some others who really cared about the membership constituents. We were willing to spend money to try and do something about it. The others talked about it, but they never did anything about it. That whole organization, and after a time, was driven by its journal and reviews and surveys and the launches of new refereed journals. There was never any serious attempt to approach the programmer in this world. Look at Communications. Communications, to this day, is not directed at the programmer. And you've got all these other programming societies and organizations that sprung up because ACM was asleep at the switch. I...was this after my term? AAAI is an example. ACM had SIGART, the first artificial intelligence organization. And then, what was her name? She worked for me at SRI. The chairman of SIGART says that Raj Reddy and Nils Nilsson and a couple other AI-ers want to set up a new society, and they wanted to know how SIGART felt about that, and SIGART told them that it had no problems. And I'm saying to myself, "Why would we say we have no problems with that?" So all of sudden, they set up a rump AI organization, the AAAI, and they're out there giving awards, they're out collecting dues, they're out there making publications, they're out there running conferences, they're competing with us. And our chair, who's an AI scientist who works for one of them, is saying, "Yes, go ahead and do it, go ahead and do it." That was the attitude here because the AI guys were intellectuals. They were scientists. And it was going to be a more formal organization. So, as far as I'm concerned, most of the ACM has always wanted to pander to their own reviewed papers, science, and theoretical base. And there never was any real serious attempt to manage. Lillian, who's a staff member at headquarters for growing membership, was always asking for more money to get promotional stuff and memorabilia and run booths. And Communications, to this day, is useless. Well, it meets a sub-journal category, as far as I'm concerned.

NORBERG: But wasn't that intended?

BRANDIN: But it doesn't go to the mass market. It still goes to the existing ACM market.

NORBERG: But isn't that true of the other organizations, too? Like IEEE-Computer Society?

BRANDIN: Well, I think Computer is a much more practical magazine. And I think it always was. These decisions are all controlled by the Publications Board, so they're picking the editors and they're picking the papers and they're picking the reviewers. So, this is what they want. Hasn't changed. That may be why they didn't want to hire D'Auria.

NORBERG: Because he was not an academic?

BRANDIN: Because he was not an academic, and he was clearly interested in broadly expanding the membership outside of the academic.

NORBERG: Wouldn't that be advantageous, to have extended the membership beyond this small group?

BRANDIN: Oh, that's what D'Auria thought, that's what I thought, that's what Joe DeBlasi thought. It seemed like that's what the Executive Committee thought when they unanimously endorsed the job offer, and then the Executive Committee folded in that Council meeting. But you have to ask Peter Denning, who was in the meeting, about it.

NORBERG: I will. So, can we conclude then that ACM was more a computer science group than it was an applications group? If I can make that distinction.

BRANDIN: Absolutely.

NORBERG: Okay. Well, how did your colleagues define these terms? We'll leave Peter aside for the moment, but what about McCracken? How did he define it? He was not-- well, that's not true. I was just going to say he was not an academic, but that's not true.

BRANDIN: Well, I never considered McCracken an academic.

NORBERG: He started teaching in '81.

BRANDIN: I feel like more like McCracken-- Well, he may have been teaching, but I never really thought of him as a theoretical computer scientist. He wrote a book on programming in the days when it wasn't even called computer science. And it was a good book. For the time, it was a great book. I used it. If you have to categorize the presidents before me, out of all of them, I think I'm more like McCracken than I am like any of the others before me. After me, there were a couple other...

NORBERG: How does Sammet fit into that picture?

BRANDIN: Well, I always thought of Sammet-- even though she wasn't, let's say, an academic theorist, she was mucky muck at IBM in programming languages, and programming languages themselves require more than just programming. So it was more challenging.

NORBERG: Yes. I can't think of any of the others that would not fit the definition that you just gave in terms of seeing themselves as academic computer scientists in whatever way they interpret that to be. Sammet wrote an awful lot of letters, and she's still sitting on them, so I haven't been able to get a look at them. They offered this to CBI years ago, and I went out to see her in New York State, and she was planning on giving us all the papers that she had. She said she wasn't finished with them yet, so she would give us half the collection that she was finished with, and we'd get the rest later on. Well, she sent us, I don't know how many, but it must have been 80 boxes of material, which we put on a very, very high top shelf knowing we would never be taking them out. [Chuckles] And the good stuff that I had seen in New York, it wasn't there, so I called her and we talked about it a little bit. She said, "Well, you'll get them, you'll get them, you'll get them." But over the time, she has not been very cooperative, so I rather suspect we may not get them, that Babbage may not get them. Which would be a bit of a shame because some of her correspondence, some of the stuff I have here, for example.

BRANDIN: But who would she give it to instead?

NORBERG: Computer Museum here in Mountain View.

BRANDIN: Oh. Good old Gwen Bell's...

NORBERG: Yes, but let's not talk about that.

BRANDIN: Hey, I'm a founder. I made a founding contribution. I was also a member of the Computer Museum until I discovered that my membership category was dissolved. [Laughter]

NORBERG: Computer Museum of Boston? Or the one here?

BRANDIN: Well, when she set up the one in Boston, I gave her 200 bucks. It made me a founding contributor. And I also got ACM to become a founding contributor in the Museum by giving her \$2,000 worth of...I think it was the programming language tapes, or something like that. And I gave her Shaky the Robot from SRI, the first autonomous robot. Self-navigating. He is now here in Mountain View, Shaky. I have a picture of me sending Shaky to the Museum.

NORBERG: I've seen pictures of Shaky.

BRANDIN: I'll show you pictures of him. Well, I'm sure she has great letters because she and I exchanged a lot of letters during the By-law 7 day.

NORBERG: I was thinking more of the controversies around the first History of Programming Languages Conference. Not you in controversy, but Herb Grosch, who, after the fact, after the program had already been developed, wanted to go in and tinker with it and take charge. She really lambasted him on that one.

BRANDIN: But you know, most of that stuff was noise, those little controversies there. There was always something going. Every Council meeting had what I described as a "surprise big deal." There was always something that came out of nowhere. Or you had it on the agenda, but you thought it would take two minutes, and it turned out to be three hours. Every single member

of the Council had to give their same opinion five times. I'd be sitting there and saying, "Thank you very much. Oh, don't forget we have a rule in Council: We don't make 'me too' speeches. The councilors are supposed to be listening to the other councilors. They don't have to hear the same thing twice." And then somebody would go ahead and completely repeat everything all over again. [Laughter]

NORBERG: Yes, I've been to meetings like that, too. Then, would you say that any particular group within ACM benefited more than the rest of the groups? Now, I'm talking about the ACM organization, not a SIG group that goes out and entrepreneurially makes its own money.

BRANDIN: The only complaint I would have would be-- First of all, most of the money was spent in the area where it was generating the most. Publications spent the most money, but they generated a lot of money. SIGs didn't spend a lot of money and they generated a lot of money.

NORBERG: How did they generate the money?

BRANDIN: Conferences, newsletters, subscriptions, dues. I think in my term or during my day, we published around 15,000 pages a year of referee journals. We also published 15,000 pages a year of conference proceedings and SIG newsletters. And those were either paid for by their dues or they subscribed to the newsletters. These were informal research reports.

NORBERG: Yes. Now, when a SIG held a meeting, what sort of fees were being charged? And I realize that we've got a whole issue to hear there.

BRANDIN: I don't know. I mean, it would be 200, 300 bucks for a conference. They were running real conferences. And on top of that, there was the SIGDA, which we ran a joint meeting with the IEEE-Computer Society in design automation. That was a big moneymaker. SIGGRAPH was a big moneymaker. And then all the other SIGs had to run at least one conference a year to maintain their status.

NORBERG: Because it's not usual in the case of academic groups, certainly. If you belong to the American Historical Association and you go to the meeting, it's \$50, \$60, and most of that you get back in the material that's been printed and various other social activities which were going on. When I first became director of the Babbage back in 1981, it was suggested that I go to some ACM meeting. I don't remember what it was now. When I saw the fee, it was \$235. I said, "Look, historians don't do that. We don't have that kind of money. Where's it going to come from?" And I always wondered, when I was talking with my computer science colleagues in the University of Minnesota, who paid for them.

BRANDIN: Well, I don't know who's paying for them. We can talk a little about stuff in the '80s where I was heavily involved in the trade show business. But I don't know. I assume the SIGs were charging a couple hundred bucks to 1,000 bucks depending on what the event was, whether a tutorial is included, how many days was the meeting, did they throw in lunches or receptions or coffee, and were they giving chocolates chip cookies or not. Another SIG that I think had pretty good meetings was SIGCOMM. I was always upset with SIGCOMM too because they didn't get in the way of Vint Cerf when Vint set up the Internet Society because,

again, there was an opportunity for ACM to be bold and to spread its wings into a different flatland, and they didn't go. They let somebody else do it.

NORBERG: Is that because of the lack of vision, I'll say, rather than incompetence?

BRANDIN: No. I think it was the perception of the organizers that ACM was a bureaucratic morass that would get in the way of them doing what they wanted to because we had by-law 7, which said you can't do this and you have to do that. And these guys, they didn't want to have to get the approval of a Council or have somebody tell them that they can't name an award after somebody or stuff like that. They wanted to be independent. And then our SIGs pandered to them.

NORBERG: Just to make sure I understood what you said a little while ago, was there literally nothing for programmers and analysts coming out of ACM?

BRANDIN: Well, yes. We had SIGs like SIGMINI and SIGMICRO. But if you were an industrial programmer at Standard Oil, I don't know, doing database work, there was VLDB, Very Large Databases, and SIGMOD, Management of Data. So there was some stuff there. But let's say you went to...I don't know, took a school that was not known for its computer science program.

NORBERG: Drexel.

BRANDIN: That was an engineering school. Like Northern Illinois University. You got a degree in mechanical engineering or chemistry or whatever, and then you get a job as a programmer. You have had no interaction with ACM. They don't even know who they are. None of the teachers are members. And there's two million of them now in the United States that didn't go to Stanford who have never really had any interaction with ACM. And when they do have interaction with ACM, they pick up the journal of the ACM and they think they're reading Gauss.

NORBERG: [Laughs] That was a very apt example.

BRANDIN: Speaking of Gauss, by the way, when the fifth-generation computer guys in Tokyo published their-- they started a new journal called New Generation Computing and they asked me to write the preface.

NORBERG: What's the date?

BRANDIN: This is Volume 2, I think. Or Number 2. The date on this is...oh, that's a good question. I was still at SRI. No, it must have a date.

NORBERG: Well, they don't always have a date.

BRANDIN: But this thing is all the same. These are all copies of the same document. 1984. New Generation Computing, Volume 1, Number 2, 1984. But I don't know the month.

NORBERG: That's not relevant, actually.

BRANDIN: But you can have one of these.

NORBERG: Thank you.

BRANDIN: It's very interesting. I managed to get to the Pythagorean, Gauss, Pascal, Leibnitz, and a few others all on the first page. And Archimedes, too.

NORBERG: It looks like they fit.

BRANDIN: The Japanese, with the Fifth Generation, really scared ARPA, you know. These guys, they really threatened ARPA. Turns out it was a big bust.

NORBERG: Well, yes. I followed it for a while. I reviewed the Feigenbaum-McCorduck book for what was then *Science* magazine that AAAS was putting out. That one went, I think, about a year and a half, and then there was simply not enough interest and they backed away. Spent a lot of money on it, just like AFIPS spent on *Abacus*. But it was not directed to an audience that really was interested.

BRANDIN: I know. Oh, we're not through on this subject. [Laughs]

NORBERG: Oh. Well, that's fine.

BRANDIN: This subject is a sore one with me and Harrison. Harrison was one of the editors of Addison-Wesley for certain books. And he still is. When we decided to write *The Technology War*, we went to Addison-Wesley and asked them if they wanted to publish the book. They sat on us for a year while they published Feigenbaum's book. My conclusion is that they sat on it because our book was a competitor to Feigenbaum's book for that subject. I mean, we were talking about competition and all. We finally, after a year of waiting for A-W to come along, and then after they basically stonewalled us. We went to Wiley and got our book published. Of course, they didn't know how to market, either. [Chuckles]

NORBERG: Yes. I've had that experience as well. How close were you and Michael?

BRANDIN: Well, there were times we were very close. I don't have a problem with Michael.

NORBERG: I don't, either. I know him.

BRANDIN: First of all, he was vice president during Peter's term, and I was secretary for a while. Was I secretary then? No, I was secretary on McCracken and Denning, and then it was Harrison and Denning. I was their External Activities Board guy, and that's when I wrote the letter to those guys asking, "What do you want me to do? How do you want to be represented?" And then they returned me to the Council. But Mike and I were friendly. We drank together. There wasn't a lot of technical activity together because he was off on his automaton computability stuff while I was really into managing research and consulting. And then, in '82, when I got nominated, Mike got nominated. I ran against Mike for president. And I beat him,

unless somebody manipulated the numbers. The Supreme Court might have been involved in this one. [Laughter] But I beat him, and I think he was--

[Session 2 – Tape 1, Side B]

BRANDIN: Anyway, Mike and I ran against each other, and I beat him. I'm sure he was shocked. I was surprised that I was able to beat him. But we stayed friendly after that, and then we wrote *The Technology War*. I think we wrote it between '85 and '87. Took us about two years to write the book—a year longer than it should have.

NORBERG: There was a lot of data gathered in there.

BRANDIN: Well, the data had already been gathered, pretty much. I chaired a Department of Commerce Japanese Technology Study in '84 on computer science. Mike had chaired a National Academy panel on international microelectronics. We knew what everybody else was doing. I was in Japan often, so I knew what NTT was doing and Hitachi and Fujitsu. Through the consulting, we were able to collect a lot of information, so there wasn't that much research that had to be done at that point in time. Then we published the book. In fact, the research was done in '84...I think it's '84. I've got to get some more documents. [Break] Those two, study of the National Academy and the Commerce study, we had travel money, and we went around collecting a lot of information on those studies, so we had completed the research on those two studies.

NORBERG: Who were some of the other members of the Department of Commerce group?

BRANDIN: Oh, God, I don't remember.

NORBERG: Was it a large group? 45, 50?

BRANDIN: Oh, no, no, no. It was more like ten people. Even ten was too big because I remember we wanted to... Maybe this was Mike's panel instead of mine, but it was on one of those trips. We went into a meeting in Japan at probably an NTT laboratory. What happened was, everybody has to ask a question. You have a one-hour meeting with the chief research engineers at NTT and then everybody gets to ask one question. You never get to get any information. Because politically, you know, protocol-wise, everybody's got to ask a question. They all have to look smart. So I found a lot of those meetings were useless. But one of the things that came out of that was, I was asked by MCC to help them overcome any antitrust objections to their collaborative research projects. And I wrote these two reports.

NORBERG: Well, they seemed to have helped because...

BRANDIN: Yes. Well, I was very friendly with the chairman of MCC at the time, who was Admiral Bobby Ray Inman. He had actually offered me a job to be his...oh, I don't know. I don't want to use the word "spook." To be his chief international intelligence competitive assessor in Austin. I wanted to take the job, but we had a home in the hills with a slide problem and I couldn't get out of the house. So, I ended up turning the job down. But I knew him very

well, and I liked him a lot. He's the smartest man I ever met. I got to know him through the Public Cryptography Study Group, which is also something you might to look up.

NORBERG: It's on my list here.

BRANDIN: Okay. Well, we'll come back to it. So anyway, that's the structure at ACM. It was the best way it was working. It didn't strike me as particularly inefficient or efficient. I always felt that headquarters had a soft job. It was a soft job.

NORBERG: Interesting. There are a lot of societies where that is true, but as I told you yesterday, I sensed, when I was talking to Jean Sammet and Aaron Finerman and Bernie Galler and several others when they were with AFIPS and I was running the Babbage Institute, that their idea of a staff member is-- this goes especially with Aaron. Their idea of a staff member was somebody who would do what they were told.

BRANDIN: Yes, but that would be the executive director. Down below the executive director was laissez-faire. I didn't think that there was a lot of management pressure that was coming down through below the main executive director. Sidney was a very relaxed, easy-going guy.

NORBERG: But yet, they did a lot of things. They conducted surveys for the Council and through the--

BRANDIN: Well, they did all this mechanical stuff, but they came at 9 and they went home at 5, and nobody had headaches. Stuff like that. I think that the leadership at headquarters now...well, maybe I shouldn't say this. Is a little too distant.

NORBERG: From whom?

BRANDIN: John White doesn't live in New York. He doesn't even live in the United States.

NORBERG: Oh, that's not helpful.

BRANDIN: He lives in London. And they hired him knowing that he was going to live in London. Then he made Pat Ryan the COO now. So they hired John and they made him a CEO. We have never had a CEO before. I've never heard of that title before. I have no idea what he's getting paid. He's living in London. I think they're subsidizing or at least paying for travel. Pat is the day-to-day operator. Pat, I don't know if she ever worked anywhere except ACM. She used to be Irene Hollister's assistant in the Jean Sammet days, so she knows everybody and grew up with the organization, but I don't even know what her formal training is.

NORBERG: How important were things like numerical analysis and scientific computing to the ACM leadership?

BRANDIN: They just gave a Turing Award to Fran Allen for it.

NORBERG: Well, I'm speaking now back in your time. Second half of the 1970s.

BRANDIN: Well, simulation numerical analysis, computation analysis, computation, was very important up until, I don't know, let's say '75. By '75, they were building machines that had so much hardware in them, and so much memory, and the cycles were getting so fast that simulation was no longer as expensive as it used to be, so they could do other things and they didn't have to throw all their resources at just solving the simulation problem, the computation problem. So I think that's when there was an explosion in programming languages and stuff like spreadsheets and all that other stuff. Word processors. I believe we were doing word processors at SRI in the '70s. And the ARC. Doug Engelbart's ARC was a collaborative workstation environment that could now grow up because the machines were fast enough and the displays were getting better and all that. So I would say by the time I took over computer science at SRI, which would be '78, computation was no longer a big deal. There were still LP problems and there was a lot of mathematics going on, but the Cinderella topics in the late '70s and early '80s were artificial intelligence, ARPAnet, Internet, packet switching, packet radio, fault-tolerant computing architecture. Computer security was a big topic. Robotics. Vision. I mean, we were getting money. We were getting about \$12 million bucks a year in my division in '78, of which two thirds came from DOD. Command and control was beginning to be an issue. So I would say numerical analysis was falling off the ladder.

NORBERG: Okay. That's in the larger profession, but how about among the leaders of ACM?

BRANDIN: Oh, I don't think they were numerical analysts. They were programming language and automaton, computability people. They were theoreticians.

NORBERG: Okay. Now, you've made a big point of telling me about AFIPS. You mentioned AAAI as a new organization that ACM didn't have much to do with. What about DPMA?

BRANDIN: You should talk to Jim Adams about DPMA, because he worked for both organizations. He lives in San Diego, by the way. DPMA was-- I mean, the guys themselves were all right. They were data processing managers. They ran big data centers. You know, big shops. And they had their own association. There were about 50,000 of them. And they were well managed. They came into AFIPS, and then they wanted their bigger piece of the pie. But I don't have any complaints about DPMA.

NORBERG: Well, I wasn't fishing just for complaints. You might be wanting to praise the organization.

BRANDIN: I mean, I would praise it only from the point of view that the guys that I ran into at AFIPS...I liked those guys even though we were competing for the money. They were rational-thinking creatures. Now, should they have been part of ACM? Maybe. There were other professional societies like Simulation Councils. I don't even know if they're still around. But should they have been part of ACM? Yes. In fact, they probably should have been the simulation SIG instead of a little thing I set up.

NORBERG: Okay, then let me sort of generalize my next question to you. Among all of these topics we've been talking about in the last half hour or so, it gives me at least an impression of what the role of ACM was as the various officers saw it in the 1970s. It sounds to me as if there was more attention being paid to getting publications out, getting some sort of conference be run,

and so on, but no real thought about which things ought to be specified, which things ought to get a big push at this time. Maybe some other part of it did feel that they ought to be pushed at another time. I don't sense there were any such discussions.

BRANDIN: Well, here's where I would say, "There were discussions at the SIG level about: "Shall we form a new SIG," because microcomputers are out now. Or should there be a new SIG? Should graphics be a SIG? Or should software engineering be a SIG? Minicomputers? At the SIG level. And then there would be a SIG in that area, and then there would be informal publications, and eventually we'd have a little conference, but it all was grassroots technical stuff. At the Publications Board level, they would talk about, "Do we want a new journal?" But IEEE's are transactions. Were ours transactions?

NORBERG: Yes, there are some.

BRANDIN: Anyway, they're refereed journals in this particular topic, so that would be handled at the Publications Board. Now, the only time there would be deep, philosophical, central management from the guiding lights of ACM was when the budget would come to the Council. During the budget discussions, there would be serious discussions about whether we want to spend money on a new journal. I'm not sure who decided whether a SIC became a SIG. There was some permanence associated with one that didn't exist with the other. The SIG Board chairman would give a report, and sometimes it would be a technical report. I didn't give too many technical reports. I used to give performance reports and membership reports and show how every time they raised the dues, our membership would drop and stuff like that. But I would not say someone was sitting there thinking, "How are we best going to grow the organization?" The organization default was support academia, research, and theory. That was the default. The assumed default. That computer science conference was run specifically for graduate students.

NORBERG: That's the ACM meeting?

BRANDIN: Mm-hmm [yes].

NORBERG: Once a year.

BRANDIN: Mm-hmm [yes].

NORBERG: And other meetings were in the SIGs, apparently.

BRANDIN: Well, yes. The other meetings were in the SIGs, right. ACM used to have an annual meeting, and then they kind of went away.

NORBERG: So, the annual meeting is different from the one that was for graduate students.

BRANDIN: Yes. The annual meeting was a technical meeting. They would solicit papers. I don't think they were reviewed. It was just a list of papers. And we would have all our business meetings. Then that kind of got preempted, and we would have our business meetings at the

computer science conference, or we would go to SIGGRAPH and have our business-- or we would do it then at NCC.

NORBERG: Was there a defined role that everybody would salute once you dragged it out onto the table? For the organization in the late '70s.

BRANDIN: Would they all march to the drum, you mean?

NORBERG: More or less, I suppose.

BRANDIN: Well, they marched to the drum on accreditation. I don't know. It's not like this was General Electric and one of us was sitting up there saying, you know, "Okay, do this. Go out and get us into the diesel engine business."

NORBERG: All right, let me try to approach it in a somewhat different way. The ACM SIG Board that you thought were pursuing a specially interesting innovative work-- The subject came up by...what was her name? Portia Isaacson? Who was interested in having a PC group. And so there was all this discussion about whether or not SIGMICRO should be folded into that and various other things. You sent her a cautioning letter.

BRANDIN: I did?

NORBERG: Yes, you did.

BRANDIN: What did I say?

NORBERG: A cautioning letter. "We probably won't be able to get this done the next meeting, but I think it will happen after that. So be patient." And she was patient. It was passed by the SIG Board. I don't know whether it ever came before Council.

BRANDIN: Well, why would it have to?

NORBERG: All right. I've not seen any reference to it in Council meetings for that period.

BRANDIN: Well, nothing would come up unless there was an unhappy volunteer that complained to the Council that the SIG Board didn't accommodate them. And then, somehow, they'd have to find a pet councilor to bring it up on the floor, so I must have accommodated what she wanted to do. But I don't remember sending her...

NORBERG: Okay. It seems like you did appreciate what she was trying to do, but it grew to a larger question when all these other SIG groups came into the picture.

BRANDIN: I remember Tony Wasserman was a SIGSOFT guy, and he was driving everybody crazy because it wasn't clear at that time that we really wanted a Special Interest Committee or whatever in software engineering, and he wasn't getting the message. I don't want to say any more about it, but there were times that people would be pests about it, and arrogant, and unrelenting, and there were others who knew how to get along with people. And the ones who

got along with people were the ones who got what they wanted. It's just like any other sociology experiment.

NORBERG: There was an important-- well, I won't say that it was important. One member of Council claimed that the December 1978 meeting of the Council was a watershed for the organization.

BRANDIN: Really?

NORBERG: Yes. Let me see if I can find the comment. Because many things had been decided, and it seems to me that after that meeting, ACM was what it is today. So a number of things must have been pushed aside at that point, but I'm not sure whether that's true or not. "The December 1978 meeting has been referred to by at least one member of the Council as a decisive one for several areas. Alteration of the membership dues and journals. Changes in editorial positions. Delegation of authority lower in the organization was appropriate. Legislative activity, such as stronger support for ICCP, to prevent state certification," which is what they were somewhat concerned about at the time. "Copyright issues. The ACM film project." Can you elaborate on any of these?

BRANDIN: No. Read the list again.

NORBERG: Sure. "Alteration of the membership dues and journals." I think that had to do with how the reserve funds were kept for future journals that went over a budget year. "Changes in editorial positions." I assume that means changes in editors—editors-in-chief, anyway. "Delegation of authority lower in the organization--"

BRANDIN: Oh, By-law 7.

NORBERG: Okay. "Delegation of authority lower in the organization was appropriate." So here, this seems to be the meeting in which this question of decentralizing authority came up.

BRANDIN: Well, yes, and the by-laws probably improved that.

NORBERG: Mm-hmm [yes]. And then, "Copyright issues." The copyright issue, as I remember it now, had to do with what would the author get from any articles of his that were approved for publication by another group. ACM, like many other organizations, was charging--

BRANDIN: Oh, something like a royalty payment?

NORBERG: Yes. And ACM was saying, "Yes, you, Mr. So and So, can go out and you can publish this at will. We'll take your \$75." But nothing was being given to the authors at that time. And that changed.

BRANDIN: Well, that could be. There was the classic "Herb Bright writes stuff about not wanting to pay competitive rates on honorarium because we should be doing this because we're volunteers."

NORBERG: But was that more than him just interested in that topic?

BRANDIN: I couldn't get them to bump up the Turing Award because they felt that it was déclassé to associate with a commercial organization. So, it was all part of the same. But I don't know if I thought of that meeting as a watershed meeting. "Watershed" is a strong word.

NORBERG: I took it right out of the--

BRANDIN: Yes, well, there were a lot of councilors who said that, I guess. [Laughter]

NORBERG: Frankly, I think it was Dan McCracken who made the comment. Since he was president at that time, it makes it look more important to him, I guess.

BRANDIN: Yes. Well, it makes for a good president's speech.

NORBERG: I didn't have any other Council members before me when I was doing this research a couple months ago, so I'm sort of probing about these other groups because they weren't in after Dan left from the Council, after his post-presidential member on the Council. The period was over in 1982, and he just stopped. I have to ask him why did he just stop. Was he not a member of Council anymore? I think he wasn't.

BRANDIN: He wasn't.

NORBERG: Was he angry about something? Did he not like what Peter Denning was doing to what he would see as his program? He wrote an interesting letter at the end of his period in which he was talking about the gains and the losses, and basically making the statement that, "Here, Peter. You should follow some of this issues." Whether Peter did or not, I don't know. I have to ask him. But there was sort of...

BRANDIN: I don't know when it started, but every president gave a memo to the next president. A personal and confidential memo, and stapled to it all the previous personal and confidential memos that had come along, that had been passed downstream. I don't know how many there were, and I don't know how long it kept up, but I gave one to Adele, and I'm pretty sure Adele passed one along. And I got one. Somewhere out there, there's a "this is what" thing.

NORBERG: What's interesting is that did not appear. That sort of document did not appear in Dan's papers, so he must have deliberately taken them out.

BRANDIN: Could be.

NORBERG: Okay. Were there multiple opinions about the purpose of ACM in the late '70s? You told me about some of these, but let's see if we can characterize it.

BRANDIN: I think that in the Council debates, when the membership issue would come up, there was always the same kind of, I don't know, three or four prongs to this. The first would be, the field is growing. Everybody would agree. The second would be that our membership is not

growing as fast as the field is growing. Everyone would agree. Then there would be, we should go after the rest of the people in the computing business. And at that point, you would not get agreement. The next one would be, we need money to go after all these others. We need to be able to advertise and promote and go to shows and maybe change some of our publications, at which point, the bleeding would begin, and it would never go anywhere. So, really, if your budget was \$20,000, maybe you get \$22,000. Enough to make cups. [Norberg chuckles] I'm serious. And there wasn't enough mass on Council to care. So, there was D'Auria, who was always championing membership from the day he got there; and there was Herb Grosch, who championed membership. There was me. For a while, Jon Meads was on Council. He would have been championing membership. He was the first chairman of SIGGRAPH, by the way. And he's a very good friend. Who else would have been really interested in membership? Portia, probably. Not a lot more. They were all professors, what did they care?

NORBERG: Well, it sounded to me, when I read through some of this December of 1978, some of the minutes, it made me think back to member organizations that I was a member of at that time. Between 1975 (the bicentennial in this country) and 1978, most of the organizations that we know today—American Chemical Society, American Physical Society, and so on—all have essentially the same administrative structure. And there may be some variation because of what each group is trying to do. But there's never any doubt about how they're operating. Number of journals they're putting out, whether they're reviewed or not, and so on. In ACM, though, there seems to me to be a difference. The difference is that the members of Council cannot agree on what the mission of the organization was. Is that a fair statement?

BRANDIN: Well, I would say it's a fair statement that they could not agree on what the mission was, but they would never admit that they didn't agree. In other words, it was not something they would confront.

NORBERG: And why weren't there people who would force them to confront it? Why didn't you force them to confront it while a member of the Council and later on as an officer?

BRANDIN: Because I was more interested in peace. I was interested in financial security because when I came on board, I had a mess. The only money that was around was the SIG's money. I wanted to get ACM to have its own money. I looked at a dues increase. As I said, I increased the pure discretionary reserves by a million and a half. And in the process, I couldn't deal with fights.

NORBERG: Where did that money come from that went into reserve?

BRANDIN: Well, we spent less, and we increased the dues, and we sucked more money out of AFIPS.

NORBERG: As I recall, the budget was something...I'm trying to remember...

BRANDIN: Seven million, maybe?

NORBERG: Yes. Half of that was for publications, and the other half was for the rest of the activities. That split seems to be true of a number of societies. ACM, History of Science

Society, if I can pick a small one. But you need to have growth in order to build a million and a half in reserves. You can't just go out two years.

BRANDIN: I think we were growing. AFIPS was giving-- Don't forget we got extra money out of AFIPS. I got a distribution out of AFIPS.

NORBERG: \$96,000, as I remember it.

BRANDIN: No, it wasn't. The \$1 million distribution, I think we got 200 grand out of that, or maybe more. It was on top of the regular distribution. I don't know. And we spent less, I'm sure. Somehow, we ended up with a million and a half bucks more of discretionary money at the end than we had at the beginning when I turned the money over to Adele, or when I turned the Association over to Adele. I gave more money to D'Auria to give to the membership staff. I was even giving money to the chapters out of my discretionary-- The president had a little, crummy discretionary fund, and I was giving money to them so they could travel and do more. The amount of money required to do this group we're talking about, you didn't need Madison Avenue to come in and fix this problem. I mean, you've got to change these.

NORBERG: Did it change after the December '78 meeting? Was the perceived mission different in the 1980s?

BRANDIN: Well, there was more autonomy. I mean, the SIGs were completely independent, essentially. They generated a boatload of money. The Publications Board is still run by the same kind of people that it was run by 30 years ago.

NORBERG: Did you propose any major initiatives during your presidential...?

BRANDIN: Only the accreditation.

NORBERG: Why? Why was that an issue? Maybe that's the wrong word.

BRANDIN: Well, because the schools were all turning out computer science degrees, and they were all different. They weren't taking the same kinds of courses, and the courses that they were taking weren't certified. We set up a system where they actually went and sent teams to schools to certify that they were delivering an accredited curriculum. And it was a big deal. That took a lot of Council time, the debates on that.

NORBERG: Were the debates on what the structure of the curriculum should be?

BRANDIN: Yes, they were.

NORBERG: Or were they on accreditation?

BRANDIN: Both, and they were also on the relationship with the IEEE because it was a joint activity. So, there was plenty of us and them, and we want this and they want that, and so forth. And then we were still debating. There was a lot of time being eaten up by AFIPS because I

would sit there and I would say, “Here’s what happened at the AFIPS Board meeting,” and I’d have to explain the inanity of it.

NORBERG: But you couldn’t bypass it, could you?

BRANDIN: No.

NORBERG: When did the money stop falling off? ’86?

BRANDIN: During Adele’s term. I think ’85. [Break]

An initiative that I started was I wanted to broadly increase our relationships with other professional societies. Based on the letter of Garcia, you can see I was trying to find a way to merge with the IEEE Computer Society, and I had Walter Carlson on my side for that. I established a relationship with the Royal Hong Kong Computing Society, with the Information Processing Society of Japan. We revitalized our relationship with the British Computing Society. I think there were more. We went to China during my administration, and this was 1982, when things in China were still pretty tough. Came back and wrote reports about computing in China. By the way, that ate up a lot of time, that trip. So I would say I was concerned about membership. I really wanted an American computing society to replace AFIPS, but I just didn’t have the... It was impossible to make it happen.

NORBERG: But there was a joint committee between the two sides to discuss the issues?

BRANDIN: To discuss the issues, and we never went anywhere. Never went anywhere.

NORBERG: Peter was on the side for ACM.

BRANDIN: AFIPS was a fat cow that was bleeding to death and didn’t know it at the time.

NORBERG: Run by the same people, basically.

BRANDIN: Run by the same kinds of people. Again, they were feeding at the trough, and they didn’t know-- it was like the dinosaurs; they didn’t know they were going to run out of food.

NORBERG: To what extent were the various boards of ACM—Publications Board, SIG Board, and so on—to what extent were these independently functioning organizations within ACM?

BRANDIN: They were pretty independent and they were completely dependent on their volunteers. So if you had an active chairman of the External Activities Board, you had an active External Activities Board. If you had an active chairman of the Awards Committee, you had an active Committee meeting regularly. If you had a SIG chair who was in Germany for the next two years but had gotten elected chairman because he had the right name, you didn’t have a very active SIG. It was all very volunteer-driven. And the president or the Executive Committee, it wasn’t often-- in fact, I can’t think of a time we fired a volunteer.

NORBERG: Usually they get pushed around.

BRANDIN: Yes, but if you get somebody who was appointed for a year-- I don't ever remember writing an un-appointment letter. I might have, but I don't remember doing it.

NORBERG: But you could just let their term run out and not reappointment them.

BRANDIN: Yes, that's what you would do.

NORBERG: I guess you have to do that with volunteer organizations, or you might lose the other volunteers in the process.

BRANDIN: Yes. That's why I said that. We had a large population of volunteers that you wish were more competent or more aggressive or more active, and we had a small population of very good volunteers who really cared.

NORBERG: Now, in mid-1977, the Council began to focus its attention on faltering special interest groups.

BRANDIN: Could be. They probably were asking me to give reports on the performance of SIGs, and I think that's what was driving a new By-law 7, too.

NORBERG: Okay. Chairman of the External Activities Board. What were your objectives with that position?

BRANDIN: Oh, well, I kept on asking, "What should my objectives be?" I never really got an adequate answer for that. It was a meaningless job. You got travel. That's what you did. You got travel to go to meetings to have meetings about nothing. [Laughter]

NORBERG: I don't know whether to say that's funny or it's not because it's--

BRANDIN: But it's typical, right? It's typical of professional societies. They create something for a reason, and then it becomes institutionalized. The reason goes away, and it's still there. And then you've got volunteers who are feeding in the trough of that activity. ICCP, I think, was one of those. It got set up because they wanted to have a relationship with these guys who wanted to certify data processing people, which always was an interesting thing that maybe we should certify them, but why are we letting another organization do it? There were all those issues, but we got some people into the ICCP, and lived there forever. That was their shtick. You know, they ate a lot of shrimp at the cocktail parties. [Laughter]

NORBERG: During your tenure as president, was the basic structure of the ACM what we've been describing here?

BRANDIN: Yes.

NORBERG: Okay, so it hadn't really changed since you've been on the Council back in the early '70s.

BRANDIN: No. I think all the boards were staffed. In other words, they were chairmen of all the boards, I think. I'll tell you one thing: You didn't mess with the Publications Board. The Publications Board was like a little self-replicating-- it was like a Turing machine. A self-replicating, breathing machine that you didn't mess with because if you mess with it, everybody was going to come back at you through Council. So it was the same people controlling the policy and the decisions. Maybe not the budget, per se, because that had to get out. Should we do a journal in pointillism? Should we pour more money into reviews? Who should be the chairman of the Publications Board? And their activities budget, you know, the travel budget and the meeting budgets and all that, that was rigidly controlled by the chairman of the Publications Board. They were, as I say, self-ingrown. It was very ingrown. Aaron Finerman was the treasurer of ACM for 25 years or so, and as far as I was concerned, the Publications Board, up until recently, was always the same people. The diffusion was very slow.

NORBERG: Diffusion of what?

BRANDIN: One molecule would leave the gas, and a new molecule would enter slowly.

NORBERG: But overwhelmed by all the others.

BRANDIN: Oh, yes, by the molecules. They're all black balls, but I love black balls. And there would be a new white ball. Right.

NORBERG: Well, I raised these questions about the organization because in one of your presidential letters, the one in November of 1982, in fact, you wrote about the state of ACM. Many, if not all, of your comments were positive.

BRANDIN: Really?

NORBERG: Yes.

BRANDIN: Well, I probably was optimistic.

NORBERG: And I'm trying to find out, were there negative aspects that you did not include?

BRANDIN: I didn't say that we should have another million members, did I? Let's see. "Future directions: We need to continue to broaden our appeal to practitioners. Increase the growth rate. Collaborate." Seems to me that these were all things I was working on. "Financial." Don't want to alienate the energetic volunteers. Less politics." I think this is completely consistent with what I've been saying.

NORBERG: Okay. And this is incoming, right?

BRANDIN: Yes. And you'll notice here I talk about my pro bono study from Peat, Marwick, and Mitchell where we talked about cascading overheads. [Laughs] That was Gene Altschuler who did that work. You know, I don't know what the membership of ACM is today. I know that Gene Spafford, when he gives testimony, says something like, "We have 80,000 members," or something. There used to be an annual report published. The annual report used to say,

“These are the members, and this was last year, and this is this year.” We don’t get any membership data anymore.

NORBERG: I see. Because I’ve noted that in ’82, it was still going on. And then Dan’s papers ended, so there was nothing after that. But there were a number of reports.

BRANDIN: It stopped. The annual reports, where you even got financial comparisons rather than just the year, all that stopped when John White came in.

NORBERG: Nobody questioned it, I assumed.

BRANDIN: Oh, I didn’t question it. I was *persona non grata*, as far as I was concerned. I wasn’t about to question it.

NORBERG: Why were you *persona non grata*?

BRANDIN: Because I tried to hire D’Auria.

NORBERG: [Laughs] On one issue, you get to be blackballed, as it were?

BRANDIN: Well, they treated me with rather-- I thought it was pretty poorly. I considered it a professional insult, what they did.

NORBERG: Do you want to say what they did?

BRANDIN: I told you yesterday. It was the third time I was chairing the Search committee, and they un-constituted the search committee, reconstituted the same Search committee, and gave it a different chairman. I thought that was an insult.

NORBERG: I see. So that happens a lot.

BRANDIN: Well, it never happened to me before. Not only that, I had the search committee agree with me, the executive committee agree with me, the executive director agree with me, an offer letter written, an offer letter accepted, at which point the Council double-crossed me. So, I took that as personal, what, umbrage? Is that the correct word?

NORBERG: Well, yes. And it should have been, on your part, taking personal umbrage because, who has the authority to write such a letter? Usually somebody near the top of the organization.

BRANDIN: Like the executive director.

NORBERG: Yes.

BRANDIN: Then it’s approved by the Executive Committee.

Session 2 – Tape 2, Side A

BRANDIN: I'm a senior retired Fellow of the association who still pays his dues, unfortunately, and can write nasty letters every now and then.

NORBERG: I've had my experiences with professional societies, too. I was treasurer of the History of Science Society in the second half of the 1970s. I was always trying to hold on people's expenditures, because everybody wanted more money out of the Council; it was all the same. And I would say, "Okay, you want some money to do something for the women. Where's it going to come from?" We can't take it out of publications, because we promised to send people journals. And so then there would be a big battle within the Council as to whether or not you could give \$50 to the women's committee. And that sort of thing went on all through my years. And by the time I was done, I was *persona non grata*.

Now, one of the important things which I think it was you proposed and managed to get through, was a foundation. Now, was that foundation holding the reserves you were talking about, or was that completely independent?

BRANDIN: I don't remember. I don't remember a foundation. It may have been to hold the reserves. It may have actually been a bookkeeping scheme, just to get the reserves set up in such a way that they could be categorized on the balance sheet. I don't remember.

NORBERG: But there was something about the Turing Award. So does this relate back to what you were telling me before?

BRANDIN: Oh yes, right. I think what we did was, we institutionalized some of the money in a foundation for the future payments of the Turing Award. So it was basically taking money out of one kitty, and slapping it in a foundation so that its income would pay the Turing Award, rather than having to debate the Turing Award money and manage it on a year-to-year basis. That's all it was.

NORBERG: Did it not handle any other issues?

BRANDIN: I don't believe so. Maybe over time, they added more awards to it, but not in my time.

NORBERG: I think it's pretty active right now.

BRANDIN: It should be. But you know, that was a terrible thing with ACM. We were giving the Turing Award. It was \$25,000. It was the equivalent of a Nobel Prize, as far as I was concerned, and it was peanuts. The other awards, like the Grace Murray Hopper Award, or like a \$2,000 award or something, the awards should have been 50 times greater than they were, and they would have been more important. But you couldn't get traction. And then, in other areas, like with the membership issues, like with the D'Auria, we had the student programming contest. This was a big deal! We would never get any press. Nobody wanted to put a marketing person and go-- or nobody was doing that from headquarters, and none of the volunteers would do it. We had the computer chess tournaments. There would always be press, but they would never say ACM. And one of the things that pissed me off at SIGGRAPH was SIGGRAPH called itself

SIGGRAPH. They didn't call themselves ACM SIGGRAPH. So ACM never got any press. It was really frustrating. And to this day, they still get a pittance of what they should get. At least the last president—the one before Stu, the one from Berkeley—he at least talked to the press and made it a point to have a lot of interaction with the press.

NORBERG: But yet there were always news documents being put out for the outstanding award.

BRANDIN: Yes, but who cared who won a distinguished service award? Hell, I won a distinguished service award, and I got a bowl. But if I won the computer chess tournament, you know, I would think a lot of people would care about that.

NORBERG: Did the Japanese upstage ACM when they established that prize, the \$300,000 thing?

BRANDIN: Oh, the Kyoto medal or something? I don't think so. I think the Turing Award is still the most prestigious award in computing you can get. There's nothing like it, even in the Nobels. Although Herb Simon got one for economics. And he was a computer...

NORBERG: He got the Nobel for his work on organizations, that is how you analyze an organization, and whether it's viable in this direction or that direction.

BRANDIN: Right, and then there was the other guy, the psycho guy who got the award for game theory. They made a movie about him.

NORBERG: Oh yes, the guy at Princeton.

BRANDIN: Very interesting game theory stuff.

NORBERG: Do you think the character of AFIPS changed after 1980, or was it still the same lackadaisical organization you've been describing?

BRANDIN: Well, AFIPS, I was on the board of AFIPS at least through '86. You're asking about AFIPS, right?

NORBERG: Well, no, I'm asking about ACM.

BRANDIN: Well, you said AFIPS.

NORBERG: I'm sorry; I didn't mean that.

BRANDIN: So did ACM change after I left? Yes, ACM changed in the sense that it shrunk the Council; it turned over more management control to the staff. Joe DeBlasi came in in '89 or thereabouts, and was a very strong manager. So the weak presidents, generally, in terms of management skills delegated more and more to headquarters, and Joe was a good manager and did a better job. During that period, they got the Council to shrink down. I think it's 12 now, or it's cut in half from whatever it was. They gave the SIG board more representation on the

Council. And then, when they hired John White, they basically said, “All right, you’re the Chief Executive Officer.” I assume it means more than just the word, so the president now is a figurehead, and has no real authority, other than, I guess, they get to approve the budget.

NORBERG: Is that typical of IEEE Computer Society as well?

BRANDIN: I don't know how they are. But the other nature, they're still academically oriented; they're still publishing refereed journals, and they're still not pandering or catering to the practitioners, and most practitioners have never heard of them.

NORBERG: Where did the practitioners go? Or do they just not belong to a society?

BRANDIN: Well, they belong to the Association of Data Processing Engineers, or the MIS guys, or the ICCP types, CDP types. But nobody reads history anymore. In 1960, we solved problems with printers and operating systems, and then they all came along with OS 360, and it took 12 years to solve the same problems again. Then they came along with the PC, and it took 12 years to solve the problems again. And then they came along with Windows, and it took 12 years to solve the problems—the same problems, the same user interface stupid problems that we have gone through four generations of the same problem, because no one reads history.

NORBERG: You paint a very bleak picture of the profession.

BRANDIN: Well, in our field, I don't think they teach history.

NORBERG: No, they don't.

BRANDIN: In Purdue, in the computer science program?

NORBERG: I did at Minnesota, and I managed to get mostly graduate students who were interested in how this thing all fit together. They had seen a piece here, a piece here, and a piece somewhere else.

BRANDIN: I majored in math, and I had learn German, and I was supposed to read original papers in German. Of course, I didn't get a PhD. If I had gone that far, I would have had to, and you had to know who the big names were, and what they had done.

NORBERG: Well, I hate to say this, but a great deal of the literature on the history of the field out there is not really very good to begin with. It's very derivative. One guy writes about Eckert and Mauchly and somebody else writes about Eckert and Mauchly using his book and so on, and down the line it goes.

BRANDIN: I met Kay Mauchly, by the way, and I liked her. I was doing some classified research on Soviet computing at SRI, and someone said, “You better read this paper.” It's a secret paper. So I get the paper. It's secret; it's got a reference. So I say, “Okay.” I go to the reference. It's got a reference. And I'm back at the original paper. Completely, completely circular. Tautological. [Laughter]

NORBERG: I've seen that over and over again.

BRANDIN: But the guy who really knows this stuff is in ACM—Sy Goodman in Arizona.

NORBERG: Sy gave us his papers.

BRANDIN: He's a good guy.

NORBERG: Okay. We've talked a lot about organization. Now I would like to shift back to the personalities again. Who were the officers that you worked most closely with?

BRANDIN: Denning, Goldberg, Sammet, hmm... Let's go through time here. It was Sammet '74 to '78, McCracken and Denning, and Goldberg during my term, and Goldberg during her term. And the ones I worked the least with would be like [Paul] Abrahams. I didn't work a lot with Mike [Harrison], even though I was on the Council at the time. There would be other Council members, but not officers. I worked a lot with Aaron [Finerman], because there was a lot of financial stuff that was going on. I had problems with the guy that replaced Aaron, Jack Esbin.

NORBERG: I don't know him.

BRANDIN: I don't know if he's still a treasurer or not. I didn't work well with him. And the executive directors.

NORBERG: And were all these contacts through ACM Council? Or had you known some of these people before, for example?

BRANDIN: I met Jean at ACM; I met Peter at ACM; I met Adele at ACM; I met Abrahams at ACM. I knew about McCracken. I met D'Auria through ACM. I knew Mittman, but he wasn't in ACM; he was the chess guy. I had started going to ACM meetings in '63, but most of the people that I knew in ACM were not officers before I became active. Oh, I remember this one. This was the corn seed or whatever.

NORBERG: Well, this follows on the report that Gary Feldman chaired a group for NSF about the state of the field, and the contention in this article is that these three men at the top of the page don't necessarily agree with the Feldman report about what NSF should be doing for the field.

BRANDIN: Well, I don't know what NSF should be doing for the field, but there was an agreement that there was a crisis in the number of people coming into computer science, and that we were eating our, that was Peter's expression, seed corn, or whatever it was. Well, we refer to the president's ADP reorganization study that I worked on. What was it? We disagreed with what NSF said? ["An ACM Executive Committee Position on the Crisis in Experimental Computer Science," Daniel D. McCracken, Peter J. Denning, David H. Brandin, *Communications of the ACM*, Volume 22, September 1979, pp. 503-04.]

NORBERG: No, what Feldman's report said. Feldman and Galler and several more.

BRANDIN: I managed to take the lead in reversing the crisis. We endorsed these concepts. What did Feldman say? Oh, Feldman wanted only PhDs. That was the problem. Well, you had McCracken and me. We didn't have PhDs, so we didn't buy into that. And if you recall-- was this '80? '79? I guess this was before the Commerce study.

NORBERG: Yes.

BRANDIN: But it is after the federal ADP reorganization study, and I had a real eye-opening experience in that study, because I was meeting-- First of all, in the US Air Force, there were something like 400 generals, and there were none that had an IT specialty. I think there were six that had communications specialty. There were a lot of full colonels that had ADP specialties, but they had nowhere to go because there were no slots. So they were retiring from the Air Force. the Air Force was losing these really sharp guys. You know, when you see a colonel, you know, if this colonel is going to be a general or not, because they carry themselves a certain way. And you saw these guys that would clearly be generals that were leaving. So there were no opportunities. The entire government was filled with...the Air Force was filled with pilots, and the Army was filled with tank jockeys, but the computer guys had nowhere to go, so they were leaving.

NORBERG: But the Air Force had this laboratory out at the Rome Development Center.

BRANDIN: They even had a computer science program at the academy.

NORBERG: How late?

BRANDIN: '78.

NORBERG: '78. Okay. And yet that didn't seem to be a line that most of the officers...

BRANDIN: Well, it had worked its way up into the general staff. By that time, I think in our report, we already said-- maybe it was in the book. You cannot fly an airplane, shoot a tank, or drop a bomb without a computer. You know, Jack Brooks was still messing with everything.

NORBERG: Well, that's too bad.

All right, I want to go back to Herb Grosch again. During Herb Grosch's presidential years, '76 to '78, he seems to be the source of significant tension within the Council.

BRANDIN: Well, he may have been the source of significant tension between him and Sammet and Galler and Ralston, but... This is '76?

NORBERG: Yes.

BRANDIN: Yes, there was significant tension between them, but it didn't affect me; I mean, I was just a happy, carefree... I had my SIGs; I had my buddies. We would sit in one corner; there were about four of us that would sit in one corner of the Council meeting and smoke cigars: Stu Lynn and me and Tom D'Auria, I don't know who else would smoke a cigar. They would

have these arguments and these debates. And then I told you there was a retroactive executive session meeting in Minneapolis, and they were fighting. Jean and Herb were just the antithesis of each other.

NORBERG: Herb seemed to not want to forgive the ICCP loan that ACM had given them.

BRANDIN: That's possible, and I would have agreed with Herb. I would not want to forgive the loan. I don't know if that's agreement or disagreement.

NORBERG: That's agreement with Herb.

BRANDIN: I would not want to forgive the loan. But I don't remember the debate. That is not the source of the executive session.

NORBERG: Okay, but the ACM ultimately took them over, didn't they?

BRANDIN: Took them over? Well, jeez, I don't know. I don't remember taking them over.

NORBERG: There was also the planning for the first history of programming languages conference, which got up to the Council level because of Grosch's objections to the whole thing.

BRANDIN: Okay, on a scale of one to a billion, that one would have been a two.

NORBERG: I think I will drop that. I want to go back to the election procedures, again. 1979, 1980, when you were coming onto the external activities board and were a member of Council, there was a dispute concerning the chair of the nominating committee censoring the two ballot statements, which I told you yesterday that were to go to members. These were changed after the candidate approved it and before it got into print to send to the members. Since this occurred in the year you were elected, do you remember anything about this dispute?

BRANDIN: I remember the dispute, but I don't remember any of the details. I can tell you this. I can't tell you; I will guess. Without knowing, remembering the facts, I will assume that the two complaints came from people that were coming up out of the chapters part of the organization. Without any other information, that would be my assumption. And the changes that were made were probably irrelevant—probably irrelevant. And they were not sanctioned, and I don't know who made them, but they certainly would not have been sanctioned by me. I suspect also that the issue was related to, well, how many words are words, and who counts the number of words and other prepositions? I remember, one of my suggestions at the time was, instead of worrying about all that crap, why don't we just allocate X inches of space, and let them put whatever they want in their X inches?

NORBERG: Makes sense.

BRANDIN: But that never happened. But I vaguely remember there were complaints.

NORBERG: Okay, because Grosch raised questions again when Abrahams was elected vice president.

BRANDIN: Grosch would have been defending the chapters. So he would have been supporting chapter people.

NORBERG: Well, he took a hard line with the nominating committee at that point, and actually dropped a couple of people from it and appointed others.

BRANDIN: You mean when he was president?

NORBERG: Yes.

BRANDIN: Well, that's fine. He owns the nominating committee, so he can appoint anybody he wants. You know, there were presidents who really cared about that, and there were other ones who figured, "Well, you know, the system is telling me this is a good person," like Herb Maisel. I think Herb Maisel was one of them, or maybe he was on one of my search committees. But they want those guys on the nominating committee, yes, put them on the nominating committee. They can nominate anyone they want. I was pretty lackadaisical about that. I didn't feel like I needed to control who was going to get nominated.

NORBERG: How did you feel about your predecessors in terms of their running the Council meetings?

BRANDIN: Well, I thought I could do a better job. You know, it isn't clear why I ran for president. Let's say it was circumstantial, and an opportunistic thing that popped up one night in LA with Seymour Wolfson at the bar. This was '81. Yes, it was '81. I think it was late '81, or early '82, because the elections were in the Spring of '82. I had just finished my Stanford executive program, and I was running a computer science division, and I was a little drunk, and Seymour was saying-- You know, he was talking about who they were thinking of nominating or something. I said, "Well, why don't you nominate me?" He says, "Well, we were going to ask you, but we didn't think you were interested." So I said, "Well, I'm interested," and they nominated me. That's kind of how it happened, but the timing was-- I don't think if I had gone to Stanford, I would have-- if I had not gone to Stanford, I probably would have been less interested.

NORBERG: Why?

BRANDIN: Well, I was beginning to think career opportunity, increased influence. Being president of ACM would certainly help sell consulting, and I knew my bosses would support me, at SRI. So it was good for business; it was good for my professional career, and I knew I could run a better meeting than any single one on the Council. And I did. My meetings always started on time and always ended on time or early. Not one single meeting ever ran over. And with Peter or with Jean, the meetings could run 12 hours.

NORBERG: Well, they always have something to say.

BRANDIN: They didn't manage the meeting.

NORBERG: How was Dan at managing the meeting?

BRANDIN: McCracken? Well, he was fine, except in the executive committee, when he turned and faced the wall. But no, everybody lost control of the meetings, because there was always some unexpected big deal, which we talked about yesterday, which all of a sudden consumed time, and the president or whoever was running the meeting just couldn't seem to control the meeting. And I would simply take control of the meeting. I would say, "Hey, we're not going any further than this. It's not on the agenda," or, "I'll give it five more minutes, and if you want to get in the queue now, get in the queue now. You're not getting in the queue later on." I would abbreviate it. So I always thought of myself as a good leader and manager, and fair.

NORBERG: Well, if you give everybody the privilege of talking, that's fairness.

BRANDIN: Well, everybody gets the privilege of talking or the privilege of getting into the queue, and if they're not in the queue later on, they're not talking.

NORBERG: A couple of names that come to mind in the same period that you haven't talked about, somebody like George Dodd, for example.

BRANDIN: George Dodd was a very nice guy—very nice man. I don't remember what his role was.

NORBERG: He was secretary at one point.

BRANDIN: Secretary. He was a very nice, smart, competent, quiet, non-controversial guy. I liked him.

NORBERG: Did you work with him closely at the time?

BRANDIN: No, I don't think I worked closely with him. I may have sent him comments about minutes, but I don't remember anything.

NORBERG: The Miller you talked about—not the Miller at SRI, but Raymond Miller?

BRANDIN: I liked Ray. We got along. We got along very well. I think Ray was on one of my search committees. We took him to China; he went to China with us. So I know Ray pretty well; I spent two weeks with him freezing our butts off in China. Ray's a computational guy who is a-- jeez, I don't know, did he become a fellow? I don't know, but I liked Ray. Nothing springs to mind.

NORBERG: I have a number of issues that were confronted by ACM in the period of the '70s, '80s, and including the '90s, and some of these, you've touched on already, and I'll just drop them from consideration, but there was no mention of the journal for all members.

BRANDIN: Oh, I don't remember the JAM.

NORBERG: Yes, May 1975, the Ralston committee.

BRANDIN: I think it died, but I thought it also kind of became the equivalent of *Abacus*. I think there was some incestuous relationship between them. I don't know.

NORBERG: Well, it might have been in the early time, but in Summer of '75, an ad hoc committee on publications was established.

BRANDIN: Yes, but that was before the publications board really owned the publications.

NORBERG: I see, because Peter Denning was on this committee, and apparently wrote the report.

BRANDIN: He's like the ubiquitous Admiral Yamamoto; he's everywhere.

NORBERG: So you don't remember discussions about this at all?

BRANDIN: I don't think I would have sat in them or paid attention.

NORBERG: Okay, because their first interim report was October '75, consistent with Sammet's ad hoc committee on reorganization which she had Galler chair.

BRANDIN: I don't remember.

NORBERG: Okay, the claim was made, at that time, that any changes in the journal would not affect the other publications, and would not reduce income from publications; it would have a minimal effect, if any effect at all. And I don't see that that's the case. When I think how you would change Communications so it would become like Physics Today or IEEE Computer, it would be going a long way to dropping out most of the technical publications that appear in there.

BRANDIN: Well, I'm not sure what the Journal for All Members was supposed to be. Was it supposed to be Communications?

NORBERG: Communications was going to be altered in order to become the Journal for All Members.

BRANDIN: Ah, so they were going to call it a journal. Well, that would have been suicide, too, expanding the membership outside of the field, outside of the science field.

NORBERG: We talked about the ACM and the research community. I want you to add one thing—that is the committee on relations between the ACM and the research community that was established. Again, this was in the late '70s, and I'm not sure what it was designed for. ACM and the research community. That's the way it was expressed.

BRANDIN: I don't know. The pertinent organizations would have been like SIAM and the American Mathematical Society or the American Mathematical Association, but we had relationships with them through the CSSP, Council of Scientific Society Presidents.

NORBERG: Tell me about that. I don't think I've come across that.

BRANDIN: Well, I became automatically a member of that when I was president of ACM. Once a year in Washington, you pontificate.

NORBERG: About what?

BRANDIN: Well, government spending on research, and how the country is doing in research, and whether Congress is spending enough money on research, and the National Science Foundation go more and more into this. It was kind of high-level muckey-mucks.

NORBERG: I was going to say, a high-level group of lobbyists.

BRANDIN: Well, they weren't lobbyists, but they could write letters on their stationery.

NORBERG: And what good is that?

BRANDIN: It shows up in a congressional legislative proposal.

NORBERG: That somebody reads. Okay. This was the period, too, when McCracken suggested the name of the organization be changed.

BRANDIN: Yes, I remember that. Drop the "machinery". I was all with that. I thought it made a lot of sense. But I don't know; I guess the conservatives-- Grosch would probably have been against it.

NORBERG: Well, the membership turned out to be against it when they were surveyed. They decided not to make the change.

BRANDIN: Yes, but it may have been poor marketing.

NORBERG: Well, okay.

BRANDIN: I'm not sure the membership knew what they were voting for.

NORBERG: All right, the big issue: ACM and the education of future computer scientists. This was a continuing discussion from 1974 to 2007 in which the accreditation issue arises. What was the field composed of? If you put all these things together, which I tried to do, you could see the changes in the field as perceived by the people on this committee, endorsed by the Council. So you get curriculum '68, curriculum '72 or '74. There's one in 2000; there's one in 2003, one in 2005, and it takes a lot of tinkering to move it from one time period to another, but nevertheless, it is a concern of the Association.

BRANDIN: Well, I believe that, and they cared about the education, and that's why we did the accreditation.

NORBERG: Did you participate in any of these groups, other than the ones we just talked about here? About the curriculum, or about the future, things like that?

BRANDIN: No, I was the one who-- I worked with Kniefel to make sure that we managed the accreditation and guided it through, because this would have been maybe not as hard as getting the Turing Award bumped up, but it was definitely not an automatic; it had to be shepherded and politically smoothed through. It required talking to people on the side and stuff like that.

NORBERG: Were people asking for some things to be put in, and other things to be dropped?

BRANDIN: Yes, and there were differences of opinion between what the Computer Society wanted, and what we wanted, and it would have to be worked out between them. Kniefel was a good point man. He did a really good job.

NORBERG: Okay, so in this situation, the accreditation is being done by both societies with the same set of documents?

BRANDIN: Yes. Well, what we did was, we both adopted the same rule, and I think we set up a joint committee to do that accreditation.

NORBERG: Okay. Denning was on one of those committees.

BRANDIN: Well, as I say, he was on everything.

NORBERG: What could ACM have done to help the development of future computer scientists—production of graduate programs, for example?

BRANDIN: Well, I don't know they needed to do more to produce computer scientists. They needed to do more to produce and attract the attention of programmers and software engineers and hardware engineers and management information systems people and database people and guys who made terminals. They were certainly affecting the computer science departments.

NORBERG: They were doing the hiring.

BRANDIN: They were doing the hiring, but I mean the curriculum. ACM was certainly playing a role in the education of computer scientists, but it wasn't playing a role in the education of anybody else. And computer scientists are probably, what, 10% of the total population of people who work with computers. And computer scientists in general are the soft side. They're not the hardware side. They don't know how to design a flip flop, right?

NORBERG: Yes, that's why the electrical engineering groups got into the picture in the first place.

BRANDIN: Right, right, and the mechanical engineering guys. Like, I recently had to triple the memory in my laptop, and I had to get somebody to do it, because I didn't want to open up the box I could have opened up the box, but I wasn't going to do it.

NORBERG: I believe that.

BRANDIN: So I think they did okay in terms of educating computer scientists. Where I think they fell down is getting people to be interested in computer science. It's not like they're pouring money into elementary schools to get elementary kids to be interested in taking computing and math in high school. The whole country has failed miserably in math. But from the point of view of once someone is in the machine, they're fine; ACM does a fine job, and ACM provides the environment for when they graduate, and where they can publish their first paper and come to their first job fair—all that stuff. I think they're fine.

NORBERG: Okay, but at the same time, the association, ACM, was looking at several other fields, like physics, that were encountering severe gluts in the total population of physicists. There weren't enough jobs for them.

BRANDIN: Aerospace recession time, right?

NORBERG: Mm-hmm [yes]. That's when I left the field—not that I wouldn't have a job; I could have continued teaching. But I left the field, and here in ACM, they're looking for some sort of algorithm to prevent a glut appearing in computer science.

BRANDIN: Well, I think they were idiots, because there was no glut.

NORBERG: They weren't producing more than they could deliver a job to?

BRANDIN: Well, there may have been a short-term recession. In '69, there was an aerospace industries recession. There were guys in Stanford who hung themselves or put plastic bags over their head. In '79 or so, Lockheed shut down for a while. I think right now, there's not much of a glut of computer scientists in the world.

NORBERG: But wasn't it true that the number of PhDs was not lessening, but going to industry instead of to the academy?

BRANDIN: Yes, so there was no glut of PhDs.

NORBERG: I didn't want to make that association. So you have all of what we would call background noise that is happening in the field, and some people are very concerned about it.

BRANDIN: Then? Well, maybe they were concerned about it, but the other thing that was happening at that time was, all of a sudden, money was beginning to really be made in that business. The PC showed up in '81. Venture capital showed up in the '70s. Machines got cheap and small, and applications for making money in special-purpose markets and applications exploded. So the forces were natural; these were natural forces, so I wasn't concerned. I mean, I was concerned that we weren't going to educate enough computer scientists-- Okay, I was concerned we weren't going to educate enough computer scientists, and we certainly didn't have a glut. By 1984, I was less concerned about educating computer scientists, because when I did that Department of Commerce study, I learned that the country was way ahead of everybody else in theory—way ahead—and holding our own in advanced development—not way ahead, holding

our own—and we were way behind in production, in manufacturing, and all the forces were in the wrong direction with all the changes. So I wasn't worried about whether we were educating enough computer scientists, because we had clear superior lead in research.

NORBERG: Okay, were these people learning on the job, then, about what they would need in a manufacturing setting?

BRANDIN: Well, we weren't manufacturing. That was the problem. We had stopped manufacturing.

NORBERG: What I was thinking of is that these training programs, wherever they are, whatever school, would be ineffective in teaching applications of the kind you would find in manufacturing.

BRANDIN: This was more philosophical, I think. I remember talking at a computer science conference to a bunch of kids in a session about what am I looking for when I hire a graduate? This was when I was at SRI. I told them, "I know you know how to solve a differential equation." I don't need to know-- "You don't have to convince me you know how to do an integral or solve a differential equation or add or subtract. What I care about is, are you going to come to work, and then three weeks later, decide that you want to go to a demonstration in Boston, and three weeks later go to a demonstration in LA? You know, are you going to be a stable and responsible person, or are you going to be irresponsible?" To me, it's more important that you're a serious person. I know you have the basic skills, and most of them did. When they came out of school, they knew how to write a program; they could write it in PL/1 or whatever it was, and they could solve a problem. But they would come to you-- I had one guy come to me one day, and he says, "You know, I can't work in my office because they put a film on the glass, and the lighting is changed; I can't work in my office." There were a lot of people like that. So my answer would be, "Well, get a note from your doctor, and we'll remove the film."

The problem was that the philosophy of the American research system was, if you didn't study under the right professor, your ideas are wrong, and therefore we're not going to use your ideas. Or, yes, you want to do basic research in this area; do basic research. A guy publishes a paper, and that's the end of it. The Japanese, they won't just publish a paper; they'll build it, demonstrate that it works—not just put it on paper. And transferring technology, you don't just transfer technology with paper; you transfer technology with people. The American system was, "Well, we do all this great research, and then we throw it out on the floor, and if somebody picks it up, great." The Japanese, they wouldn't do anything about working in that room with the basic research, but they come running down and pick up everything they find in the hall.

NORBERG: Yes, they did that for quite a while.

BRANDIN: Yes, and it was extremely successful, and gave them a real edge in manufacturing.

NORBERG: Why do you think it didn't happen here in the United States?

BRANDIN: It's just societal forces endemic to the system. Same way in Japan. You know, nobody wanted to do anything that stood out. The hammer that hits the nail that stands out. So it

was systemic. DARPA was very worried about that fifth generation project, because one of the goals was they were going to train research managers to do research on computer science.

NORBERG: The Japanese were?

BRANDIN: The Japanese were. And of course, they trained some, but it didn't go anywhere. And the machines themselves didn't go anywhere.

NORBERG: Because I remember a lot of smug statements, too, about, "Yes, we knew they couldn't do it," ??? in the software.

BRANDIN: Well, you know, they weren't very good at research, but they were really good at producing production code. Really good. They wrote third-generation code for 360 compatible machines. They could turn it out in droves. But to do research on new compiler technology or whatever, they were very weak there.

Session 2, Tape 2, Side B

BRANDIN: I wanted ACM to go along with it, because I thought it was good for the country. I'm not sure, you know, how the international members would feel about it, but I thought it was a good thing for the country. I believed what the things said in the report, that the cryptography issue is so important because of the timeliness of the information that the scientific community should cooperate with the government if we could construct a system which is rational, and we constructed a system which I thought was rational. We came up with a pre-review publication option, and NSA agreed that they would respond within 30 days if it had a problem, and the author could change the paper or not, and the editors could print the paper or not, regardless of what NSA had said. I worked on that. I remember the meetings in the PCSG. Inman had assembled that committee. The American Council on Education ran it. Mike Heyman, who was chancellor at Berkeley was cochairman with some guy from the American Association of University Professors or something, and Heyman, me, and Dan Schwarz, who was general counsel of NSA ended up becoming the writers of the report. So I was one of the writers of the report. And when we presented it at ACM, there was a lot of resistance, because the scientific community doesn't like the idea of being told to pre-review their papers. And NSA was saying that these are not classified papers, so they can't control them; these are not government financed research papers, so they can't control them; these are unclassified, private. So there was a lot of scientific community hostility and resistance to the proposal, but the ACM council agreed to go along with it, and published in the Communications the procedure. NSA set up a review committee. You probably don't know this, but about two or three years later, I went to a meeting at Carnegie, I think, where the speaker was Link Farhrer, who was at that time the general in command of NSA, and he said he reported on the progress of that, and said they had received about 200 papers. They had had problems which they commented on in seven of the papers, and all seven of the authors had changed their papers without complaint. So as far as the NSA was concerned, the system had worked perfectly.

NORBERG: And yet, there was a good deal of discontent.

BRANDIN: Well, they dropped it eventually. While I was there, it worked.

NORBERG: What is the relation between this work of the Public Cryptology Study Group and what I will call the attempt to have the IBM DES system become the certified cryptographic device that was going to be used in commercial activities?

BRANDIN: That's a different issue. That's the question of whether you're going to plant a Trojan horse or a system that's known to be crackable, or that's already been cracked, in commercial systems. We never debated that. It had nothing to do with the study group. The study group strictly addressed the question of pre-publication review.

NORBERG: How about the general view among the ACM people, though, about this DES thing?

BRANDIN: I don't know what the general view is. I know George DeVita was paranoid about the government role, and he was the minority position in the public crypto paper, and we gave him a lot of space to take his position. I know David Kahn said that if we voted in favor of this idea, of a public crypto study group meeting, that we would be the greatest traitors to the Constitution since Abraham Lincoln had been president. Honest to God, he said it. And in fact, we're in the book—you know, in his book, *Codebreakers*. Yes, we're in the book. There's a footnote. My name's in the book. But I don't know, the DES thing, and Marty Hellman from Stanford, and the Rivest, Adleman, Shamir stuff, that's all a different issue.

NORBERG: Okay, let me deviate for a moment and say, did you deal with that issue at SRI?

BRANDIN: No, not really. We dealt with the provability of secure operating systems, and that was theoretical work; that was before the RSA algorithms. And we dealt with computer security from the commercial consulting point of view, and even things like is there a fire extinguisher in the computer room kind of stuff. But I don't remember anybody in my division working specifically with Hellman on that kind of stuff.

NORBERG: Peter Lycos was chair of the Computers in Society Group.

BRANDIN: Wait a second. When you're talking to Peter Denning, Dorothy Denning can tell you anything you want to know about that kind of stuff. She is first class, and she worked for me at SRI in the Computer Science Department of my division, and she may have been doing stuff that I didn't know about; there may have been a classified contract I don't know. But you can ask Peter.

NORBERG: I'll try to get that into the record, so to speak. I mentioned Lycos, and this special interest group on computers in society. There was also McCracken, who chaired the committee on computers in public policy, and there was an AFIPS group that was headed by Willis Ware in 1975 or so, and Sammet decided to push Willis Ware along. He was being too slow, as far as she was concerned, and so she wrote him a letter telling him how to run his committee. And she sent this letter to McCracken and Denning, suggesting that they get on the back of Willis Ware and push this agenda along. Well, Peter wrote a letter at the time, in September of '74, I think, which he thought that the direction that McCracken was taking was not fully satisfying, and that

his position was somewhat complementary to McCracken's, and so he suggested that Dan and he draft up a report on ACM's view on public cryptography. Now, Sammet, to give her fair due, here, was interested in privacy issues, and that's what she wanted the committees to do, but Peter was interested in something else.

BRANDIN: It doesn't ring a bell, but I will tell you that what rings a bell with McCracken is that McCracken supported the proposal to make copyright law applicable to software. I think it was the 1978 revision to the Copyright Act or something. I don't agree with that. I think it is wrong. It's institutionalized and established now; there's so much case law, it will never be changed, but I thought it was a mistake from the get-go.

NORBERG: Why?

BRANDIN: Because copyright protects expression and not idea. And code, you cannot separate the idea from the expression. And so I always felt you needed a separate law to protect code, and the result was that they then got into protecting look and feel, and you have the insanity of Windows stealing the Macintosh look and feel and winning. Actually, the better way to describe it is Apple losing, because that's the only lawsuit that that general counsel of Apple ever lost, and he never should have lost it. It was criminal that they lost that lawsuit—criminal. But you know, the whole thing was set in place when they set up that stupid copyright law.

NORBERG: Well, it's certainly difficult for us in the Babbage Institute, because of who owns the ideas, and whether we can even release something. The State of Minnesota passed a data practices act, and that got us off the hook, because now there are certain procedures that you must follow in order to release personal information, and there's a time period beyond which you can do anything. So we think we're safe, but we may not be.

BRANDIN: Just don't cross the Mississippi River.

NORBERG: My oldest girl called last night, she wanted to know where I was, and if I was involved. No, I'm not involved; I'm in California. And a side question: what was the Turchin Resolution? Do you remember?

BRANDIN: Yes, that must have been the letter. Turchin was the guy in prison in Russia, and Tony [Ralston] wanted a letter signed. I thought it was Scharansky yesterday. It was to go to the director of the prison. I don't know, but he wrote the letter and wanted my signature on it, and I said, "Well, if the Council says, 'Sign it,' I'll sign it." And that was my objection, that the *New York Times* had paid his dues. So one of our members was in prison in the Soviet Union for-- I don't know what the crimes were, but Tony was up in arms about it, and I said, "Well, I'll sign it if they..." So I signed it, and that was the end of it.

NORBERG: Well, it got him out, I think.

BRANDIN: I think he did get out, but not right away.

NORBERG: The discussion about privacy could also be extended to security. Did ACM or ACM councilors, or any committees or special interest groups take up the question of security?

BRANDIN: I don't think there was a SIG on security.

NORBERG: I didn't come across one.

BRANDIN: What about computers in society? They would have [inaudible].

NORBERG: Well, it's not clear that they took up the issue.

BRANDIN: There was a SIGSAC—Security, Auditing, and Control—but I don't think they were into security. In fact, I don't remember meeting anybody from that SIG. I wonder if it really existed.

NORBERG: How can you say that when you were chairman of that board at one point?

BRANDIN: Are you kidding? Talk about a virtual world, my God. Have you seen that movie “The 13th Floor”? It's a science fiction movie about Los Angeles in the '20s, and the people discover that they're a virtual world running on a computer, and then the people running the computer discover that they're a virtual world.

NORBERG: Sounds like a Star Trek episode.

BRANDIN: It's emulated, emulated, emulated, and it's a very good film.

NORBERG: You gave a lot of attention to “War Games” when it came out, too.

BRANDIN: I was very unhappy with that movie, because I knew that it was impossible. What they postulated was impossible, because those kinds of computers were not connected to dial-up. I know those computers; I saw those computers. I was in the command post at SAC. I was in Cheyenne Mountain. I know those computers were not connected to any telephone lines—not that those kids could get at, anyway.

NORBERG: Well, there was some umbrage, to bring that word into the conversation, again, among some members, at least, at ACM, who wrote into ACM Forum, saying, “This is the second time Brandin's brought up War Games,” and so on, and, “We don't think that we should be interested in that problem,” and so on. But anyway, the Ralston business with Turchin revolves around the question of whether ACM should have a committee on scientific freedom and human rights. This was a big period in which all sorts of such committees are being established in other groups.

BRANDIN: You know, I was less inclined to do those kinds of things.

NORBERG: Well, you said that yesterday.

BRANDIN: I was pushed into it.

NORBERG: You said that yesterday, when you talked about seeing that ACM should not be in the business of looking at social issues.

BRANDIN: Social issues that don't involve technology.

NORBERG: All right. We can probably skip over the history of programming languages.

BRANDIN: I don't know that much about it anyway. It was a Tower of Babel.

NORBERG: I want to solidify a couple of comments that you have made and put them into perhaps a different context. How effective do you think ACM was in the '70s and '80s in disseminating information about computers and computing?

BRANDIN: I think it was very effective at disseminating technical research information about computers and computing.

NORBERG: Okay, but that would be to the specialist community. How about the general community?

BRANDIN: Well, the general community wouldn't know what to do with it. So I think the audience that they were directing their attention to, they were disseminating good research-- I don't know if the research was good. They were disseminating the research that was happening to their field, and I think they did a good job. I mean, they published 15,000 pages a year of refereed journals, and I would say that in general, the refereeing was fair. In general, it was fair. And then there were plenty of conferences they were running, and there were lots of attendees at the conferences. They did a fantastic job in graphics. Hollywood would not be anywhere it is today without SIGGRAPH. There is no doubt in my mind about that. So I think they did a good job.

NORBERG: And therefore, you would say that it advanced the field and the profession?

BRANDIN: It advanced the field and the profession of computer science research.

NORBERG: In this respect, what do you think ACM's strengths and weaknesses were in this period? We're talking roughly from '75 to '85.

BRANDIN: Well, you know, you have to define the value function here. If the value function is the computing research field, it had plenty of strengths and very few weaknesses. If you're talking about the entire computing spectrum, it was a very narrow band on a spectrum, and therefore, it was very strong in that very narrow band, but it was weak everywhere else—shallow. That was bad, from my point of view.

NORBERG: I wanted to call it a weakness, and you would agree, I think.

BRANDIN: It was a failure. They failed to expand to fill the spectrum, and they had an opportunity at one point in time.

NORBERG: Which opportunity? You told me about several.

BRANDIN: You're getting into more detail. I would argue that in 1947, ACM was the only game in town, and let's say in 1960, when there were still only maybe 100,000 people in the business, all those people knew who ACM was, and at that point in time, they could have become the American Computing Society and satisfied everybody. Not everybody; you know what I mean.

NORBERG: The majority. And why do you think that didn't happen?

BRANDIN: I think that there was nobody at the helm who cared, who wanted it. They didn't want it. They wanted to stay in their little niche. The guys in those days were like the von Neumanns, the Oettingers. They were research people; that was their interest.

NORBERG: Did the changing nature of the computing field-- the introduction of applications in the middle '60s that were for general use in various kinds of industries, the changes that were taking place among the boundary or the interfaces with other groups, like the Chemical Society on the one hand, and say physics on the other hand, and also, there was more interest in the '60s and '70s, certainly, in what the computer really could do for someone who was not a computer scientist. That's a little early for PCs, of course, and so that question doesn't get answered in the large until PCs were available. But with the hobby kits that were being marketed, with the various sorts of games that were available early from Atari and companies like that, it seems to me that that would have been a way to direct interest into ACM, and it didn't happen.

BRANDIN: Nobody did it. So you may want to go back and look at who the executive directors were in those days, and say, "How come they weren't doing it on their own? How come they weren't showing initiative?"

NORBERG: Well, that's where I'm back to my distinction between staff and authority.

BRANDIN: Well, but again, to me it's the executive director issue, and it would not be below the executive director, also because you're really dealing with grunt-level support staff, and the executive director is supposed to be an imaginative thinker. You know, they could have been sticking ACM brochures in Atari game kits—a classical marketing strategy—and they didn't. I don't know why. Maybe it's because-- I know in Jean's time, when Sydney was executive director, there wasn't enough money. So maybe Sydney wanted to do that or that kind of stuff, and she told him no. I don't know.

NORBERG: Do you think that the IEEE-Computer Society did those things, and therefore...

BRANDIN: No, I don't think so. I think they just pandered to the computing engineering field. Their big debate was, should Texas require a license to be a computing engineer, because Texas requires a license to be an engineer. Those were the high-level issues that were being debated over there. I do know that we had an executive director named Gordon Smith, who was a first-class marketing guy, but it was so far back in time, that I just don't know. The staff was very small then; this was like '67. So I don't know what resources were available for marketing.

NORBERG: Is this the same Gordon Smith that worked for Memorex?

BRANDIN: Yes, and UNIVAC. Great, wonderful guy.

NORBERG: What might seem like a strange question: did you ever participate in the Rand Corporation round tables?

BRANDIN: No, I don't think so. Although I did know Willis and I've been to Rand, and I was at Rand probably on classified events, but I don't remember being on a round table.

NORBERG: JCCs?

BRANDIN: Yes, I gave papers at the JCCs, and I was the vice chairman of the NCC in '76, because Steve Miller was general chairman of the NCC '76 and worked for me at SRI.

NORBERG: How did you get to take on the job? Why didn't you do it?

BRANDIN: I took on the job of vice chairman; it was symbolic. He worked for me at SRI, and they made him general chairman of the NCC. You know, AFIPS asked him or something. He asked me to be vice chair, and I said, "Sure," and then I hid out and stayed away from there as much as possible. I heard the walkie-talkie saying one day that there were 67,000 people trying to register, and I turned off the walkie-talkie and went back to the pool. So yes, I gave papers at the JCCs. I went to most of them. I gave papers at the NCCs. I went to most of them. And I think there were times I was even a reviewer. We weren't really referring papers so much as selecting papers.

I even gave a talk at the AAAS once. It was in lieu of Bob Inman. The way it started out was, the AAAS was meeting in Toronto in '81, and they wanted Bobby Inman to come to talk on the need for secrecy in the interest of national security, and Inman said, "I'm not going to give a policy talk outside of the United States." So at that point in time, they started calling other people, and I have no idea where in the sequence I was—either two, or two to the infinite. It got to me, and I said, "Yes, I'll be very happy to take that position if the government will tell me what its position is. You arrange an audience for me with Inman, and I'll come and give the talk." So they did. They arranged a breakfast meeting with Inman, and I went to NSA headquarters, had breakfast with Inman. We talked about the findings of the thingy and all that, and then I went and gave the paper in Toronto—the need for secrecy in the interest of national security.

NORBERG: But you weren't speaking authoritatively in the way he would have?

BRANDIN: No, and I wasn't representing the government; I was giving an interpretation of the government's position.

NORBERG: ACM established a committee on education even before Sputnik. They were in this game a long time, and as I understand it, this work has tended to diversify over time. That is, the sort of program you would say computer scientists should study in 1958 is different from the one in 2000, because of the maturity of the field. I see that difference. I don't have any

problem with it. But what I'm interested in is, was this a major initiative of the ACM over the years?

BRANDIN: Yes, it got a lot of attention in Council. Education and curriculums always got a lot of attention.

NORBERG: Was that work only done by volunteers, and therefore, did they travel to anywhere else, or did they just get together at meetings that they were going to go to anyway?

BRANDIN: Well, I wasn't involved in any of the activities, so all I would hear about would be at the executive committee meetings and at the council, where they would be talking about the new curriculum being developed, and the committee met. I don't know if they were going and interviewing the chairman of the computer science department at Purdue as part of their activities or not, but there was always activity about education—always.

NORBERG: Were there any contentious issues at these discussions?

BRANDIN: Not that I know of. I don't think so.

NORBERG: Incidentally, I meant to ask you yesterday: what is a TEC group?

BRANDIN: That is a technical subcommittee of the IEEE-Computer Society, isn't it?

NORBERG: It's one that was in ACM at one point, because the chairman of the SIG board was talking about these types of meetings and, they wanted to establish some sort of authority behind them, as opposed to just letting somebody come in and say what they wanted to say.

BRANDIN: Well, they were after my time, so I don't know. I don't remember any TECs. We had SICs, and we had SIGs, and if they had technical subcommittees, I don't remember them. That sounds to me like an IEEE term.

NORBERG: What was your opinion regarding the proper role of computer science education in the 1970s and early '80s?

BRANDIN: The proper role would have been to turn out people who knew how to solve problems on computers. I didn't think we needed to still do a lot of research in software. I don't think there's been that much progress made in software engineering in the last 20 years. I really don't think there's been that much progress. You've got faster machines, and infinite memory dirt-cheap, so you can keep on throwing layers and layers and layers and layers of software on there, which make it look like they're more efficient, but my gut instinct tells me that they're wasting horrible amounts of resources because the software is so inefficient. I don't see anything that's become more productive because of research in software engineering. It's just a kicker from the combination of the hardware and the cost. That's what I told DARPA about three years ago: stop pouring money into software engineering. Spend it on problem-solving instead.

NORBERG: Interesting. What do you think would be the reaction of somebody like...

BRANDIN: Denning?

NORBERG: Well, Denning comes into this, but I didn't want to put you on the spot with Denning.

BRANDIN: Well, he would be outraged.

NORBERG: Okay. Why would he be outraged?

BRANDIN: Well, because Denning makes his living publishing stuff about the theory of programs.

NORBERG: Well, I read some of that, and it doesn't strike me as being Earth-shaking.

BRANDIN: Well, he's got his famous paper. What is it, The Working Set paper?

NORBERG: Yes, which I imagine was his dissertation.

BRANDIN: I think so. It's been referenced and cited all of the time. I've never considered myself a theoretician.

NORBERG: All right. Now, I want to turn to money. Would you describe for me what you understood about the budgeting process at ACM?

BRANDIN: Sure. Well, first of all, there was a steady-state condition when I came into ACM, and so the budget would have been adapting over time to the boundary conditions that existed when I came in. And the boundary conditions that existed when I came in were, the publications laid out a budget which showed how many subscriptions, how much revenue they were going to get, and how much money they needed to print a review, and all this other stuff. There was a very small SIG activity at the time, and so that was probably handled by somebody at headquarters because there was no SIG board then. Anyway, it was pretty casual and informal at those days, and it was 99% dominated by the publications and the assumptions on membership. And at the time, we were not accruing revenue, I think. We didn't do an accrual of revenue until Jean ran into the wall and ran out of money, because what they were doing was, they were spending money that had not been earned. In other words, you had a 12-month subscription.

Now over time, during the period of the '70s and '80s, the budget process evolved so that the board chairmen put together their budgets. They usually had a staff person from headquarters working with them to help them assemble the budget. So the SIG board had a person from headquarters who helped us assemble the complete budget. We'll talk about overhead later. So the boards would make their budgets up. The publications board would have their budget up. Mark Mandelbaum would be in there with all the cost per page and ink and all that stuff, and then all those budgets would be taken by McKegney, the chief financial officer, or whoever it was, and it would be integrated, compiled, assembled, printed, and delivered to the executive committee. The executive committee would then scratch their bellies over that one. They would call on the board members to justify their expenditures. The board member, when asked a question, would immediately turn to the staff person to get the answer to the question. I'm being facetious, but in general, that was the case. And then the executive committee would

finally say, “Okay, strike this; strike this; add this; do this; do that,” at which point, headquarters would take it, make the changes, and probably bring it back to the executive committee for them to say, “Okay.” At which point, that then went to the Council, and the Council officially was required to approve the budget. So there was a formal, reasonably rational process for setting up the budget in ACM.

Now, the technical difficulty they got into with Sammet was, they were spending money they hadn’t earned from an accounting point of view, and the spending ran away from them, and the revenue didn’t pop up. Later on, they adopted a financial system which solved that problem, and then they instituted an overhead management system, and that was the cause of some dissention, because how you allocate overhead to different operations makes those operations look good or bad, and groups that were not getting allocated overhead in the original computations all of a sudden were getting overhead, so they were upset, and so it had to be worked out in management. And you’re dealing with amateurs that don’t know overhead or G&A, and then as I said yesterday, on the SIG board, they were charging for the-- I think there were two things; there was an overhead charge, and there was also a direct charge, and the direct charge was like 20 cents a year a member that had to be assessed against the SIG board, but that was the way they got to moving the money around between the different pots. I thought it was quite rational. I once told the CFO at SRI that he was the single most important man in the organization, because with just the flick of a pen, he could make somebody profitable or not. He could just say, “All right; we’re going to change the assumption. Wet lab space is three times more expensive than dry lab space.” And that would automatically put some groups out of business. But it got more professional over time. It really did, it got much more professional.

NORBERG: Did it turn to the outside to get that...

BRANDIN: Well, I brought in my Peat, Marwick guy to talk about the way you do overheads. I don’t know if they turned to the outside or not, but I think from Jean forward, all of us, including me, at least until I stopped being around, and certainly Adele, kept on pounding at more professional accounting, although I don’t remember if they went to outside firms or not. I don’t remember going to outside firms.

NORBERG: I would think they do now, because the budget is so large.

BRANDIN: Well, they have auditors. They certainly have outside auditors, but I don’t know if the CFO brings in a firm to help him do that, or he just brings them in for the audit.

NORBERG: Many organizations are always in a financial crisis, and you’ve only talked about one financial crisis during Sammet’s period, but there must be varying levels of financial crises within ACM over the years that you were with them.

BRANDIN: Well, the second financial crisis that I really knew about was when the AFIPS money dried up. I was not president; that was Adele. Otherwise, the financial crises were picayune. I mean, you wanted a little more travel money for the chapters or for the regional or for the lectureship series, so instead of doing 80 lectures over the year, you can only do 60 or 40. I don’t think they were of any consequence compared to the Sammet problem and the AFIPS problem. They were piddly. And as I say, I used my discretionary presidential funds to oil squeaky wheels.

NORBERG: Did anyone at the time—speaking of the ‘70s and ‘80s, still—think about how best to allocate resources, or was that a full-blown...?

BRANDIN: Well, this is beginning to sound a little Kafkaesque, because the only two places where the resources could be allocated were publications and the SIGs. The other stuff was in the noise. Maybe the lectureship series. But education was getting all that it could possibly get. Putting more money into education wouldn’t have developed any curriculum any faster. You could have had more lectureships, but in order to have more lectureships, you really needed more chapters, and the chapters were a weak sister in the organization. In fact, they had a lot of political attention, but they didn’t produce anything. It was like a black hole. So the SIGs and the publications were where the allocations went, and that’s where the revenues came, and the conferences.

NORBERG: We were talking about the financial aspects of ACM. Were there other publications or activities that you now wish you had supported?

BRANDIN: No, I wish I had been more insistent about killing Reviews—or not killing it, but more negative. I just made negative comments, but I never actually tried to stop it.

NORBERG: And no one has.

BRANDIN: As far as I know, no one has.

NORBERG: Okay, let me skip over Jean Sammet again, and just for completeness, let me ask you what some of the other positions you held within ACM were, besides being president, secretary, chair of two of the boards.

BRANDIN: And a SIG.

NORBERG: So that’s five roles. Were there others besides those five?

BRANDIN: I’m sure I was on other committees, but I don’t remember. Maybe we can table this one until tomorrow, and I can see if I remember some tonight. I was on one of the AFIPS committees, too. I was on an AFIPS committee called The Vulnerability of US Society to Computer Abuse or something like that in ’74, maybe or ’76. We basically said that, “Yes, we’re vulnerable, but we’re not really, because there’s so much redundancy in the system that if somebody brings down the air traffic control system, people can take trains.” This was of course at a time before the Internet was so pervasive. But I think even today, it’s still pretty redundant and pretty safe.

NORBERG: I don’t think I would want to switch to trains.

BRANDIN: Well, the point is that the society would continue to function. It might be impaired, but we weren’t going back to the stone age.

NORBERG: To your knowledge, did the manner of ACM's origin leave any imprint on the organization, or was there a major turning point that changed the character of the organization?

BRANDIN: Well, it sounds to me like that's a Jean Sammet question. It sounds to me like a survey question, where you already know the results you want from the survey. Well, the organization that was set up in 1947, I have absolutely no idea if the constitution that they adopted in '47 was the constitution I found in '67. The constitution was pretty reasonable. The AFIPS constitution was completely flawed, and created this deadlock in the board of directors that allowed conspiracies to change the original intent of the founding members, but I don't know if that was the case in the ACM constitution.

NORBERG: Was there a watershed in ACM?

Session 2, Tape 3, Side A

BRANDIN: Is there a watershed in ACM? I suppose if there was a financial crisis that, you know, hit the wall in about '68, say or '73, and after that financial crisis, the thing has been pretty much ballistic. It's been catering to its audience, and it's in a rut, but it's doing an all right job of catering to its audience.

NORBERG: Has the audience changed?

BRANDIN: Not really. I think if you were to look for the statistical average ACM member, it would be an associate professor of computer science at, I don't know, the University of Michigan, or an assistant professor. And there would be a bunch of graduate students on the bottom side, and a bunch of senior guys on the other side. But you won't find the kind of people that you might have found in the '60s there, because in the '60s you had people who worked at Standard Oil and at IBM, and they were selling computers, and they were building computers, and they were making supercomputers and stuff like that. I don't think you'll find a lot of those kinds of people at ACM anymore; you'll find people down on the academic spectrum.

NORBERG: Now, that leads to a question, though, about computer manufacturers. Was there any interaction, I'll say, between ACM officers or whatever else and the computer manufacturers?

BRANDIN: Well, I mean, there were. You had a bunch of ACM presidents that came from IBM. You had Sammet, Carlson. Isn't Feldman from IBM? Fran Allen is from IBM. She was one of the leaders. There was a lot of interaction at the research laboratory level. I don't think there was any interaction at Armonk. And there would have been research activity at Control Data and UNIVAC and RCA, you know, during the days of the seven dwarves, but I don't think there was any corporate activity. Maybe occasionally, someone will try and get some money out of corporate for a student programming contest or something, but that's not institutional. Oh, DeBlasi came from IBM, came from a very high-level position in IBM. But I don't see it as--there's not a program inside the organization where there's a tickler file and there's someone is charged with the responsibility for maintaining the relationship. I always wondered why we didn't do that, and I also thought that we should have an additional sub-association,

subcommittee board called the Association of Former ACM Presidents, because I thought that if - you know, we could spend a little travel money, too, but it would seem, to me, useful for the executive director, CEO, and the president to assemble all of the former presidents once a year and ask them questions and get some guidance and get some feedback, and no one ever did that.

NORBERG: Why do you think they didn't do it? It seems like a natural.

BRANDIN: Well, I thought about doing it, but then I realized that most of the former presidents hated each other. [Laughter]

NORBERG: The way the History of Science Society has dealt with this is that past presidents automatically become members of the governing council.

BRANDIN: Well, in ACM, you are president for two years. As past president, you're on the Council, and when you become former president, you're no longer on the Council. So you're only there for four years.

NORBERG: Well, it's a smaller group, so they can only do this from time to time. Was ACM, to your knowledge, ever involved in any government initiatives? That is, did they take grant money from NSF to look at problem X, publish a report, send it to NSF—whatever?

BRANDIN: I do believe that in the area of online publishing, they had grant money, looking at online publishing. I know at SRI, we were-- I just don't remember if the money was running through us to them, or them to us. But I believe there was an NSF grant in the area of online publishing, and this would have been somewhere around '80.

NORBERG: What about all the science and technology manpower requirements that people were looking at during the Cold War?

BRANDIN: Well, if anybody was getting money, it was probably individually or through their organization. I don't think it was coming through ACM. ACM did not have a security clearance that I know of. We didn't have a classified container. We didn't have a secure facility, so I don't think there would have been any defense-sensitive stuff being done at ACM. I mean, I had a lot of classified research at SRI, but none of it through ACM.

NORBERG: Well, I was thinking of some simple things like scientific freedom and human rights, and maybe something on public policy issues to do with the computer. When you think about the Computer One and Two put out by the Department of Commerce through one of its offices, which changed the nature of telephone activity in the United States—it laid the whole thing open for the possibility of people to set up phone companies that would be in competition with AT&T.

BRANDIN: Are you talking about when Ma Bell got broken up?

NORBERG: Yes, around '82.

BRANDIN: Well, SRI had a lot to do with that. SRI had a lot to do with trying to not...

NORBERG: That was my position at the time, but I had no power to-- I think it was a terrible mistake.

BRANDIN: We had a bunch of DCA money to try and analyze the possibilities-- the results that would-- the defense that would be incurred by that. But no, I just don't remember ACM ever getting grant money. It probably did, though.

NORBERG: Okay. Just trying to cover all the bases, here.

BRANDIN: I think it would have been dependent on, you know, who was the president at the time, and the executive director, and who knew how to get money.

NORBERG: Well, some of those people knew how to get money.

BRANDIN: Yes, but why would they do that through ACM?

NORBERG: Okay, I see. That AFIPS panel you were just talking about, was that on the resiliency of the...?

BRANDIN: Yes, [inaudible]. I may have the report.

NORBERG: Was this your view of the society? You just expressed it.

BRANDIN: Yes.

NORBERG: I'll just drop that.

NORBERG: Was there any relationship between this panel's work and the book that you and Michael Harrison wrote?

BRANDIN: No.

NORBERG: Okay. I was going to say in addition to what you covered in the Department of Commerce study, and then in the...

BRANDIN: No, and in the book, we don't really get into that issue of resiliency.

NORBERG: Okay. So it had no effect?

BRANDIN: It wasn't really relevant.

NORBERG: I'm tempted now to talk about the *The Technology War*, but I'll let that go for the moment. You had a group of successors as president, and some of them are better known, I think, than others. What did you think of these people? What kind of presidents do you think that Paul Abrahams made and Adele Goldberg and so on?

BRANDIN: Well, Adele Goldberg is a very bright woman, and extremely competent—probably a good manager with-- but you know, everybody has a couple flaws. I think Adele was a good president. She drove people crazy, but she was a good president. Paul is a very smart guy, but I don't think of Paul as a manager. When Paul wants to read something for pleasure, he reads a mathematics book, and I think he lives in a much more esoteric world than real, practical managers. John White struck me as an okay president. I agreed with Adele that we should try and talk John into running for president. I talked Adele into running, and she talked John into it. And I had known John for a long time at Xerox through SRI and whatever. I don't know who came after John. Do you have a list?

NORBERG: No, I don't have a list with me. Frailey was in there somewhere.

BRANDIN: Denny is a good guy. I like Denny. Denny is good. There was a guy from Boston, I would not think very highly of him, and I almost gagged when I heard him give a presidential speech when he talked about how this was his first term. And I kind of laughed my way out of the audience on that one. Gwen Bell was okay. Charlie what's-his-name...

NORBERG: Bachman?

BRANDIN: Was he the one who was president after Gwen? No, it wasn't Bachman.

NORBERG: No, he was earlier than that.

BRANDIN: We need a list of presidents. Why don't I go print one up? Irene Holliser, Executive Secretary, was the Pat Ryan of ACM before Pat Ryan—a wonderful woman, class act. And she used to list the former presidents, and when they were dead, she would put an asterisk next to their name. I told her, "You've got to get rid of the asterisks, because I don't ever want to think about an asterisk being next to my name." To which she responded by taking off the asterisks. Okay, so after me, there was Adele—competent. Paul Abrams, not a manager. Bryan Kocher, who thought he was going to run for two terms. I would probably give him a C+. John White, okay; Gwen Bell, okay, but she got ill, I believe. Stewart Zweiben, B; Barbara Simons, well, Barbara is one of these social freedom and USACM and public policy and all of that stuff, so if you're into that, I'd give her an A. As a manager, I don't think she's much of a manager. Maria Clau, I don't know at all. I didn't really know her, and I don't know what she did. Dave Patterson, I think, was a great president, because he was really interested in the press—you know, getting the name out. And then we've got Stu Feldman, who I don't really know at all. I left out Chuck House. Chuck House came after Stu Zweiben. Chuck was the one who told me, "Yes, go ahead and hire D'Auria." And then he and his executive committee caved and left me out on a limb. He was president of that council meeting when they changed their mind.

Now, you asked me what were the AFIPS board meetings like? This was an AFIPS board meeting.

NORBERG: Can I have that?

BRANDIN: Yes, I'll make you a copy. What else did I bring? I brought the privacy protection study commission testimony, if you're interested in that. That's SRI.

NORBERG: Okay, well, let me come back to somebody like Adele Goldberg who succeeded you as president. Did she continue to promote issues that you thought were important in your final statement, let's say?

BRANDIN: No. Well, she always laughs when I say it, but I generated \$1.5 million, and then she pissed it away. She had this very serious problem with AFIPS to deal with, and we knew it was clearly coming, because the whole thing was going down the toilet in my term. Us and the IEEE, we were doing everything we could do to beat some sense into Sylvia, the president of AFIPS, but it wouldn't take. So Adele had this impossible problem, and then she decided she wanted to also replace the executive director at the same time. I would say that the financial security that I managed to leave, she dissipated—some of which because she had to, and others because she just wasn't willing to tighten the belt. Because she could have tightened the belt; she could have cut down on the spending. In fact, she had to cut down on the spending anyway, because there was less revenue. She could have also gone to the SIGs and borrowed money from the SIGs, but by then, it was so emotionally institutionalized that I don't think it's possible to take any money away from the SIGs now, although legally there should be no problem at all to take the money away—absolutely none at all. But I'm not sure now; I just don't know.

NORBERG: What do you believe was your most important legacy as an office of ACM?

BRANDIN: I brought peace to the Council. There was a dissent—a seething. I used the word “bonfire” yesterday. It was a seething, angry, partisan, unruly group, and I brought peace.

NORBERG: That's quite an accomplishment when you think about it. I expected you to say the accreditation issue.

BRANDIN: Well, that was the most important program I got approved. But you can't imagine what the Council meetings were like, you know, to try and get business done, when you're in a meeting where everybody hates everybody, they're not screaming, they're not swearing, but they're not really talking to each other. It's like a divorce; they're talking like in a divorce where they're skewed; the lines never meet.

NORBERG: Now, is there a role to be played by the past presidents? You suggested that you have an idea.

BRANDIN: I think that if they would listen, they might hear things that would be useful. And I would be happy to join in a group of former presidents—even the ones that I didn't like.

NORBERG: What sorts of things would you propose now?

BRANDIN: Well, first of all, I would like to hear what's going on with membership. I would like to know what the numbers are. I don't know what the numbers are.

NORBERG: Don't they have to be published every year?

BRANDIN: It's supposed to be. If it is, I don't know where it's published. It isn't in the annual report anymore.

NORBERG: It's supposed to be in a public place in the principle journal.

BRANDIN: Well, that's right. I mean, Communications is a circulation journal or whatever they call it now. It's supposed to be in there for the advertisers, right?

NORBERG: Once a year. There's no requirement to do it every time.

BRANDIN: Well, it used to be in the annual report, and the annual report no longer tells you what the membership is, and tells me what the balance sheet is, and tells me what the income statement is, but it doesn't compare them to previous years like it used to. So I figure they don't want to publish the numbers because they suck.

NORBERG: Well, perhaps that could be true, but what sort of things could past presidents do—certain responsibilities that they dig up?

BRANDIN: Besides going to the funerals? I volunteered that to Gene Spafford. I am on his committee—the USACM. And you want to know what we're good for? Well, we want to volunteer, and I said, "Well, I'll be happy to go to funerals of dignitaries in the computer business." Also, I'm good on this question of scientific cooperation with the government." That's a very sensitive issue, and I think I did a good job, and I think I can do a good job representing science without selling them down the river. I think we could listen to their problems and give them good advice. We could listen to their problems on various committees. Awards is always a snake pit. It's always a snake pit. Should there be more awards? Are they doing a good job at giving awards? They have new membership categories, new awards categories now. I never heard about these; they just came out of nowhere. Why they have them, I don't know why they established these. They had a fellows program. What else did they need?

NORBERG: Well, to belong to a professional society, there have to be hierarchies of authority and presence, so you want to give more awards so that you can keep the organization going.

BRANDIN: Well, I like awards; I like contests; I like tee-shirts.

NORBERG: I noticed.

BRANDIN: I understand all of the management incentives and motivations. But I remember the debates-- Jean wanted to start the Fellows Program when she was president, and they didn't want to do it, because they didn't want to. This was such a divisive issue. Some wanted fellows, like Jean, and others said, "No, no, no; we're all the same. We don't want to have a special class of membership. We don't want to differentiate; we don't want to distinguish. We're all equal." And so they did not adopt a fellows program during Jean's presidency. I don't remember when it came in. I think it was McCracken. No, it was after McCracken.

NORBERG: It would have to be after McCracken.

BRANDIN: It would have to be after McCracken. And then they had this weird automatic system of establishing the first set of fellows. It was really weird. But if it hadn't been for McCracken, I have a feeling I wouldn't be a fellow. So anyway, but you can have more awards, but now they have new categories of members. And I don't really understand—new senior research members or senior something. We could give advice about stuff like that, and talk about previous problems and raise issues. We're senior, with a lot of experience.

NORBERG: My thought is that no group of officers want to have that sort of advice. 1960s, 1990s, it doesn't matter.

BRANDIN: Well, it's a good excuse to have a party.

NORBERG: You need an excuse to have a party?

BRANDIN: Well, if there is no excuse, there isn't going to be a party. I don't see anybody saying, "Let's do it."

NORBERG: Well, SIGBOOZE is one place where you don't need a reason.

BRANDIN: There is no SIGBOOZE anymore.

NORBERG: Oh, that's too bad. Why did it fall apart? Everybody got too old and couldn't drink as much? That's what happened to me.

BRANDIN: Alan Perlis told me the reason why he stopped coming to meetings. I asked him, "Alan, why don't you just stop coming to meetings?" He was at an AFIPS reception or something in a wheelchair. He said, "I used to come to the meetings because my friends were there, and my friends aren't there anymore, so I don't go to the meetings." And that's what happened to SIGBOOZE. We founded SIGBOOZE in 1967 in Washington, and eventually there were five founders of SIGBOOZE, and in 1990, 23 years after the founding of SIGBOOZE, we had the 25th anniversary of SIGBOOZE in Washington, DC, and that was it; that was the last official SIGBOOZE. And then most of us stopped going to meetings, although that was the year the five started an annual adventure, like a City Slickers adventure, and we still do; we go every year, approximately the summer solstice. The five guys who founded SIGBOOZE go off somewhere—Scotland, Lake Mead, sail off to Strait of Juan de Fuca, Ireland—for a long weekend to celebrate the good old days. But there's no more SIGBOOZE. The guys who picked it up just didn't have the class.

NORBERG: You mentioned two things yesterday that we didn't talk about. One of them was your uncle's story.

BRANDIN: That's really an SRI story.

NORBERG: Then we'll take that up at a later time. The other was IBM stories.

BRANDIN: I gave you the Symbolic Control story about APT, but I was also involved in other litigation with IBM. Again, it's not ACM, though. But I worked for the lawyers who defended

NEC in NEC v. Intel. That was a micro-code copyright infringement action. And I worked on clean room procedures, I believe—software clean room procedures: what are industry standards for software clean rooms? The purpose of a clean room is to extract the idea of the code without extracting the expression.

NORBERG: What?

BRANDIN: Remember, the expression is what's protected. So when you want to reverse-engineer or duplicate or be compatible with, you've got to know what the idea is, but you can't use the expression. So a software clean room is for the purposes of extracting the idea without extracting the expression.

NORBERG: Sounds convoluted to me.

BRANDIN: Well, you can thank Dan McCracken for that. Then I worked for-- what was the other one?

NORBERG: Award Software?

BRANDIN: Well Award Software, I know-- Susan Nycum, who was the chairman of the legal issues in computing committee at ACM. We're buddies. And she was on the board of Award, and Award got sued by IBM for allegedly infringing their BIOS. So she asked me to come on board and join Award to help them negotiate a settlement with IBM, because I had experience negotiating settlements with IBM. So I did that, and we ended up-- Award settled amicably with IBM about that infringement. And I had to go to Taiwan and find out what-- there was all kinds of interesting technical stuff, but again, I'm negotiating with IBM lawyers who know my background. But the NEC thing was not IBM.

Okay, then one day, a phone rings in my office at SRI, and it's the president's secretary: "Please come to my office." I go down to the office. Dan Evangelista, deputy-general counsel—perhaps now general counsel—of IBM is there. IBM has just settled with Hitachi over a trade secret action. Actually, I don't know if it was a settlement or a court-imposed settlement, but they had been told to go into arbitration, they had each picked one arbitrator, they had jointly picked SRI as their neutral arbitrator, and they needed from my division—computer science division—a hardware person, a software person, and a manager. So I offered myself and a few engineers.

"Okay, your hardware person is blah, and the software person is blah, and I'll manage the project." And Dan, who I had just spent six months negotiating the settlement with, says, "Oh, you know, Dave, you still have your antitrust lawyer, and you know, I don't think-- I think it's just better if you're not involved." I said, "Well, okay. As long as my division gets the money, I'll get you somebody else; I'll recuse myself from the project." So I did, and I had three people on project for IBM working on the thing. It was completely unnecessary. I was perfectly capable of being neutral. I mean, I even bought their computers. So then in 1974 or so, we started working for Fujitsu. And remember, I mentioned this guy yesterday. He was a very senior guy at Fujitsu. Well, when he was senior executive managing director of Fujitsu and owned all of systems engineering—including all of software, about 18,000 people—IBM sued Fujitsu for infringing code in the operating system that-- in OS 360 that presumably Fujitsu was using in the operating system for their Machine Lines, which were IBM knock-offs. So I left

SRI in '86, and this is approximately the same time, and I proposed to this guy at Fujitsu, "Hey, I'm going to leave SRI, so how about you buy half my time?" I went to Tokyo, and we're at dinner, and I knew there was something brewing, because there was another senior executive managing director there—the guy in charge of hardware—and a translator, and they never, ever brought a translator to the meetings, because we never needed one. I mean, their English was good enough, or there would always be an engineer there who was not a translator who could deal with special problems. So this guy says, "You know, Brandin-san, we see from your resume that you have been in litigation with IBM."

NORBERG: Leading question.

BRANDIN: "And we wondering, I am perfectly happy to buy half your time now, but perhaps you would consider, before we start that project that you would work for our attorneys who are defending us in the IBM copyright infringement action. They need a consultant, and we know that you understand that." I did a bunch of things in terms of doing research, but establishing information they needed to know about—software clean rooms, and reverse engineering, what's the industry practice, and stuff like that. And then it turned out that it went into arbitration, and in arbitration, Ellen Hancock was the IBM person who trained the neutral arbitrator, and I was the Fujitsu trainer who trained the neutral arbitrator so that we both were training the arbitrators, and I did that for quite a while, and then I went back to work and established a relationship with Fujitsu of my own. The SRI one lasted 17 years, but I was consulting to them for, I think until '93, half time, steady state. It was a wonderful deal, wonderful deal. So I had a lot of interaction with IBM and the IBM lawyers. I'm sure that I'm on an enemy list. I'm positive.

NORBERG: Well, as I look down the list of companies that you say you were associated with in the period after, well, say, 1982, since all these dates are in the '90s, and Fujitsu is not on it.

BRANDIN: No, I was a consultant. It was a client.

NORBERG: And these are official positions?

BRANDIN: A lot of this was done in parallel. I had a company called Strategic Technologies, Inc., which unfortunately, I forgot to renew the incorporation papers one year in Illinois, and then I lost the name, and came back as Strategic Software Services, Inc.—same company. That company did a lot of consulting over the years, even while I had these regular jobs. I kept them in parallel. In fact, when I was with McKinley-- was it McKinley? No, no—AT Kearny. I don't know where McKinley came from. Oh, that's a different McKinley. AT Kearny, I was actually doing consulting for Fujitsu and pushing the money through ATK, so it looked like ATK business. But I kept that relationship with Fujitsu all those years, and I was doing other consulting at different times. Like I might have had a different client when I was working for Interop. While I was working at Interop, I kept my relationship with some of the Japanese.

NORBERG: Now, how is this possible?

BRANDIN: The bosses agreed. It wasn't done on the sly; it was completely above-board. Well, you know, from '86 to '90, I don't think I had a real job between '86 and '90 then; I was just consulting, and I was on Board of Directors of Interop. Yes, I left SRI in '86. So from '86

to '90, I was just consulting and on the board of directors of a bunch of companies. I was on the board of a company in New York called Fifth-Generation Computer Systems. The chairman of the board of the company was Jacob Sternberg, and they were using Columbia technology for binary computing—binary architecture computers. The board of Interop, the board of Award, and more. And then Interop, we sold Interop to Ziff-Davis. Do you know the story of Interop?

NORBERG: No.

BRANDIN: It's a very interesting story. Dan Lynch was the founder of Interop, chairman of the board, the one who made the real money. He had worked for me at SRI. He ran the data center and the AI center, and then he had done some consulting for me on my Fujitsu contracts in the area of, did they really infringe, and in what area, and why does it affect performance. And Dan was very close to Bob Kahn in DARPA and the Defense Communications Agency. Dan had an idea one day. He said, "You know, this question of interoperability, there are people out there that are building networks, and there are people out there doing research in networks, and the Defense Department wants to be able to buy routers, and none of this stuff is interoperable. And there's no place where they can come together and talk about interoperability." He talked to the Defense Communications Agency, I think, into coming up with \$50,000 to run a little conference in Monterey. Then he borrowed some money on his house, asked me to join the board of directors, and we put together Interoperability One. 200 people came to the conference, paid \$1,000 a piece. So the conference made money, and at the meeting, he said, "You guys want to have another meeting?" And they said, "Yes."

So we had another meeting in Washington seven months later. 900 people showed up at that one. And at that meeting, they said they wanted exhibits. Or maybe it was the next one that they wanted exhibits. They wanted the manufacturers to have exhibits. So anyway, it went 200, 900, 2,000, 4,000. By then, we had exhibits. I think by the time we got to 8,000, we had constructed a thing called InteropNet, and if you wanted to exhibit at Interop, you had to connect to the network and demonstrate interoperability. And we started offering tutorials, and by August of '93 in San Francisco, we filled up Moscone Center. We had 53,000 conference registrants, including 6,000 tutorial attendees paying \$1,000 a pop. We sold the company in 1990 to Ziff, and in 1990, I was a director, but I joined the company as Senior Vice President for International and Technical Operations. So I owned the conference program; I owned the tutorials. My group built the network, InteropNet, and I launched the international shows. I launched Washington and Paris. I quit the day of the Paris show, but I had launched it, hired a group in Paris. I launched a show in Tokyo, which to this day still fills up Makahari Center, 50,000 square meters.

NORBERG: And is it still doing business under that name?

BRANDIN: Yes. It's changed ownerships a bunch of times, but I think it's still doing business as Interop now. Hey, we drove out the major competitor in the networking business. There was a show called Net World which was-- I don't remember who it was run by. We drove them out of business, too, but it was Novell's show, and eventually Novell came to us and said, "How about we integrate Net World with Interop?" And it became Net World Plus Interop for a long time. We were getting 100,000 attendees in Vegas. It's a big show. That's Interop. Well, I left Interop in '93 and went to work for IDG. I was the Executive VP of World Expo for International Data Group. And I had shows all over the world. While some of this was going

on, I think I still might have even been consulting to Fujitsu occasionally, and I think I may have left out some jobs, too, because I think there was an AT Kearny period. Is that on the list?

NORBERG: No.

BRANDIN: Well, some time between '88 and '90, I was an advisor to AT Kearny Technology, a consultant, a vice president, and then I was fired. Actually, I was fired when they did a ram-down merger of ATK Technology up into ATK. Then I became a consultant for a while. So it was all very, very slippery. In '95, I left IDG, and I went to McKinley. McKinley was the builder of the Magellan Search Engine, which was an engine that competed with Yahoo. And from there, there were a few things that happened, but the key thing on the McKinley thing was that the chairman of the board of McKinley ended up being the chairman of the board of Critical Path, and I made a bundle on Critical Path stock. I was only on the board of advisors of Critical Path, but I was not an employee. The stock was worth a fortune. And then Women.com was my last real job. Then they were-- there was a lot of Hearst money in Women.com. I was doing a lot of international stuff, and then the market collapsed in '90.

NORBERG: In '90?

BRANDIN: I'm sorry. In 2000–2001. And then after that, I was consulting with my buddies in New York for a while. They do a lot of public safety work.

NORBERG: The IMI group?

BRANDIN: Mm-hmm [yes]. And then over the last couple of years, I basically said screw it, and stopped working. I started writing. I started writing fiction on the eighth of September last year.

NORBERG: Good for you. There are some discrepancies, shall I say, in what I read and what I see in the various documents that are available. You continued service at SRI after you were done with the presidency and so on. 1972 to 1986, you were listed as vice president...

BRANDIN: No, I wasn't a vice president that entire period; I was at SRI that entire period. I was a vice president of SRI from...

NORBERG: '78 to '83?

BRANDIN: Well, no, until '86. The question is which year did it start? I was either executive director or vice president from '76 on. No, it would be '78, '78, when I came back from Washington.

NORBERG: '78 to '83, vice president and director of computer sciences and technology division, SRI research group.

BRANDIN: And then '83 to '86 vice president director of the information systems and services division.

NORBERG: The 1972 there threw me off. That was from what? Did you say 1978 to '86 for that, too?

BRANDIN: Computer science division was '78 to '83. The other division was '83 to '86, and then between '72 and '78, I went up this various little ladder we talked about yesterday—the research engineer, department group, department head, laboratory director, division marketing guy, executive director, and then vice president.

NORBERG: Now I'm going to try and get to the bottom of this. In the citation for the ACM Distinguished Service Award, the following paragraph seems to me to be the most significant: "Your technical contributions include seminal work in artificial intelligence, telecommunications, robotics and vision, computer science, and applied systems engineering, and to furthering the international dissemination of emerging information network technology." That is more than being a manager.

BRANDIN: Well, I'm not sure who wrote that citation. It was probably my buddies. But it would reflect the fact that I provided a fertile environment for research in all of those topics, because those topics were all the topics in my computer science division that flourished under me, and I created that division, although I have to admit I created it out of existing units. I brought it back together, or I made it come together, and not only that, but for spending the IR&D money, I insisted that I would only give them the IR&D money if there was interdisciplinary cooperation on projects, and not all of them but some. So it's probably written that way in order to make sure you get the award, but it also reflects the contributions I made there.

Now in my consulting practice, and in the work we did at SRI, we were very interested in technology transfer, and we did consult in how to transfer technology—help Fujitsu be compatible, help Berlin learn how to attract software industry, help BMFT concentrate their research in areas that the German computer industry could use rather than having them do research in fields that weren't being commercially exploited by, you know, like, Siemens or whoever it was. So does that answer your question?

NORBERG: Yes, but I want to change the question a little bit by asking you what you think your primary contribution to the field has been—the field of computer science and engineering, now.

BRANDIN: I would have to go back and say that the weapons work that I did in the '60s was the most significant technical accomplishment.

NORBERG: Can you talk about that a little more, or is it classified?

BRANDIN: Yes, I can talk about it. And Interop was the most significant commercial contribution to that area.

Session 2, Tape 3, Side B

BRANDIN: It was a national asset. Without Interop, the Internet would not have developed at the rate it did.

NORBERG: Would you get the people on the Internet Society to agree with that?

BRANDIN: I think they would. I think Vint Cerf would be the first one to agree with that. It was the only place you could go where all the stuff worked, and we created, you know, an excitement or a verve that the world picked up on, because otherwise, the research committee was off doing other things. And guys at defense, they didn't know where to even go to buy equipment. And Cisco wanted to sell routers, but there was no market.

But I would say the most interesting technical problem I worked on was the weapons project—the simulation of six-degree-of-freedom interceptors. It was a lot of work. In terms of actual research at SRI, packet radio would have been the most significant project I worked on, but my contribution there was mechanical; I built a terminal. I was on the packet radio working group, so I was in the meetings, so I would listen to Norm Abrahamson talk about how he had a great idea in the shower, and Fralick and Kahn would argue over the physics—partial differential equations all over the blackboard. And I understood the math. But I don't think I ever did that many, let's say, fundamental contributions to the--

NORBERG: Well, I didn't want to make you think it was a fundamental contribution, because people make different kinds of contributions.

BRANDIN: I made a commercial contribution, and a technical defense contribution.

NORBERG: What made you go into the consulting area?

BRANDIN: It was the Stanford education, the summer at the Graduate School of Business. Well, what do you mean by consulting? Do you mean management consulting, or just going into the research...?

NORBERG: Well, all of these companies.

BRANDIN: Oh, well, I was looking for gold. Interop was fun, and during the first three years, there weren't even any fees. I was on the board; I would go to the board meetings, and the fee would consist of a shot of scotch. And it was amazing to just watch the thing grow. It was so exciting, and it was wild. I think by '89 we had about 30 employees, and I think maybe the director fees were starting then at maybe \$1,000 a month or something. That was a rocket ship. That was great. But all the other companies I was looking for more rocket ships, and I was also slowly getting ready to retire.

NORBERG: Okay, IBM stories, we started there, but I don't recall you said very much except for the lawsuits.

BRANDIN: Well, the Fujitsu one was interesting. Everyone was going up against Ellen Hancock, and I told Fujitsu that they should not settle. And they settled, and it cost them almost a billion dollars.

NORBERG: Ooh, that's a lot of money.

BRANDIN: My attitude on that was, I don't know if-- I won't comment on the question of whether they actually infringed or not. No one ever admitted to me they did, and I don't know if they did, but the area in which the infringement occurred was a very high-performance issue area—LU 6.2, I think it was called, Logical Unit 6.2; it was a disk controller area. But my attitude on settling was, the reason Fujitsu cared was because their customers wanted them to be compatible with IBM, so they needed to be compatible with OS 360. But OS 360 was an obsolete operating system, and it was on its way out. So my attitude was, well, live through the transition, and then forget it. I don't see any reason to-- But you know, they may have had them by the throat, and they may have had to settle.

NORBERG: IBM had Fujitsu by the throat?

BRANDIN: Yes, they may have; I don't know. I mean, it was in arbitration, so it wasn't in court yet.

NORBERG: And did that settle the case?

BRANDIN: They settled. It went away, and they got access to OS 360, and they set up a software clean room. I'm telling you, it's a very intense process. You have two teams of people. Think of it this way: you have one set of guys who look at the code you want to be compatible with, and they write down what the code does. And then they push it through a semi-permeable membrane to a different team who have never seen the original code, and the new team writes new code.

NORBERG: To do what?

BRANDIN: To do what the old code did, but without stealing the expressions.

NORBERG: Oh, okay, yes. Now I understand.

BRANDIN: It's nuts! It's nuts.

NORBERG: The whole field, sometimes, seems slippery.

BRANDIN: It's not really science; it's engineering.

NORBERG: So you don't think there's any science...

BRANDIN: Well, sure there's science. Of course, there's science in the physics of electricity and magnetism.

NORBERG: I wasn't thinking about physics? But in the case that we're dealing with here, the knowledge base in computer science, some of it must be science.

BRANDIN: Some of it must be. Some of it is certainly mathematics. And you can argue that there are experimental requirements, too, and demonstration requirements, and measurement issues. You can certainly argue all of that, but it's a push. It's a push for me.

NORBERG: So would you want to put the whole thing into computer engineering, perhaps?

BRANDIN: I would rather call it that.

NORBERG: Because we went through these debates on the Minnesota campus when I was in the computer science department, and the question always was, well, there's a science in computing, and somebody would speak up and say, "Well, for me, it's engineering; there is no science," but they could never decide to call the department computer science and engineering. They didn't want to do that; they didn't want to put the "engineering" in there. And then the electrical engineering department beat them to the punch.

BRANDIN: Yes, they call it EECS or something, right?

NORBERG: Right. And so now there was a real problem, and so what they did was they just changed the name of the computer science department. It didn't satisfy me. Of course, I'm an outsider in the field, for the most part. I did have a tenured position in computer science, but that's because the university wanted me to do something else, to administer this new institute. But I could never understand the computer science field. It took me a long while. When does a conference paper merit to be on a list that you can get tenure with? I just couldn't see that at all.

BRANDIN: Let me tell you, not tenure, but refereed papers. In my opinion, it is a highly politicized process to get a paper refereed. Who do you know and who is your PhD advisor has more to do with the paper getting approved for a journal than the quality of the paper. That's in my opinion. Second, I believe that the peer review process in military black programs is better than the peer review process in the open.

NORBERG: Now, can you explain why to me?

BRANDIN: Well, I can't explain my specific project experiences, but I can say that my experiences in classified programs with peer review is it is extremely high-quality. They don't care who you went to school with. It doesn't matter who you know; what matters is, does it solve the problem? Does it address the problem? And in the open world, it doesn't matter if it addresses the problem; it's who you know. It could be completely theoretical, but it doesn't matter; it's irrelevant in many cases; it's politicized. So I don't have a lot of respect for the peer review process. I think the same thing happens a lot in grants, that the money coming out of NSF or AFOSR or ONR, that you very frequently find money being granted that should go somewhere else and doesn't.

NORBERG: Interesting. The is only one more thing that I would like to do, but probably not today, is the litigation issue that we talked about yesterday—that is, taking me through the process.

BRANDIN: Okay. Well you might want to read this article tonight. It's flagged. A lawyer wrote this, and I think it starts on page 241, and our part ends at 260. You might want to read it. I'll be happy to go through the litigation.

Session 3, Tape 1, Side A

NORBERG: What I asked you about yesterday, that we would do this morning, has to do with litigation. I've read the article you recommended to me on the Symbolic Control case, and I had chosen two others just because I thought they might be interesting: the IBM versus Award Software and the other one on Micro Electronic and Computer Technology Corporation. What I'm interested in hearing from you, with respect to any one of these cases, is first of all who's involved? What are the companies? Who are the people that you remember and so on? No proprietary information, although it's probably long unused. But nevertheless, I'd be interested in how you went about deciding what the state of the art is and therefore what case you had and what role the lawyers played in all of this in developing the information, the data, the discovery process to pick out materials that would be relevant to the case. In the Babbage we have about eight or nine litigation collections, and they're enormous in some cases. The CDC versus IBM was enormous, and I'd like to learn how to pare that down so that you can do an effective article, unlike Lee, in regular layman's language. That was very legalistic. ["Symbolic Control, Inc. vs. IBM: Eliminating Affirmative Defenses in Antitrust Treble Damages," Bartholomew Lee, *Golden Gate University Law Review*, Volume 11, Spring 1981, pp. 241-260.]

BRANDIN: Well, I actually am considering writing a book on Symbolic, and I wrote a preface for it, which I call "A Day of Infamy". I consider the Day of Infamy to be June the 23rd, 1969, the day IBM unbundled.

NORBERG: They said.

BRANDIN: It's a cute story. What was the third case you mentioned?

NORBERG: Micro Electronic and Computer Technology Corporation and the Justice Department, 1984.

BRANDIN: Okay. Well let's do that one first because I think it's an easy one. At the time of the incorporation of MCC, which was a consortium of electronics companies formed to do research in the new fifth generation technologies—in a sense it was a response to the Japanese threat of the fifth generation project. All the big guys were in it and they hired Admiral Bobby R. Inman to run the company. At the time, they had problems with antitrust law because, presumably, they could be accused of colluding to do research, which would only benefit them.

NORBERG: I'm not quite sure the way you would state the law there.

BRANDIN: They wanted to get an antitrust exemption from the Justice Department. Their general counsel was a guy who had been Admiral Inman's general counsel at NSA, Dan Schwartz, who I'd worked with on the Public Crypto Study group. I had met him through the

American Council on Education which funded the Public Crypto Group. I believe that the Commerce Department and NSF also funded the PCSG group.

Anyway in '84 MCC contacted me. I was vice president of computer science at SRI, which offered a useful imprimatur. They wanted someone to document all the competitive research projects in the world and the areas that MCC would be working in, so that they could demonstrate there were a host of global competitive programs.

NORBERG: I think it was the one you showed me yesterday.

BRANDIN: The one I showed you yesterday. The contents were proprietary to MCC, but if you can get a release from MCC I'd be happy to get it to you. The report basically said, "Look, the Japanese are doing all this, the Europeans are doing all this. There're all these other competitive programs in the United States, and therefore there's a lot of competition." MCC submitted it to the Justice Department and got their antitrust exemption. I don't know if it was the controlling element, if it helped it or not, but they got their exemption.

NORBERG: Now MCC is no longer in business, is it?

BRANDIN: I don't think so.

NORBERG: I thought they dropped out.

BRANDIN: That could be. They were a consortium and they were in Austin, and this was approximately 1984.

NORBERG: And it was promoted by Bill Norris. He was one of the people who was trying to seek the corporations' gifts in order to keep this thing going.

BRANDIN: I know Harris Electronics or Aero-something was in this. And a lot of the electronics outfits. Texas Instruments probably.

NORBERG: Well, the reason I brought that up is if they're no longer in business that it might not be able to get permission from somebody to use this report.

BRANDIN: You can ask Dan Schwartz, and he's at, I think it's Bryan, Cave, & MacPherson in Washington.

NORBERG: Okay. If they would release those or if he knows what the status could be, then we could add those to what else we have.

BRANDIN: I would not say there's a hell of lot material there that is not in *The Technology War* or not in the J-Tech reports. And you might have the J-Tech reports. They didn't pop out at me when I went scrounging around the house. There were a series of J-Techs and the first one was computer science, which I chaired. We established a notation system for measuring relative performance of: for example, are you even with the competition, is the rate of change favorable or unfavorable, and we used arrows and the magnitude of the arrow, like vectors. That notation ended up being used in all the other J-Tech reports when they studied other topics.

NORBERG: Now wasn't this just after the time when the Congress ordered, I guess is the right word, the National Science Foundation and the National Research Council to look into both short range of technical development of ICs, the various kinds of computing devices and so on. And to do that at five-year intervals and to do it at two year intervals, so you'd get the short run of developments and you'd know where it was going next and then you'd get this sort of longer term. They did it for biological sciences and they did it for several others. I was at the Foundation when they did the first ones. And this sounds like a continuation of that, when NSF essentially made their reports fit the academic community. What was really hot research and how did it look like it was going to develop. It seems to me that Commerce wasn't happy with those for their use so they went about developing their own system.

BRANDIN: Well, Clyde Prestowitz, from Commerce was involved. He's president now of the Economic Strategy Institute in Washington, was basically a trade rep type guy. He negotiated the semi-conductor agreement with the Japanese in the '80s for whoever was Sec Commerce at that time. There were a lot of forces that came together then. First of all, this was almost the end of the Cinderella period of computer science research. So many important research program—AI, telecommunications, the ARPANET were robust then. NSF.net was, I think, in play. The Internet was essentially coming up out of the primeval mud. The Japanese were publishing aggressively on this fifth generation stuff. Europe had just established the Espirit program in Brussels, and the British had just formed the program called ALVEY, which pursued similar research topics.

NORBERG: Yes. There's a book on that.

BRANDIN: In fact, SRI got ALVEY money to establish an SRI AI laboratory at Cambridge University. We were supposed to put up half the money, but we didn't have the funds. I scrounged around and got ALVEY to agree to permit an industrial sponsor to put up half the money. We organized a laboratory called the SRI at Cambridge AI laboratory. and ALVEY put up half the money. We got HP to put up the other half of the money.

NORBERG: How much money are we talking about?

BRANDIN: I think we got \$600,000 or \$800,000 from ALVEY and so HP put up the matching amount. It supported a handful of people. I was very proud of that accomplishment.

But anyway, so there was ALVEY, there was Espirit, there were other Brussels programs besides Espirit. I don't remember their names, but if you look at the aerospace programs there were a lot. Everybody was pouring a lot of money into competitive research programs and they all overlapped with advanced materials and structures and electronics and super conductors. ARPA was concerned. There was all sorts of research bonfires underway and that's what drove the interest in the J-Techs and interest in Mike Harrison's National Academy International Micro Electronics panel. Both got organized about the same time. We had overlapping membership. We visited, I think, every NTT laboratory in Japan, every major computer laboratory in Japan—what's the question again?

NORBERG: That's a good point. I asked you the comparison between what NSF was doing and therefore the Department of Commerce wanting to do more on ???.

BRANDIN: Oh, well they all wanted it. They were all sniffing around. I took ARPA people to some meetings. I took Esprit people to some meetings in Tokyo.

NORBERG: Well, I know there's an ALVEY book because the British were quite distressed that they didn't really succeed in what they had hoped to do.

BRANDIN: Well, no one succeeded from the point of view of the fifth generation objectives.

NORBERG: While on the subject then, what stimulated you and Mike Harrison to write the book, *The Technology War*?

BRANDIN: We had just done the Commerce study and completed the International Micro Electronic study and I had just done the MCC study—we had all this data. The lion's share of the research was done. We figured, "Hey, how about writing a book about international competition in IT?" It was a natural to write. The only competition was the Feigenbaum book, which unfortunately was being published by A-W, and that cost us a year while Addison-Wesley dithered with our book proposal. But we eventually got the book out with Wiley.

NORBERG: Okay. Back to the legal issues. Which case would you like to talk about?

BRANDIN: Well, the MCC case we just talked about. The other two are my case, the Symbolic case, and what's the third one?

NORBERG: IBM versus Award Software.

BRANDIN: Oh, I could talk about that one.

NORBERG: Well, let me perhaps amplify the question that I asked you before about why I have an interest in these things and what could you supply. When I was looking at the electronics industry back in the period from 1910 or so through to about 1942 or 3, I came across a body of material that was written by lawyers when they were looking over the technical accomplishments of company X, company Y, and company Z, and they were consorting with the technical people of the company that they were representing, consorted with the technical people as to what is the state of the art and how do we do this. At least I drew the conclusion—I would not say it was evident to me that that may be too strong—I drew the conclusion that basically the technical people were defining what the state of art is for the attorneys. Very few attorneys were really up to speed on the electronics industry. And that was then used in their cases. I can think of several, two cases for example when somebody like General Electric was trying to keep a small company like Eitel & McCullough out of the game, so they couldn't produce any tubes. They produced tubes that weren't produced by General Electric. So I'm interested in how that process goes. Now you've been deposed any number of times.

BRANDIN: In fact I always thought that the best job that I could get would be a job working for corporate counsel of say Apple or Microsoft—I'd prefer Apple..

NORBERG: You don't like those guys in Seattle, do you?

BRANDIN: It's not that I don't like them, I think that Bill Gates is a really smart guy and he's an absolute master at marketing, but from a technical point of view, in my opinion, Microsoft never made a significant accomplishment and has proliferated a lot of weak software. But his marketing program was wildly successful and he made a lot of money. Anyway, if someone came to me today and said, "Hey, you know, we work for Intel and there's this major litigation coming up in antitrust or patent or infringement or whatever. And how would you like to work for our lawyers as the..." I think I'd grab a job like that in a minute because it's a lot of fun.

All right, well Award Software. Award was a little company in Los Gatos, down here on the peninsula, with offices in Taiwan, and they made and sold a ROM BIOS. The BIOS had to be compatible with PCs. IBM had a BIOS for their PCs. If, for example, you made a thousand machines and you didn't want to pay IBM money, for machines which were clones, or knockoffs, but you needed an IBM compatible bios in your machine, you could buy a license from Award and they'd give you a thousand copies of the code (or one master copy), They'd put a little sticker on the back of the box that said it was an Award BIOS.. Award, of course, would receive a payment for that license.

Well, I was not in that business, but the ROM BIOS is programmed basically in C++. One day I received a phone call from Susan Nycum, who was the former chairman of the Legal Issues in Computing committee of ACM. I'd known Susan since '74 when I visited her committee and suggested that the council members needed an education in their legal obligations and asked her to give a lecture. Susan told me that Award had been sued by IBM: for copyright infringement of the ROM BIOS. Since I had some experience negotiating with IBM she asked me to join the Award board.

I became a director of Award and made a trip to Taiwan to meet the alleged rogues. I'm not really a programmer at that level anymore. I mean the last time I really wrote code was about 1980 at SRI—real code. But I looked at some of the alleged copied code and it looked suspiciously to me like subroutine calls. Subroutine calls are not something you can change. You can change the names of the variables, but calls have got to be in exactly the right order and the sequence, you know, and that stuff has got to be exact. Well, in any event, the guys in Taiwan had said, "We didn't copy." They were quite emphatic Of course, no one ever admits they copied, but to be specific here, I don't know if anyone actually copied any code.

Anyway, I went back to the States, and then Susan and I and the Award CEO went to Armonk or White Plains. As usual, in walked a troupe of IBM lawyers. These were house lawyers; they were not outside counsel. One of them, who I didn't know, knew about me. They had a techie with them. The techie got out his overhead projector and started showing slides and lining up the code side by side. There's Award software code and here's their code and, you know, it's exactly the same. Maybe the variable names were different. I'm saying to myself, "You know, I'm not programming anymore, but it sure suspiciously looks to me like what we're looking at are subroutine calls." And, of course they dissembled. I knew we were never going to find out for sure, but Award decided that it was in their best interest to reach an accommodation with IBM. They ultimately settled.

NORBERG: And that settlement you didn't agree with, did you?

BRANDIN: I didn't disagree with it because I was surprised IBM did not pound Award into the ground with a hammer the way they drove Fujitsu into the ground. I mean the way they beat up Fujitsu, over OS 360 was sad. In the Award case, it was money, but it was not--there was a lot

of money, but the cash flow was such that it wasn't going to destroy Award. So Award went and did it, and then I left the board after that. In litigation, you must have highly qualified technical people because the lawyers don't know squat. They know nothing. And the judges are even worse. Judges are terrible. I believe my judge in the Symbolic case was practically asleep during our trial.

We can talk about my case in particular, but before that I have one more case that I want to talk about. This was an SRI case. When I was at SRI we first began paying royalties to software inventors. Prior to that SRI paid royalties only to patent holders. And in fact, the guy who invented ultrasounds for the carotid artery is a very rich guy with royalties from SRI patents. But anyway, this was circa 1982. One of the guys in the AI center, Dave Kashtan, developed a software system called Eunice. Eunice was an emulator of the Unix shell. With Eunice you didn't need Unix to run applications, which at the time I think cost about \$15,000 a pop for a VAX. And it worked. We distributed it a little over the ARPANET and people loved it. Then we received a phone call one day from Wollongong a software company, "We want to license Eunice. We'll sell it. We'll give you the royalties." That sounded good to us so SRI entered in a contract with Wollongong to allow them to distribute Eunice. The original contract was for Eunice to be run on one single machine at a time. It was a single processor emulator. Money started pouring in and Dave Kashtan, received some very large royalty checks.

NORBERG: Lucky man.

BRANDIN: It was a great deal for Dave, and I was really happy because, finally, it wasn't just the hardware guys who were being rewarded. So it was a precedent for SRI. Bill Miller helped me do that.

Anyway time went by and now we get into the question of the facts, now we get into the litigation. At some point I remember having lunch with the president of Wollongong, and he claims at that lunch that he asked me if it was all right with me if they started licensing Eunice in network applications. I don't remember it. It's possible. But I really don't remember that. I do remember thinking of Eunice as a single processor system.

Eventually Kashtan left SRI, formed a new company called TGV Incorporated. TGV sold Eunice network software. I don't know if Wollongong actually sold network applications or not, but the next thing I knew I got a notice-- I start getting phone calls from lawyers from TGV, Wollongong, and SRI. This was 1990.

NORBERG: How did I get 1968 out of that?

BRANDIN: I don't know. This was 1990. And there was a three-way daisy chain of litigation on this where I was a witness. Perhaps the central witness. When it came to the central question of whether I the guy who had approved the networking, I couldn't remember. I just simply could not remember. I remembered the meeting, the lunch, and I admitted that it was certainly possible, but that in my mind Eunice was a single processor package and it's hard to imagine that we would've just expanded the license on a casual basis, on a casual basis.

NORBERG: Was Wollongong an American company or was it Australian?

BRANDIN: I think it was an-- Well, the name's Australian, but I don't know. I don't know. They were here in Mountain View. Their office was in Mountain View. The president's name was Frank Zehna I liked him.

NORBERG: So now how did the case turn out?

BRANDIN: I don't know. I think they all ended up settling, I think.

NORBERG: But a three-way settlement seems pretty difficult to achieve.

BRANDIN: Yes. Well, SRI, I think no longer-- I mean they were probably getting piddly royalties and I think it got overtaken by events, new software systems, and Unix wasn't as expensive anymore. They were probably moving to different operating systems.

All right, so Symbolic Control—the predatory pricing, illegal tie, antitrust case against IBM. My partner and I ended up being the paralegals because the lawyers had no idea how to deal with the technical issues. We had a trial problem in that the judge had bifurcated the trial and said we were only going to trial on fact of injury—did any act or practice of IBM damage Symbolic. The judge had accepted an IBM argument that due to a Dupont- Buckeye Powder case from circa 1914, antenatal acts, i.e. releasing NC360 to the public domain in 1967, couldn't be used against IBM. Hence, we could not use price in our arguments. So we were forced in the trial to basically mount a technical attack. In other words, the case was driven out of the legal issues and into technical issues, which the judge absolutely hated. We were in a lose/lose situation because the technical issues were very complicated. Even we may not have understood all of them. And IBM had a fleet of people. They set up an office in San Francisco called Litigation West, and there must've been 30 people working there—they must have spent over \$10 million on their defense. We, of course, printed business cards that said we were at Litigation North. I'm sure that drove them crazy.

So what happens is, let's go through the history of the case again. File in November '71; four years of discovery. When we make a document demand, a production demand, they say, "Okay. The documents are in White Plains." So my partner, whose name was Art Pfeiffer, and I went to White Plains. They put us in a room and there were a gazillion documents. We somehow managed to microfilm 30,000 documents. A document that I called a smoking gun was one of them. We didn't know what we were doing, of course, and we were ignoring lots of information. There probably were a million punched cards in there that we didn't even look at. We came home. The lawyers went through the documents. IBM demanded all our documents. We shipped every single document we had. We shipped them everything including boxes of blankets and pots and pans.

Both sides started scheduling depositions, discovery. There was a total of about 80 depositions and they ranged from a couple of hours to four days and they ranged from Armonk-- I don't know if we ever actually got anybody in Armonk; they would've come down to White Plains or something, although we were in Armonk. Armonk, White Plains, Long Island, Long Beach, Boeing Seattle, St. Louis—the depositions were all over the country. They took probably about 60 and we took about 20, but we were at every deposition. I was personally at almost every deposition. The only deposition I missed that I regret was the deposition of Vinnie Learson, because Learson was the one who said, "We could price it [NC360] from now until hell freezes over and nobody would give us a dime for it." That was in his testimony. I think Bart Lee might've made a reference to that in the article you read.

So 1975 rolled around, the trial was scheduled, and right before that there was a motion for summary judgment. IBM said no act or practice of IBM injured their plaintiff. The judge said okay, we're going to bifurcate and go to trial on the issue of injury, or impact or fact of injury; it's called impact. And you can't introduce price, he said. At that point in time we reasoned, "Well, okay. What can we do here?" We're going to go on trial. We're going to show that we built APT 70; we're going to show that IBM maintains NC360; we're going to show that they improve the performance of it; and we're going to finish up some more depositions and see what we can do arguing that this maintenance is antitrust violation.

When we took the deposition with the document that we considered a "smoking gun" there was a guy in the LA Century City NC Development group that we were quite confident had written it. But it was handwritten and we had not paid much attention to the handwritten documents. But a couple of nights before the depositions down there, I went through the microfilm. A handwritten document popped out that talked about how IBM had "bridged the gap" between NC360 and priced program products. It talked about tens of millions of hardware drag which presumably referred to hardware rental revenue based on NC360 usage. So I showed it to the lawyers, and everybody liked it. It was, indeed, a sweet document. In the deposition protocols, the deals we had with IBM, the lawyers would exchange documents they intended to use the night before the depositions.

Now meanwhile, my partner and I are the ones who are looking at all the documents and writing all the technical briefs and technical arguments and analyzing what IBM does, like when they went into APT and replaced some code with a new code, which they would label "cleanup." And of course the new code ran a hundred times faster than the old code, which was one of our problems. So we gave that document to IBM, to the IBM lawyer, and his name I think was Russ Johnson. For some reason he didn't look at the document. The next morning we were in the middle of the deposition, and our lead lawyer, handed a copy of the document to the IBM lawyer. Our lawyer began trying to authenticate the document, saying, "Do you put little circles around the numbers in the upper right hand corner of the documents that you write," and so forth. About 15 seconds later, the IBM lawyer said, "Off the record. Off the record." He had a spontaneous nosebleed. I elbowed my lawyer saying, "Get it on the record. Get it on the record he's bleeding on the documents." Well, sadly, it didn't make it onto the record.

In any event, the guy who we believe wrote that document refused to admit it was his document. Flat out refused to admit it. He just, you know, weaseled out of it. Dissembled, wouldn't admit it. So anyway we went through all these documents, we went through all these depositions, and we finally got to trial. We presented our case in chief, which is that IBM was predatory pricing at zero, that they were bundled, that it was an illegal tie, that they were doing maintenance, that if you read any of the antitrust literature, user groups are a classic tool of a monopolist—to maintain a monopoly. Naturally, IBM had a users' group, there was no cost for any of those services. They were giving these guys information. They were fixing errors for these guys. The priced program products weren't ready for five more years, although every other program product at IBM was priced; this was the only one they didn't price.

At the end of the trial IBM said, "Your Honor, we move for a dismissal. They haven't proven impact." And the judge said, "I'll take it under advisement," Six months later he said he agreed despite all the technical work that had been done by us, all the analysis. I wrote factual briefs that had to be 300 pages long, and I literally took stuff out of IBM APT and listed it in the briefs, to show what their comments were, and report what it did and how it changed. And, it was free software. The lawyers could never have done that. But we were screwed the moment

the judge said he accepted Buckeye Powder; we were screwed in terms of any chance to win that lawsuit at that trial.

NORBERG: Now Buckeye Powder was challenging DuPont at the time, were they not?

BRANDIN: Yes.

NORBERG: Back in 1912 or something. And was the nature of that case also technical or was it based on price?

BRANDIN: I don't know, but it involved antenatal acts.

NORBERG: You said something about it in that article you had me read last night.

BRANDIN: Yes, but it's legalese.

NORBERG: Yes, that's why I didn't really catch it all.

BRANDIN: Anyway, at that point in time, in 1976, we tried to get a new trial. As I said, I think earlier, we were getting additional evidence. It's not like the APT users were cooperating with us. They had good things going with IBM and they weren't all that friendly, but we were getting information about beta tests where our product was being compared with IBM. The intel said they would have a problem with IBM APT on a run and they'd call in IBM and IBM would fix it. This is something you're not supposed to do in a beta test because we weren't getting the opportunity to improve our performance in the comparison. We tried to introduce new evidence. The judge wouldn't buy it.

In '77 we appealed to the Ninth Circuit and they took two years but then reversed saying that Buckeye did not apply—that you could be liable for antenatal acts if they were illegal, sent it back to the same judge and then it just kind of trickled on. In the final negotiations—the terms of the settlement were confidential, but it included business with IBM. And really, once the envelope of the agreement was established, the lawyers went away and we did all the negotiating. Once we knew the boundaries, we did everything in the middle, all the detail.

NORBERG: Now it's my understanding that when a legal case is settled rather than brought to judgment, that there's a clause in there that gives confidentiality to both the settlement and to the materials and then they just disappear. So if I went down to the archives of the Ninth Circuit I couldn't get that case. Is that true?

BRANDIN: Well, you can from what I believe-- First of all that's optional; you know, it's up to the parties to agree to that. I believe that all the pleadings and trial exhibits, because we went to trial and that's a public record, I believe that you can get all the exhibits from the original trial. You may not be able to get all the other documents that were produced in discovery, but there were 3,000 trial exhibits. So there's a lot of fodder out there.

NORBERG: I did a lot of work on the Honeywell-Sperry case where the Honeywell people were trying to get the judge to say that the patent on the UNIVAC series was invalid because it had been announced more than a year before the patent was first granted. I don't remember how

many exhibits there were, but there were at least 3,000, and I went through them all because we had the Honeywell side of it and our colleagues out on the east coast has the Sperry side of it, so you can examine both sides of the exhibits. In fact what we did is we had all the stuff microfilmed, and that microfilm is available to anybody—online even. But there were thousands. I can't say there were 10,000, but just looking at the exhibit numbers and often it went A, B, C, D in five different versions from five different places to try to demonstrate that in fact this was common knowledge, and that Sperry was not entitled to have a patent on this machine.

NORBERG: Has the nature of these legal cases changed much over the years?

BRANDIN: I don't think so. You know, in the Microsoft-Apple case-- I knew the General Counsel of Apple, Al Eisenstadt. He is retired now. And the issues there are very similar. They're technical issues of what's a user interface, how does it work, the point and click, the dragging, all that. The only part is the legal stuff here, is that copyrightable? But what is look and feel? Those are all technical issues, and you've got to have technical people working on it or you'll go crazy. I don't understand how Apple lost that case.

NORBERG: Maybe it's like the IBM cases where the other companies didn't win because IBM had this phalanx of attorneys.

BRANDIN: Oh, I think the main reason is I think the judges are idiots. I think that's the main reason. I did meet the guy in New York who prosecuted Microsoft in the Justice Department case and I thought that that was a weak case.

Session 3, Tape 1, Side B

NORBERG: Is there anything we didn't cover that you think should have been?

BRANDIN: There's probably a thousand things that I could remember if I had reminders and pointers. I mean those council meetings lasted eight to twelve hours, so there had to be something. If we had council agendas maybe that would remind me, too, but I didn't keep any of the old ACM records.

NORBERG: I'll have to get out the McCracken papers for '74 to '82.

BRANDIN: What'd I really like to see are the agendas from '78 to '86. Adele might have some of them, and I suspect Peter, I don't know, but I suspect Peter saves everything. He seems like the type to save everything.

NORBERG: What was a typical meeting like among the Council? You described some of your own...

BRANDIN: Do you mean in the raucous days or the non-raucous days? [Chuckles]

NORBERG: Well, I mean you've brought peace to it you said when I asked you...

BRANDIN: I brought peace and order.

NORBERG: What did you have to be peaceable about and what had to be ordered?

BRANDIN: Well, I had to enforce the agenda. I had to enforce the starting time, the closing time, and the breaks. And I did. I did not dally. I said 10-minute break. At the end of the 10 minutes the meeting reconvened whether everybody was there or not. If you weren't at the table you were not allowed to talk.

NORBERG: Now why is that?

BRANDIN: Because the only purpose of the meeting is for councilors to debate, and if you were not at the table, you're not in a position to debate.

NORBERG: Okay. The meetings were open then.

BRANDIN: Well, sometimes the meetings were closed when we would throw out the audience. And usually the audience consisted of ACM staffers and an occasional volunteer.

NORBERG: So enough seats had to be available around the table to accommodate all of the people.

BRANDIN: Oh, well the meetings were in a room with a large square table. There were, I'd say, 25 members of council plus 3 or so staff, and we probably had an extra two seats for guests. If someone wanted to speak, they had to come to the table—I mean they had to get the attention of the chair. In general, I would let any people that asked to speak. I maintained a queue. I'd say all right we're going to debate the budget for the external activities board. Who wants to speak to this issue, and then I'd write everybody's name down who wished to speak and I'd go down the list. If someone raised their hand again, I'd say, "Sorry, you already spoke. Wait until I get to the end of the list; and we'll see if there's time left," I would allocate time to each topic. When someone like Paul would say the same thing that 14 other people had said, I would say, "Thank you very much, Paul. But let's not forget we don't make me-too speeches."

NORBERG: Well what was it like in the raucous days then?

BRANDIN: Oh, in raucous days you could get lost in a subject and people would yell and get up and walk away, and people would stand in the back in the corner shouting. The chair would go, "Hrumph, hrumph, hrumph," and pound on the gavel. When he'd tell us to take a 10 minute break—we'd all leave the room, and 30 minutes later some of them would trickle back. It was chaos. And they couldn't agree on fundamental issues. I mean it wasn't that bad all the time, but then there was always a big deal that snuck in that no one had anticipated. There was always something.

You know, someone would accuse someone of something. In my favorite case.—I don't remember if it happened at Council; it might've been an executive committee meeting where Jan Matser, one of the chapter people, said, "They've been manipulating Kathleen Wagner's

petitions, and you're the secretary and you're responsible." It was that kind of stuff. It'd come out of nowhere, you know, completely unanticipated. And then there'd always be some supporters of the goofy issue. Then, there were other little vignettes. We had a member on Council from Germany. He was the European Regional Representative. His name was Horst Hünke. George Dodd, the secretary could never pronounce Hünke properly. Carl Hammer, a council member of German descent, recorded on a tape recorder, "Horst Hünke," and, when they called the role, George Dodd would play the tape recording.

NORBERG: That's spelled H-U-umlaut-n-K-E.

BRANDIN: Yes. But there was a lot of good-natured spoofing and that kind of stuff, too, but there were undercurrents of very bad feelings in some cases. Sadly, there wasn't enough money to go around, so competition for resources made for nastiness.

NORBERG: Yes, I've witnessed that at the National Science Foundation when the budget was being considered and the blood-letting was terrible between divisions.

BRANDIN: At SRI when I was competing for capital equipment I was always very uncomfortable because I always felt we were getting the short end because the boss, my boss was more interested in capital equipment for the electronics, EW, electronic warfare guys and radar guys, and the systems guys. We always wanted more computational equipment and more laboratory stuff. So it could get nasty.

NORBERG: When you traveled for SRI to Europe say, what sort of things would you be doing? Would you be demonstrating something newly developed or would you be getting information from them?

BRANDIN: Well, there were marketing trips and there were management trips. The marketing trips, we would run around talking to potential new customers. I remember I made one trip to Asia with all the vice presidents of SRI Engineering. We went to Korea, Taiwan, Japan, and met with all the military research organizations. We were doing a lot of electronic warfare work. So we would meet with the Chungshan Institute in Taiwan, which was the equivalent of...or the ROC version, of a combination of Mitre Labs and Air Force Weapons Lab, something like that. We were in Korea talking to the guys who were putting jammers on Korean fighters. In Germany, SRI had a project to build electronic warfare equipment. Not only design the systems, because we had a lot of experience forming threat issues, but build them too. I worked on a project called over-the-horizon radar. I was the technical consultant for the project on computational issues. I worked on some projects that I can't talk about, but were computationally intense. When I had the computer science division, I had all my people based in the States, but we had projects for the Japanese and the Germans. But when I had the consulting division I had a group based in the U.K., and they were doing work in the U.K. as well as Scandinavia and on the Continent. So I would either go over there for meetings or to meet with the clients or in some cases work on the projects. I did a lot of the work on the work that we did for German Minister of Research and Technology because that project was to help them determine where they should invest CS research money that would support German Industry. I think I told you about Siemens and Nixdorf. I met Herr Nixdorf and I got a lecture from Herr

Nixdorf, too. “Don’t ever give money to Latin America. They won’t pay you back. They can’t pay you back and they don’t want to pay you back.” This was quite gratuitous advice.

NORBERG: That suggests to me that SRI was no longer a not-for-profit organization.

BRANDIN: No. Oh, that’s a very, very interesting question. It had nothing to do with ACM, but it’s a very interesting question. SRI had three groups that I mentioned earlier, or primarily three groups. There was the engineering group, which was basically a defense funded, mostly research organization. Physical sciences group, which was basically defense or other government agency funded. We had our NSF money too, but—by the way, most of the NSF grants didn’t come directly to SRI. It usually went through universities and then we kind of subcontracted. We got grants, but it wasn’t always direct... And then there was the consulting group whose divisions had an entirely different fee structure than SRI in general. I don’t know if there was a higher tax assessed on the profits made by the consulting divisions. There weren’t that many profits at the institute bottom line. But again no one really knew what the profit was of any single element, because the manipulation of the overhead rate and the G&A could make any unit profitable in SRI at any point in time. However, SRI eventually did spin off that entire consulting group as a separate profit making company.

NORBERG: Called?

BRANDIN: I don’t know. It’s since been sold off. I liked some of the people over there, but I felt that they were taking advantage of the engineering group. For example, my AI center was the largest non-academic research AI group in the world. It had a great reputation. Whether that was justified is a different issue, but it had a great reputation. The consultant group always wanted us to sprinkle holy AI credentials on their commercial proposals. Then they would get a job, because of the credentials. Of course very little of the money would trickle across the barrier. So instead of making my troops do that the marketing, I basically did that. I was always going out on a consulting call to sell somebody because we were so good in these other fields. And SRI, if you compare them to AD Little, for example, SRI had spectacular laboratory capability in electronics that the competitors didn’t have. In the soft topics, the paper studies, you could say they were equivalent.

So my attitude with the consulting guys was they should sell consulting in the areas where we’re specialists, but SRI was out selling work in food economics, agricultural economics. There were few at SRI who were specialists in botany for example, and stuff like that. So the consulting group always had problems because they didn’t have critical mass in any single area, and they were trying to compete with everybody, with all the consulting companies, plus they were expensive. They were much more expensive than the engineering group. I think if you went through a cost structure where say \$100,000 a year person sold to an Air Force project who would probably bill out at about (this is in the ‘80s) \$250K.

NORBERG: \$250,000?

BRANDIN: Yes. And that would count 100% overhead, some G&A, and fee, which would be CPFF or fixed fee or whatever, incentive fee. But the multiplier would be around two and a half. The multiplier in commercial work would’ve been more like five.

NORBERG: Now to get back to the original question, that suggests that they were profit making. Where'd the money go?

BRANDIN: Well, they were making profit if they were sold 100% of the time.

NORBERG: But your people weren't sitting on their hands when there was no task that they were directed at.

BRANDIN: As you went from an individual person to a department to a laboratory to a division to a group, the average sold time, time on project, decreased. So if an individual person was sold let's say 80% of the time, his department might only be sold less, and the organizations higher in the chain would be sold even less. Because people were writing proposals that, you know, they're not being—and there's no customer paying to write proposals or they're doing house research, or they just really had nothing to do. If the divisions could operate at a reasonable sold time level they did well. They could make money even with the high overhead rate at SRI but it depended on sold time.

NORBERG: Well, it certainly sounds that way, 100%. Although I remember having conversations with one of the deans from the University of California, San Diego, about the difference between the overhead rates in businesses and the grant structure from places like NSF and so on. He was a molecular biologist. He was arguing that the 120% overhead that some places were getting (he mentioned a few companies, but I don't remember them now) was fully justified. And I came back with, "Well, wouldn't that be fully justified in the University system," because we were getting 5% at that point, in the history of science anyway. And the overhead was being paid by the state as opposed to being paid on the grant structure. But he taught me a lot in that conversation.

BRANDIN: Here's is a multiplier. If you start out with a salary of X and you've got 30% benefits, and then you add 100% overhead, you're at 2.6 multiplier. Then you add G&A say 30%.

NORBERG: What is G&A?

BRANDIN: Well, "general and administrative costs." it's for the president's office, security, and the limos and the administrative expense of the human resource—costs that can't be allocated to individual units. It's different from overhead, sometimes they're combined, but SRI had two categories. So then you get the 2.9 and then you add fee on top of that. So let's say it's 5%, so I round it down and you get a multiplier of three on the salary. That's what you've got to bill out.

NORBERG: Yes, because that's not going to disappear if they're not working.

BRANDIN: And these go up if the staff isn't sold 100% of the time.

NORBERG: The overhead and the G&A go up if the person isn't sold 100% of the time.

BRANDIN: Yes, the following year they had to increase because overhead ultimately would reflect unsold time, salaries for unsold time. Or, the organization might decide to support a group at a lower sold time as an investment.

NORBERG: And in the case of the university grants, that wouldn't be necessary because they would be spending some of their time teaching, which does pay out through the instructional budget.

BRANDIN: Well, you have a different overhead structure. And then this would be for let's say a cost plus fixed fee kind of a structure, with an incentive fee you get something else. And in the commercial world you don't even do this; you just come up with a big number.

NORBERG: We tried that. Thank you very much, David. You were very generous with your time.

END OF THE INTERVIEW