An Interview with

BERNARD A. GALLER

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Conducted by Enid H. Galler

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Abstract

In this wide-ranging interview, Galler describes the development of computer science at the University of Michigan from the 1950s through the 1980s and discusses his own work in computer science. Prominent subjects in Galler's description of his work at Michigan include: his arrival and classes with John Carr, research use of International Business Machines (IBM) and later Amdahl mainframe computers, the establishment of the Statistical Laboratory in the Mathematics Dept., the origin of the computer science curriculum and the Computer Science Dept. in the 1950s, interactions with Massachusetts Institute of Technology and IBM about timesharing in the 1960s, the development of the Michigan Algorithm Decoder, and the founding of the MERIT network.

Galler also discusses Michigan's relationship with ARPANET, CSNET, and BITNET. He describes the atmosphere on campus in the 1960s and early 1970s and his various administrative roles at the university. Galler discusses his involvement with the Association for Computing Machinery, the American Federation of Information Processing Societies, the founding of the Charles Babbage Institute, and his work with the Annals of the History of Computing. He describes his consultative work with Israel and his consulting practice in general, his work as an expert witness, and his interaction with the Patent Office on issues surrounding the patenting of software and his role in the establishment of the Software Patent Institute.

BERNARD A. GALLER INTERVIEW

DATE: 8 August 1991 INTERVIEWER: Enid H. Galler

LOCATION: Sutton's Bay, MI

ENID GALLER: Professor Bernard A. Galler is talking about his long career at The University of Michigan. We are at Sutton's Bay, Michigan. It is August 8, 1991. My name is Enid Galler.

At what point in your life did you decide to become a mathematician?

BERNARD GALLER: I believe it started when I was 10 years old, and with watching my father help my sister with her algebra in high school, and I got interested in mathematics and was pretty good at it. The teachers in high school encouraged me, and it just went on from there. I think I thought from the beginning that I would be a teacher. And they encouraged that, also.

E. GALLER: Why did you think that you would be a teacher - from observing how your father tutored your sister?

B. GALLER: Yes, and I think that any time I mentioned it, he strongly encouraged it, so that it simply built on itself, and it was what I wanted to do. The teachers in high school would let me tutor others, occasionally take over the class and teach it, and I was good at it, so it was a natural thing to do. When I got to the University of Chicago, I had already had a fair amount of college math, or at least advanced high school math, so I went into the divisional courses, even though I was in the College, and I remember that my first teacher was Charles Brumfiel, who I learned later had just become a teaching assistant - it was his first teaching experience - and later, we were colleagues at The University of Michigan. He died a short time ago. But we were always friends, and I really appreciated his encouragement back then in mathematics. In fact, he wrote a letter to my parents once, saying, "Bernard could be a very good mathematician if he were just a little more careful and didn't make all those mistakes." And that made a big impression on me.

In any case, I got a bachelor's degree in mathematics, Bachelor of Science, in 1947 at the University of Chicago and went on to UCLA for a master's degree. While there, I wrote a master's thesis with Arens on the Theorem of Pontrjagen Without the Second Axiom of Countability; that was in topological algebra, and it was very interesting. I actually thought I had something new there. Then I went back to the University of Chicago. Fortunately, they didn't ask me to take their qualifying exam, because I already had a master's degree, and so I went on and got a Ph.D. in mathematics there. And that really began my professional career. After getting a job at The University of Michigan, based on a 10-minute talk at the spring meeting of the Math Society - in those days it wasn't expected that you would go to the university that was recruiting you to give a lecture and meet everybody. I was made an offer by The University of Michigan based on a 10-minute talk at the big meeting at Chicago in the spring. So when we got to Ann Arbor, we really didn't know anything about The University of Michigan, but I came into the Mathematics Department. I was expected to teach Logic because of my thesis work, and I taught Advanced Calculus that first year. That's when I got into computing, because I also audited the course that John Carr was giving that fall, and we got to be friends. But that's another story.

E. GALLER: Why don't you right now tell about that course that you audited from John Carr, and what that led to.

B. GALLER: All right. Actually, it starts back a little bit earlier, because in the course of my work with Marshall Stone and Paul Halmos on my thesis, I had gotten interested in mathematical logic, and the trail that I followed led me to a book on computing, the book called *High Speed Computing Devices*, because I had come across an article in *Mathematical Tables and Other Aids to Computation* by a group at Michigan that I didn't know, but of course, later did get to know well - Arthur Burks and others - which described a machine they had built with Burroughs to evaluate well-formed formulas in logic. A footnote in that article pointed me to the *High Speed Computing Devices* book and got me interested in computers. When I got to Michigan, I happened to notice in the time schedule a course by John Carr, and I sat in on it. And that's when we became friends. He introduced me to the people at General Motors, but after he introduced me to Paul Dwyer. John recommended that the best way to learn about computers was to actually write some programs and do some computing, and he suggested that I work with Paul

Dwyer on some linear programming stuff. We did that, first on the MIDAC, which was my first computer, out at Willow Run Labs, then when the IBM 650 came in 1956, Dwyer and I used that, but we outgrew it. Our programs and the size of the arrays we wanted to process were just too large for the 650, so John took me to General Motors and introduced me to the people there, and they let me use their [IBM] 701 computer, and then the 704 when it arrived. That led to a whole series of developments in computing at the University, which I suppose we should get to somewhat later. But halfway through the first semester of that course, I got a call from the chairman of the department, Hildebrandt, who said, "John says you ought to teach that course next term." And I said, "Fine, that's a good way to learn it." And so I did, and that was the course which was largely numerical analysis, but also talked about programming languages. It was everything that was known about computing at the time, really. Because John had been at the MIT summer courses and had brought back a lot of notes and was teaching from that, and I was teaching from my notes from John's notes, and so I learned some numerical analysis, actually published a paper with a student in the course that year. And that course evolved eventually into Computer Science 473, which has been renumbered a number of times and actually broken up into a number of different courses as each part of them expanded. But I think its direct descendant is probably the 280, 380, 381 sequence now.

E. GALLER: Tell more about Professor Dwyer.

B. GALLER: Well, Paul Dwyer was a very eminent mathematician and statistician. He and Cecil Craig had set up the Statistical Laboratory which was, in fact, where the IBM 650 computer came and was the beginning of computing on the campus itself. Paul Dwyer had worked with the Air Force on linear programming techniques. The Air Force was interested in optimal ways of assigning different people to Air Force crews. They wanted to look at the various psychological attributes that the candidates had and match them according to certain objectives so that they made a coherent crew, and they were trying to use linear programming for that. So one particular kind of linear programming problem was called the assignment problem, and was directly applicable to that kind of problem. Paul Dwyer was interested in doing that, but he needed someone to help him with his computing, and so that's where I came in. I wrote the programs; Dwyer worked out the methods and we published several articles together. Somehow, it was a sort of a special-purpose technique and wasn't anywhere near as general as Danzig's linear programming iteration

method; Danzig's method really caught on and ours never quite did. But it was very interesting work, and surely gave me a lot of experience in programming, which was what I was interested in at the time. We did transfer our programs to the IBM 704, and the General Motors people were very interested in them and did use them somewhat for a while.

E. GALLER: Tell more about Professor John Carr.

B. GALLER: John was a very dynamic person, very dedicated to computing. Unfortunately, he had a way of sometimes going over people's heads in trying to accomplish his goals. He would send a letter to the president of the University, with a copy to the chairman of the department or whoever, which didn't always make friends. I like him, personally. I learned a great deal from him. But when it was time to set up the Computing Center in 1959, I think John hoped that he would be the director of it, and he should have been, except that I think people thought he was sort of abrasive. He used to stand up in the Math Department meetings sometimes and tell the faculty that they would soon be out of a job because the computer was going to prove all their theorems. That doesn't make friends, either. So when the Computing Center was established and Bob Bartels was made the first director, that's when John left. He went to the University of Pennsylvania. Well, he went to North Carolina, I think, first, but he is now at the University of Pennsylvania.

E. GALLER: Tell about your experiences as a member of the Math Department. How did the Math Department view your interest in computing?

B. GALLER: Mathematicians, you would think sometimes, would be the most avid computer people, but they're not. They're more theoretical. They don't really like to think in terms of approximate solutions; they want the analytical solution to a problem, the theoretical solution. So in fact, mathematicians are always among the last to begin to use the computer on any campus, unless they are trying to solve problems which are applied, and need solutions whether they are approximate or not. So the mathematicians very grudgingly got into the computer field. I remember once it took several votes over a period of a couple of years, each time being voted down, to even recommend that

mathematics students take computing courses. And, in fact, they made the distinction that honors students wouldn't be recommended to take computing courses, only other students who might end up in computing of some sort, but not honors mathematics.

E. GALLER: Do you remember what years that was?

B. GALLER: Well, I think it was in the early '70s, still, which was fairly late in the life of computing, but now, of course, there is a fair amount of computing in the Math Department, and they built up some numerical analysis staff, and they have computers there. I still think that most of the theoretical mathematicians never touch a computer. They do like the electronic mail now, some of them, so that's catching on. But they very seldom use it for mathematics. Now, I was not bothered by them, by anybody. I was in the Math Department for a long time. I came in 1955. Hildebrandt, who brought me in, retired maybe a couple of years after that, and George Hay took over. George was really very applied in many ways, and he did encourage my work in computing. He and Bob Bartels, who was the Computing Center director, encouraged me and protected me in many ways. I really have no idea, but somehow my career was moved along very rapidly. Promotions came well, and I had no particular problem. On the other hand, around the early '70s, I began to feel less and less a member of the department. I remember my salary increases began to get less, and I felt that computing was very peripheral. Hay was no longer chairman, and by that time the Computer and Communication Sciences Department was well established. I had become associated with them since about 1964, and after a while - I don't remember when - I had a joint appointment, and around 1973, I saw that the handwriting was on the wall. Mathematics really wasn't interested in what I was doing, and I switched over entirely into the Computer and Communication Sciences Department. I had many friends in the Math Department, I have always been very friendly with everybody there. It's never been a negative thing, I simply wasn't a mathematician anymore in their eyes, and it was time for me to move over. Interestingly enough, I am now a Category Editor for Computing Reviews, responsible for the review of mathematics in computing. So in that sense, I am still a little bit of a mathematician.

E. GALLER: Could you tell now about the history of the Computing Department at Michigan?

B. GALLER: Well, first there was, of course, no department. The only computing on the campus was John Carr's course - I mean the computing teaching on the campus. John Carr's course, and then there was some activity in the Business School. Carl Pollmar taught some things there. Then, the main development was a group that got together in the late 1950s, I think around 1956 or 1957. They called themselves something like Language Models and something or other. It was a degree program approved by Rackham, and the first Ph.D., in fact, was John Holland. The people who formed that group were Gordon Peterson from Linguistics and Art Burks from Philosophy, actually, to start with. But they drew in other people like Bob Thrall of Mathematics, Gunner Hok in Electrical Engineering. I think Swain from Pharmacology was in there pretty early, and some people from Psychology. And it was a very interesting interdisciplinary group. They also started, a little bit later, Arthur Burks with John Holland started a research group called The Logic of Computers, and I'm not even sure where that was housed originally. It ended up in our department. Now, that group met and offered some courses over several years. As I said, John Holland got his Ph.D., and I was invited in the early 1960s to join the group, and in 1965 we actually formed a department. It was interesting that we wanted to be a department in Rackham, because we had representatives from LS&A, Engineering, and Medicine. The Rackham people said, "You can't have a department in Rackham, you have to choose a college." After some soul-searching, we picked LS&A. And so we became a department in LS&A, with Arthur Burks the first chairman, and called ourselves Communication Sciences. We didn't want to call it Computer Science because it wasn't by any means a standard computer science department. It had a great deal of interdisciplinary stuff. We had a course on biological systems as information processors, we had a course on behavioral systems as information processors, along with the programming and other things that one would normally find in computer science, except that the numerical methods and the numerical analysis was in the Mathematics Department, and we decided that we would not try to include that. So, in 1965 we became a department in LS&A, and the name was changed about a year or two later because we discovered that, for example, there was a department at Michigan State called Communication Sciences, which was really a speech department, and there was a lot of confusion. We found that we were forever explaining who we were. We changed the name to CCS, Computer and Communication Sciences. And that's what it stayed until 1984. It was a very good department, but very non-standard as far as computer science went because of our interdisciplinary nature, and so we never really were rated in any of the computer science rankings. But we felt

fully justified in what we were doing because we did turn out some very excellent Ph.D.'s who ended up in research labs all over the country. Now, in 1984, just to finish the story on that, we merged that department into Engineering. During those intervening years, there was a lot of activity in the Business School, which still goes on, the CIS department, and there was a lot of activity ...

E. GALLER: What does that stand for, CIS?

B. GALLER: I think Computer and Information Sciences. And in the Engineering College, there was a department called Electrical and Computer Engineering, ECE. There was a graduate program called CICE, Computer Information and Control Engineering, which was again, a Rackham program, interdisciplinary, which involved people from Aerospace because of their analog computing, and a bunch of other departments, and in LS&A we had CCS, and we offered graduate degrees, so there were three centers on the campus. We had many discussions over the intervening years about some day we ought to merge because the outside world, we knew, saw us as very fractionated, and sometimes people would know about the activity in one place and not in the other, and then we looked very small. So we finally decided in 1983, and it became a fact in 1984, that we should merge the LS&A and Engineering departments. LS&A really didn't have enough money for the equipment we needed, and they encouraged us to make this merger, although they wanted very much to hold on to the undergraduate degree program that we offered in Computer Science. And so when we made the merger finally, LS&A in fact continued to pick up 25% of the salaries of all of those of us who moved over into Engineering. The CICE program was phased out, we formed a new department called Electrical Engineering and Computer Science with three divisions, one of which is Computer Science and Engineering, CSE. All of us who came over from LS&A went into the CSE division, together with those of the ECE department who chose to come into that division. It turned out we were 12 and 12, and we've been getting along very well. The interdisciplinary aspect, represented primarily by Burks and Holland and Steve Kaplan from Psychology, was downgraded once we got into Engineering. They wanted us to be more of a standard computer science curriculum and department, largely because they are so controlled by ABET, which is - I don't know what it stands for, but it is a standardization and accreditation board in engineering. It probably stands for Accreditation Board in Engineering and Technology. They have very rigid guidelines as to what departments ought to be offering

and so on, and all of this interdisciplinary stuff didn't fit into that. So that was downgraded, and we added more and more of what you might consider standard computer science, to the point where John Holland has in fact now moved over back entirely into Psychology. His undergraduate degree was in psychology, and he no longer felt welcome in EECS, so he has now moved back. So that's sort of the history of the department. I think the merger was quite successful. I was on the six-person, three and three, that negotiated the merger, and I think we did a good job in organizing it, and it's been quite successful. I am now a professor of Electrical Engineering and Computer Science, even though I never had any engineering background, but it's working out okay.

E. GALLER: You mentioned that the department turned out some wonderful doctoral students. Why don't you talk a little bit about the students that you were chairman of for their dissertation committee?

B. GALLER: Well, I've been chairman or co-chairman - two or three were co-chairmen - of 15 students. The first one, Glen Graves, did a very ingenious linear programming technique, but he disappeared after that. I've never had any contact with him since. That was back in the late '50s, I guess. I just have no idea. The next one was Bob Rosin, who has been a long-time colleague and friend, and we are still very much involved with each other. We are both on the Steering Committee right now of the History of Programming Languages second conference, and Bob has been just a tremendous friend. We have also collaborated several times in leading discussions which eventually got transcribed and published in the *Annals of the History of Computing* on historical topics.

Another one was Bob Korfhage. Again, I am still very involved with Bob Korfhage. He is now chairman of the Library and Information Sciences Department at the University of Pittsburgh. He just was very helpful to me in the Software Patent Institute meeting that we'll probably talk about later. He wrote one of the position papers, from which I learned about some imaginative techniques he is now doing in information retrieval, and he is very active, and it's great.

Another was Frank Little, who did a nice thesis in the Mathematics Department, actually. He ended up after that at Texas Instruments, and is now a consultant in Phoenix, and whenever he gets to Ann Arbor, we do see each other.

It's always a big joke, of course. His last name is Little, and he seems to be seven feet tall, but he's probably about 6'2" or 6'3".

Tad Pinkerton was the first one - he was in the Mathematics Department, was the first one to do a thesis at Michigan on the computer, and he was instrumental in getting Rackham to accommodate to some of the idiosyncrasies of theses that are produced on the computer. He ended up at Wisconsin as what some people call the czar of computing at Wisconsin, a very influential position. We do see him occasionally, wonderful person.

Ron Srodawa did a nice thesis on languages, probably the thickest thesis I've ever seen. It is probably 500 pages long. He is now teaching at Oakland University, and I do have some contact with him on occasion.

Al Springer did a nice little thesis, too, but ended up at IBM and seems to have been buried in IBM somewhere. I haven't had any contact with him except soon after he went to IBM, he was doing a comparison of MTS and Unix for IBM, and we had some contact then.

Carl Landwehr is with the Office of Naval Research in Washington. I see him and his wife Jurata occasionally, and we have always been good friends; wonderful guy.

Steve Weiss is still in Ann Arbor, and he did an operating systems thesis. He is heading up one of the groups at Comshare, and on occasion I see him and talk to him and I send him students that he hires. And we have a very good relationship.

Jim Hamilton did a thesis on disk cacheing and so on. He went to work for Apollo. As far as I know, he's still there, very influential in their operating system work. I haven't had much contact with him for some time now.

Dave Beard. I think I was a co-chairman for Dave. He went down to Ohio State, I believe, afterwards. I've heard from him once or twice, but not very much.

Marty Stytz, with whom I was a co-chairman with Gideon Frieder, did a nice study of imaging techniques for medical images and scanner images, and was in the Air Force and has just been promoted to Colonel, or I guess, Lieutenant Colonel, and was in Dayton teaching, and should do very well.

Mark Segal did a nice thesis on dynamic updating of computer modules and is working for Bellcore. We are trying to collaborate on a paper to apply his thesis work to dynamic updating of human organizations; it's not going too rapidly, but I think it will be an interesting paper to write. He comes to Ann Arbor recruiting for Bellcore very often, and we see each other a lot.

John Sayler lives in Ann Arbor. He is a consultant, does a lot of work with the Japanese, was on our faculty for a long time as an adjunct professor teaching software engineering for us. He is a very good friend, and he also helped out in the Software Patent Institute meeting by writing a position paper on the kind of education and training that the Patent Office people ought to have, which, I think, got a strong endorsement from the group that was there.

The last one on my list here is Martin Piszczalski, with whom I did a lot of work on the analysis of musical sound, and we published a number of papers together. He is living in Ann Arbor, now working for the Yankee Group as a consultant on Computer-Integrated Manufacturing, but in fact, he is very seriously still involved with the music business, and we're in very good touch with each other. I'm very pleased to have so much contact still with so many of them. They're all good guys.

E. GALLER: Tell about the doctoral students who are working on their Ph.D.s right now with you as chairman.

B. GALLER: Yes. Tim Howes is working on an interesting topic involved with name directory service and organization of database systems connected with the X.500 standard, and Avi Rubin doesn't have a topic yet, although we're thinking about one in the IVHS area, Intelligent Vehicle Highway Systems. We'll describe that a little bit later. So I've got these two students working now with me, and enjoying it very much.

E. GALLER: Could you now tell about the history of computing at The University of Michigan, starting from way back?

B. GALLER: Actually, The University of Michigan was into computing and computers very early. In 1953, which was, of course, not as early as the University of Pennsylvania, but in 1953, a computer was built called the MIDSAC, which was apparently a very fast, very good computer, but for some reason it was dismantled a couple of years later so that they could build another computer called the MIDAC. The MIDAC was the first computer that I had contact with. It was out at Willow Run. The computer was built with an Air Force contract because of the work on the BOMARC missile that was being developed at that time. BOMARC was, of course, Boeing Michigan - who knows what - ARC. The computer was in a classified area at Willow Run, so when John Carr wanted to have his students run programs on it, they couldn't come out to the computer. He used to carry the programs back and forth. He would take them out there, have them punched on paper tape, run them and bring the results back. After a while, the actual console area was declassified, so I could go out there and work there. That's where I met Bob Graham, because he was an operator on the MIDAC. The interesting thing was that they were still working on the computer, so the definition of add might change during the day, and I'd come in at night and try to run my program and something wouldn't work because addition was no longer the same as it was the day before. But that's where I first developed the linear programming algorithms with Paul Dwyer, and we went on from there.

In 1956 - I came to Michigan in 1955, and the MIDAC was essentially done, but they were still working on it. In 1956, the IBM 650 computer came to the University. This time it was brought in as a publicly available computer. It was established as a facility in 106 Rackham, in the basement of Rackham, which was part of the Statistical Research Laboratory, and the people who were - Bruce Arden was, in fact, the person who was the main operator of it, under Dwyer and Craig in the Stat Lab. And Bob Graham eventually came over as an operator again. And they pretty well ran it. I was a heavy user, so I got very much involved with them, and again, I developed the linear programming stuff. The 650 computer brought a lot of computing to the campus. Unfortunately, it was seen as closely tied to the Mathematics Department because of the Stat Lab being part of the Math Department. So there was still the feeling

by engineers and others that it wasn't as available a facility as they would have liked. In 1959, when the Computing Center was to be established, it was decided that it would be under the Vice President for Research, making it totally available to the whole campus. Not only more available, but perceived to be available.

E. GALLER: How were the decisions made to set up this Computing Center? Was this a facility that was on many other university campuses already, or was this a new idea?

B. GALLER: Well, it was beginning to show up on different campuses. We - I say we - I was sort of involved. I'm not exactly sure how the decision making totally went, but I remember having some input into it. We decided that, unlike many other universities that were starting up computing things at the time, we would separate the academic from the administrative computing. We did not want the computer to be unavailable to people suddenly because the payroll was late or whatever. And so we said we wanted two separate facilities. Now, there had been a Tabulating Service, I guess it was called, for the administration, before that. They had a 602-A system and a CPC system which were punched card systems, and Bruce Arden, in fact, had been Director of that. Bruce had been a graduate student in Electrical Engineering, had had some problems with his adviser, or whatever, and dropped out, and was doing that job as a tabulating service director. And thus, when the 650 came in, he was a natural person. He knew about punched cards, he knew about elementary programming and stuff, and he came over to help and be part of the 650 operation. A few years later, he went back into Electrical Engineering, finished his Ph.D. and, of course, went on to a tremendous career from then on. He was a member of our department, the CCS department. In fact, he was chairman from 1971-73, went on to be chairman at Princeton, a very prestigious department at Princeton, and in fact, is now the Dean of Applied Science and Engineering at Rochester. So we were lucky to have had him in those early years.

Now, the 650 served a great many people, and it was on that computer that we installed the IT compiler, which Al Perlis had developed at Purdue. That was a very pioneering thing to do, because IBM had just brought out FORTRAN and in those days, programming was generally a one- or two-man thing to do. It was a sort of a lone operation to write a program. IBM said that FORTRAN took 30 man-years, which was probably the biggest programming project anybody had heard of at that time. But Perlis took it upon himself to develop the IT compiler for

the 650, a very small machine, with Joe Smith, his colleague, to prove that one could do a compiler without investing 30 man-years. It was very important. We were in touch with him. All the Midwest universities were in touch with each other, largely because everybody was getting 650 computers at that time because IBM was giving a 60 percent discount to universities, a very intelligent marketing strategy, which got computers into where all the future leaders would be coming from. But the 650 had a board for input-output that had to be wired very laboriously; like their previous printers and other machines, each had a board to control input and output, and the university people immediately saw that the only way to use the computer and exchange programs was to standardize the board and never touch it from then on. We had a series of meetings, primarily in the Midwestern universities - Ohio State, Illinois, Purdue, Michigan, Wisconsin - in which we ended up standardizing - I don't know what happened to Board Number 1, but we standardized on University Board Number 2. And we, as a result, had very good connections with all the Midwest computer people. And that's one way we got to know Al Perlis. We installed IT and began to use it on the campus. IBM recognized that they needed a FORTRAN, which had started out on the mainframe computer; they needed one on the 650. They wrote a program called FORTRANSIT, which would accept FORTRAN input and translate it to IT so that it could run on the 650. That was the quickest way they could get a FORTRAN processor on the machine. So they knew about IT, and they appreciated IT. Of course, Perlis chose the name IT because of all the different funny ways it could be used. The technical name was the Internal Translator.

Now, about that time - that was in 1956, 1957 - about that time we began to see the explosion of languages, and that led eventually to the MAD language, but that's another topic. Let me continue for a moment the development of computing on the campus.

With the establishment of the Computing Center, Bob Bartels was named Director of that.

E. GALLER: Why was Bob Bartels chosen? He was a wonderful choice, but why was he chosen?

B. GALLER: I don't know how the University happened to be so lucky as to find Bob Bartels for that position. Actually, he was an applied mathematician. I think his field was magneto-hydrodynamics, or whatever. He had been

to Oak Ridge, and in order to do some of his work there, had become somewhat familiar with their computer, the Oracle, which is a one-of-a-kind machine they had. And so when he came back to the campus, he knew a little more about computing than anybody else except John Carr and me, but I was much too young for that position. So, if they were looking for an alternative to John Carr, he was probably the one person on campus who knew something about computing from his experience at Oak Ridge. It was a major change of career for him, and he knew it. A couple of months before he became the director, he and I happened to be at the University of Wisconsin for a short course on something or other, and we both remember taking some long walks discussing whether he ought to take this job. He ended up taking it. He was director for 19 years. His main attribute, or main strategy in running the place, was to say he had some good people with him, and he wasn't going to get in their way. He organized a very loose organization. We had almost no structure in our organization. We were a group of people who were doing our thing with his encouragement and support, and he buffered us from the administration and got us the support we needed, and it was great.

Now, I should perhaps talk about the operating system and the group that developed it and so on. When we started the Computing Center in 1959, we had ordered an IBM 701. It actually never arrived, because by the time it would have come, it was clear that the 704 had replaced it. The 701 had cathode ray tube display storage, which was obsolete already because the 704 had core storage. There was no point in bringing in an obsolete machine, and so when the Computing Center opened on August 1, 1959, we brought in a 704. Now, in order to run the 704, we needed an operating system. It turns out that in my consulting work at GM - well, it was called consulting work. At first they let me in to use their 701 computer for my work with Paul Dwyer. Then they got a 704, and I converted over to use the 704. And I was going there every week, and they decided the simplest way to give me access at the gate on Mound Road was to give me the symbol that lets everybody in. They said, "We'll call you a consultant. Here's the sticker for your car, and you can get in." And later on, people heard I was called a consultant, and they began to ask me for help on their problems, and then I became a real consultant, and that lasted 20 years. In the process, I was going there all the time and getting very involved with their programming of their system and so on; although I was a user, I was interested in how it worked. And I got to be very good friends with Jim Fishman, who was their operating system guy, and when we decided to get a 704, I got from Jim Fishman a copy of their operating system, and I

reworked it so that a lot of the stuff that was good for GM but wasn't needed at Michigan I took out. I replaced it with a lot of stuff that would be better for small programs of the kind students wrote. I think I really rewrite 90 percent of the thing, but it was based on their system, and even before we opened the Computing Center I used to take programs written by mainly engineers and scientists on our campus back and forth to GM to run on their machine. I was a kind of a messenger, which helped encourage people on our campus to start writing programs for the 704, using the conventions of the GM operating system, which were largely carried over on our system, so when we opened up our shop as the Computing Center with the 704, there were a dozen or so faculty that were ready to use it. Their programs ran in our system. We simply announced that this is the way you use the 704 computer: you run it in this operating system, and you put your name on the front this way, and so on. And nobody knew any different, and everybody used the operating system.

Tape 1/Side 2

B. GALLER: So that was a very comfortable environment in which to teach computing on the campus. Some of the people who were very helpful at that time as users of the system - I remember Vic Streeter in Engineering, who was doing work on the water hammer problem; I'll never forget Chris Nordman in Chemistry, who wrote a large program that filled up all of the 704 core. He wrote it in assembler language. It was his first program, and it ran the first time. He was that careful. And we never forgave him for that, because it shouldn't happen.

I should go back just a minute about the history of operating systems on campus. In addition to bringing over the GM system, on the 650 - the 650 was a machine where people would walk up to the machine, each person setting the dials in his own way and running his program, and leaving. We had a sign-up sheet; you could sign up for half-hour blocks. And we soon realized that this was quite wasteful, because if a person ran into trouble after five minutes and left, there may or may not be the next person around, although we usually had a few vultures who missed signing up and were just waiting for some free time to jump in. But after a while, we said: "Since many of the jobs were short, we would provide an operator, and you just bring your job in and leave it, and the operator will run them one after another and you can come back for your results." Now, a lot of people were not happy with this, because they had

no chance to watch how the program ran, what kind of trouble it ran into, and personally maybe even interact with the program, although that wasn't too common. But that was the way people should use the computer to get more productivity out of the machine. In those days, productivity of the person was less important than the productivity of the machine. The machine was expensive; we had to keep it going.

One of our first operators, I think it was Frank Little, but I could be wrong, devised a one-card program which could be put in between people's program decks, which would, after one finished, automatically call in the next one. We think he really wanted to go out for coffee and didn't want to stay around to run the next program, but it was a clever idea, and it was, in a sense, an operating system, at least at that level. And so we began to be able to run batches of programs. Of course, eventually, the whole major operating system of the 704 included that and many other services, like accounting and everything else. So we became very knowledgeable in operating systems in the 1959-60 time. I did most of the work on that operating system. I was thoroughly familiar with it, of course, since I'd rewritten most of it. In 1960, we brought in from Willow Run, an IBM 709, which was like a 704, except it had input/output channels and so on, but it was mostly compatible. And so we decided to convert the operating system to run on the 709. For that, we had a group of people who helped, and Bob Rosin was there. He and I still remember the night when we stayed up all night finally debugging it, and finally, as dawn came up, it worked. But other people were involved at that time - Tom O'Brien and Frank Westervelt, and, well, of course, MAD was getting going at that time, which I'll talk about. Bob Graham and Bruce Arden were around. Don Boettner was an operator on the computer. Paul Anderson was an operator on the computer at that time. Paul later went to work for Ford as director of their computing. I think he's retired now. Don Boettner is still a computer programmer with us at Michigan. And we converted the operating system to run on the 709, and then eventually on the 7090. And that took us up through the time I went on sabbatical in 1965, which turned out to be a time of major change. Maybe I should talk a little about the history of MAD, along this period, because MAD was one of our major activities at the Computing Center.

It goes back to the 650, when I mentioned that we had the IT compiler from Al Perlis. IT, because of its constraints, mainly the size of storage, and the fact that there were no index registers on the machine, had to have some severe limitations on what could be expressed in the language. And Arden and Graham decided to take advantage of index

registers when we got them on the hardware and also to ... we began to understand compilers a little better, and they decided to generalize the language a little bit, and so they wrote the compiler called GAT, the Generalized Algebraic Translator, which Perlis then took; by that time he had gone to Carnegie-Mellon ... it may have been Carnegie at the time. Perlis took GAT and added some things to it to call it GATE - GAT Extended - and he went on to add a few things to it in that direction.

GAT was not used very long at Michigan. It was okay, but there was another development, because in 1958, there was the ALGOL development in the European-American cooperation between ACM and the European group, and they announced a standard language called ... well, first it was IAL, the International Algebraic Language, but they changed the name to ALGOL, Algorithmic Language, ALGOL 58. They published a description of the language, and said, "Please everybody, implement this, let's find out what's wrong with it. In two years we'll meet again and make corrections to the language," in the hope of getting everybody to use this one wonderful language universally instead of the several hundred which were already developing all over the world. Everybody was doing his own thing; there was an issue of the ACM where the cover showed a Tower of Babel with all the different languages on it, and so on. So the attempt was to try to get a universal language. John Carr was very active in this process, and he came back to Michigan and said, "We've got to do an ALGOL 58. To help the process, let's find out what's wrong with the language. We know how to write language compilers, we've already worked with IT, we've done GAT, and so on. Let's see if we can help." So we decided - I was involved, and Arden and Graham, and John Carr a little bit, but he never really was very involved. He left in 1959. We decided to do an ALGOL 58 compiler. It turned out that there were some things wrong with the ALGOL 58 language specification. There were some things that were very difficult to do, very foolish inclusions and so on, and when you actually write a compiler, you discover that you have to make a number of decisions, and so by the time we designed the language that we thought would be worth doing, and we could do a compiler for, we said we couldn't call it ALGOL any more; it really was different. What should we call it? And so that's when we adopted the name MAD, for the Michigan Algorithm Decoder, and we had some funny interaction with the Mad Magazine people, who finally gave us permission, in a very funny letter, which told us that they would take us to court and everything else, but had a P.S. at the bottom - "Sure, go ahead." Unfortunately, that letter was lost. But we decided to write a compiler, and at first it was Arden and Graham who did

all of this. I was helping, watching and so forth, but it was mainly their work because they had worked on GAT together. And then at some point I said, "I want to take over part of this thing, too, and really join the group." And they said fine. Arden was doing the back end of the compiler, Graham was doing the front end of the compiler. We needed someone to do the middle part which would glue the pieces together and make everything flow and provide all the tables and so on, and I said, "Fine. That's my part." So Graham did Part I, Galler did Part II, and Arden did Part III.

A few years later when Bob Graham left and went to the University of Massachusetts where he is now, I took over Part I. So I had Parts I and II, and Arden had III, and we kept on that way. I was very glad that Graham had developed some very powerful macros in Part I which made my job a lot simpler, and for which I want to absolutely give Bob credit for doing a good job.

So we did the MAD compiler in 1959 and 1960, and I think it was 1960 when we went to that famous SHARE meeting and announced that we had a compiler that was in many ways better and faster than FORTRAN. It was a better language, and so on. There are people who still come up to me and tell me they remember my standing up at that meeting and saying at one of the FORTRAN discussions, "This is all unnecessary, what you're doing here, because you've got to come to our session where we talk about MAD and you'll see why." And people remember that, because it was true. I remember one ...

E. GALLER: Did they think that you were being very ...

B. GALLER: Brash?

E. GALLER: ... brash, because you were so young?

B. GALLER: Of course, and well, who would challenge IBM? But okay. I remember one time, a little bit later, we had a visit from a guy from IBM at Michigan. He came to Ann Arbor, and we went to lunch, and he was telling us that

they were so excited; they had discovered how to have the computer do some useful work during the 20-second rewind of the tape in the middle of the FORTRAN translation process, and we sort of smiled. And he said, "Why are you smiling?" And we said, "That's sort of funny, because the whole MAD translation takes one second." And here he was trying to find something useful to do during the 20-second rewind in the middle of their whole FORTRAN processor.

What happened in developing MAD that was important was that we were able to get the source listings for FORTRAN on the 704, and Bob Graham studied those listings as to how they used the computer. The 704 computer, at that time, had 4,000 words of core storage and, I think, 8,000 words of drum storage. And the way the IBM people overcame the small core storage, the 4,000 words, was to store their tables on the drum. And they did a lot of table look-up on the drum, recognizing one word for each revolution of the drum, and if that wasn't the word they wanted, then they'd wait until it came around, and they'd look at the next word. Graham recognized this and said, "That's one of the main reasons they're slow, because there is a lot of table look-up stuff in a compiler. You look up the symbols, you look up the addresses, you look up the types of variables, and so on." So we said: "Fine. The way to organize a compiler then is to use the drum, but to use it the way drums ought to be used. That is, you put stuff out there for temporary storage, and you bring it back once and you use it again when you need it." And so we developed all of our tables in core; when they overflowed we stored them out on the drum. That is, Part I did all of that. Part III, then, would call in the relevant table when it needed it and use it. We did no look-up on the drum, and we were able to do the entire translation in under a second. It was because of that that MIT, when they developed their time-sharing system, which they called CTSS, the Compatible Time-Sharing System, and they needed a fast compiler for student use, used MAD, and it was their in-core translator for many years. So we developed, then, MAD in 1959, 1960. It really got finally working in 1961. In January ... well, February, I guess, 1961, we went to New York. Our 709 had arrived, or was about to arrive. We went to New York, the three of us, in the middle of that big snowstorm, to debug the new MAD version, the 709 version of MAD. And we were using the Socony Mobil 709 at night after they used it in the daytime. So we would sleep most of the day and work at night. And we got the 709 version of MAD debugged, so that when our machine arrived, we were able to use it. Our campus used MAD until the middle of 1965, when the 7090 computer, which we had by that time, left. I think during the last four years of its life, we found no

additional bugs; it was a very good compiler. And the important thing about it, of course, was that we had a number of language innovations, and notational innovations which were, some of them, picked up by the FORTRAN group to put into FORTRAN IV and its successors later on. They never really advertised the fact that they got ideas and in effect got some important notation from MAD, but they told me that privately.

So MAD was very important. We published a number of papers. One important thing we did was we had a language definition facility. Now people refer to it as an extensible language facility. It was useful and important, and it worked from 1961 on, but somehow we didn't appreciate how important it was, so we didn't really publish anything about it until about 1969. And I was later lectured to by John Reynolds - I think he was at Argonne, an important language guy - who told me that we were very derelict in not publishing it sooner because it was so important, and the world should have known about it sooner. Well, in hindsight, yes. There is a lot of work in extensible languages now, and unfortunately, not a lot of people credit the work we did, partly because we didn't publish it for so long, and while people knew about it and built on it, there was no paper they could cite.

So we think the work on MAD was very important, and the work on the operating system stuff was important. That was the foundation of Michigan's Computing Center activity.

Another important series of things that was happening at the time, in the late 1950s, was the Engineering Summer Conference courses. I mentioned that John Carr had in the early 1950s gone to the MIT summer course and learned a lot. There had been a tradition at Michigan for having Engineering Summer Conferences in a variety of engineering disciplines. These were one-week courses; later, a lot were two weeks, and now they're down to three and four days just because everything is so expensive. But there was this tradition, and Arden and I in particular decided to offer some programming courses. It was all very new, and there was a use for it, so generally Arden ran the more advanced course and I ran the more elementary course, but we collaborated on everything. And we brought in, especially in the advanced courses, the very top people from all over the country -sometimes from Europe as well - to lecture on the latest things that were happening. Perlis came almost every year. In fact, since Perlis had been involved in ALGOL, I have, in his 1959 lectures, which was right in between ALGOL 58 and ALGOL 60, I have tapes

of his ten lectures on ALGOL and its structure and some criticism and so forth. I've donated those to the Charles Babbage Institute. Very beautiful lectures. But he came almost every year. Bob Barton was there, Tom Cheatham, Tolly Holt, Ben Dent from Burroughs. We had a great many wonderful lecturers over the years, and those were documented. We insisted on notes from everybody. We got notes from almost everybody, and those are in the library. Beautiful notes, generally, and those were very important in spreading programming and ideas around the world. In fact, one of the papers I published with Al Perlis was developed in a conversation we had at one of those summer conferences, the one on matrix notation. So those were important courses. They went on for a number of years.

E. GALLER: Who attended those courses?

B. GALLER: People from primarily industry; sometimes universities, but primarily industry. they would send their experts for a few days to get up to date on the latest ideas, and they were the latest ideas. They were really leading-edge stuff, because we had the most important people. Andy van Dam would come in and talk about his graphics stuff, and we would talk about operating systems and things. It was really excellent.

Now, getting back to the Computing Center activity, in the early 1960s, we needed to continue to expand our computing facilities, and we talked to IBM about the 360 family. They were interested in time sharing, but not too interested. They had talked to MIT; the MIT people were very active. They had had success with their CTSS system, they had ideas about how to build computers better for time-sharing systems, and we knew about those developments. We also knew that IBM and MIT had had big arguments and had had a falling out. They were not really talking to each other. We began to talk to IBM about what we needed. And at first, we had some cooperation with MIT and their GE system. They invited us, in fact, to join them in developing an elaborate time-sharing system, which ultimately became MULTICS, with GE. We decided, with some internal discussions, that if we did that, we'd always be second fiddle to MIT, that we thought we knew a lot ourselves, and it would be better if we didn't join that particular consortium. Also, there was a great deal of secrecy of what was going on there, because it was considered a proprietary product for General Electric, and that just wasn't our style. We decided to talk to IBM about how to

upgrade their computer to be better for time sharing. Later on, it turns out that the MIT people accused us of stealing our ideas, and we didn't think so. We had some very soul-searching discussions. Did we, in fact, not treat what we did know about them properly? We decided that we acted ethically, but for about ten years, the MIT people didn't talk to us; in fact, were pretty negative in talking about us to other people. But it blew over finally, and now we're friends, and it's okay.

The discussions we had with IBM took the form of "You've got this new 360 system. It needs some additional hardware in order to make time sharing work better." We had some software ideas, we published them in the ACM journal, and we did influence IBM to add some additional registers, and, in fact, it was virtual storage, the beginning of virtual storage. MIT had developed one view of it, and one way of doing it; ours turned out to be a different abstract model of what it should look like, and so it ended up different on the IBM machines. We asked IBM to build a machine for us with that stuff on it, and we said we would develop the operating system for it, because we knew how to do operating systems. Just give us the hardware that we need. And they began to build the model 360/66M. The M, we think, was Michigan. It may have been for "modified"; we don't know. But in any case, they started to build that for us. We mentioned what we were doing to some other people - Perlis at Carnegie, the General Motors people, and so on - and they got interested and began to tell IBM that they wanted the same thing. And IBM finally realized that they had a potential product. So they committed to building a new machine, called a 360/67, and they said, "Because we've got these customers, we'd better build an operating system for it." So they came to us and said, "We're going to build a number of them, and we'll provide an operating system." And what they did was they brought together six of their most influential customers. We were one of them, General Motors, Lincoln Labs, Princeton University, somebody else, and we were the inner six that were advising IBM on what the operating system should look like. Unfortunately, each of us had different needs. We needed to run lots of small problems for students, General Motors needed to have very large data sets for their automobile model designs, Lincoln Labs needed ... they argued for a long time; we finally talked them out of it ... two special bits in each word so they could recognize a specific kind of data coming in from a satellite to Hawaii, and that sort of thing, and everybody had different goals. IBM tried to accommodate everybody by putting everything in, everything, they only missed the kitchen sink. The system grew. It was called TSS, Time-Sharing System. It grew and grew and became so unwieldy

that on Black Monday, January 18, 1967, they sent representatives to everybody at that time who had ordered that computer - there were at least a couple hundred universities by that time that had heard about it and were waiting for it and had ordered machines - they sent people out that day to say to everybody at once, "Sorry, we are canceling the operating system. The machine is now going to be called an experimental machine, and we're not so sure we are going to deliver it." And so on. And they lost 120 orders that day. Michigan decided to stick with it. We had confidence in it, and we said, "We think it will work. We think the ideas are good." That was in January. We began to have some meetings ourselves as to what to do about this, and we realized that we'd better do it ourselves and take advantage of the fact that Mike Alexander, who had joined us as a programmer, was playing around with those little time-sharing systems that people had developed by that time, and was somewhat familiar with the multi-programming system from Lincoln Labs, and we said, "Let's develop our own based on that, and make it available to the campus." The model 67 had just arrived, and it was possible, using very elementary techniques, to get a few people on in a time-sharing mode. We said to people on the campus, "We will deliver services in May free of charge on this thing, and in June we will start charging you." It was a rather elementary system at that time, but because we adopted the standard IBM linkage conventions, we were able to bring in all kinds of software from other places and just dump them into our system, and it worked. So it grew very rapidly. All of that software was public domain at the time. There was no question about being able to use it in terms of copyright and so forth. The computer software industry hadn't started yet. That really started, in terms of selling software and so forth, in 1969, when IBM decided to unbundle their software and charge for it separately. So we were still in a period when people were exchanging software quite freely, and certainly all of what IBM delivered with their computer was considered public domain. We didn't use the IBM operating system, but we were able to take whole sections from it like the FORTRAN translator and so on.

So we were in business quite rapidly, and MTS, as the new system was called, the Michigan Terminal System, grew. It was quite good, and people - Alexander and Boettner were the two who really developed it. Other people jumped in and helped developed stuff. I remember Westervelt and O'Brien and other people were there. It was a very exciting time, lots of new developments, and especially the demonstration that the model 67 worked. IBM had said, "We don't have any confidence in this machine." We were able to make it work. And it was, I believe, at the 1968

international [IFIP] conference in Edinburgh that people were making disparaging remarks about the model 67, and I got up and said, from the audience: "We at Michigan are making it work. We think the hardware is very good. If IBM has overextended itself on the operating system, that needed additional work, and we don't know if it will ever work, but we are successful, and the idea of virtual storage is important and good." And I think ... a lot of people later told me that was an important statement because it was some years after that, not too long after that, right about that time in 1969, 1970, I believe, that IBM had the tremendous internal debate as to whether or not virtual storage should become its standard way of doing things. I was told later by Walter Carlson that the success of Michigan was very important, very influential, in getting IBM to adopt virtual storage. I remember being invited to Yorktown Heights to give a lecture on it, and hearing at that time about the internal debate that was going on. So the fact that IBM later did adopt it for the model 158 and 168 was influenced by our work at Michigan, and it's a good feeling to know that you had some influence, because now every computer in the world has virtual storage on it.

Maybe I ought to say what virtual storage is. It is a technique which is partly in hardware and partly in software for presenting an image to the programmer as if there was a very, very large storage, very large set of addresses to use, so that it appears as if all of the programs and the data that you are using are in storage, and you can work directly with them and not have to worry about the fact that some of it should be physically seen as on the disk or secondary storage, and so the problem of bringing in programs and data from secondary storage into core to use it is automatically done by the system and hidden from the user. I don't have to think about bringing in parts of my program and data, whereas we used to have a big process of overlaying previous program segments with other program segments brought in from the disk, and so on. The big management of all of that stuff now is totally automatic by the system. And at the time when we were arguing for it, we used to estimate that people spent perhaps 50 percent of their programming time on that aspect of it, and so it provided a great productivity improvement for people, and also led to much more efficient use of the computer overall, although there is some overhead associated with control, managing all of that movement of stuff in and out of storage, and so on. Later on, machines were developed so that the hardware was more efficient at doing that, and the overhead was cut down. But the concept was very important. And it was actually that concept that led to my trip around England after our sabbatical, which I'll come to later, because I was lecturing on that topic. It was new for most people, and I was lecturing on that all

over England, which led to my connection to Newcastle, because they were very interested in that, and eventually they used MTS at Newcastle because of those lectures and that time-sharing system.

So the late 1960s and the 1970s were the heyday of time-sharing, and the Michigan system was one of the best in the world, as a matter of fact. IBM never really got too involved in timesharing as such. They saw a large market for what they called transaction systems, where people would interact with the computer, but mainly individual transactions like banking or airline reservations. It was not interactive programming, it was not really interacting, except that when the computer needed input it would signal to the operator, "Here input is needed," and they would enter a name or something, and it would go on. It was all driven by the computer, and the person was doing what the computer needed. Those are transaction systems. Time sharing was really quite different. IBM never really saw that there was a big market for it. They eventually added a time-sharing option - they called it TSO - on top of their system. It never really ... it got widespread use because it was all they had, but it had tremendous overhead, very slow, and nobody liked it, really. We never could persuade IBM to market MTS, though, because it was not seen as an important part of their market, time sharing, and they had their huge standard operating systems and everything else. There was an internal IBM operating system called CMS, which ran in their VMS, virtual operating system. CMS got some popularity, but never really was as good as MTS. We looked at it a few times, decided it wasn't as good as MTS, and we never adopted it. The virtual operating system idea eventually became important, because that was a way in which people could run several different operating systems on one machine. And when IBM developed different versions of their operating system, they saw that they could market a very large mainframe to someone and say, "You can replace the different machines you have running different operating systems by this one huge machine, the virtual VMS operating system would let you pretend to run different operating systems all at the same time." That turned out to be very important for them.

Now, this was, then, the heyday of time sharing, and especially at Michigan. It was such a good system, and so widely used on our campus. Everybody used it for courses and everything else. We didn't really recognize the impact that personal computers would have on the world and on our campus, and we were too slow at the Computing Center in moving in that direction. The Engineering College picked up on it, especially because the early personal

computers were really pretty powerful workstations which they needed for engineering work. So they acquired a number of these. They made a big deal with Apollo and eventually had more Apollo machines than any place in the world, although now Apollo isn't seen as the best machine to have, and so we are almost a little behind now in some aspects. Apollo was actually acquired by Hewlett-Packard not too long ago.

So the Computing Center began to look pretty stodgy in terms of not moving fast enough to personal computers, and we began to get a somewhat tarnished reputation on the campus. We eventually did start to move in that direction, but we were always being dragged, in a sense, by the Engineering College.

E. GALLER: What years were those?

B. GALLER: Those were the late 1970s. And in fact, independent of all of this, of course, Bartels retired in 1978, and Aaron Finerman came in. And Finerman began to move somewhat in the direction of personal computers and workstations, pushing for networks, but the University didn't really respond. We put together a big proposal for a major network activity, and it sat on [Vice-President] Billy Frye's desk for three years with nothing happening, partly because the Computing Center's reputation was a little bit tarnished, and we didn't have the clout we'd had in the previous years. So that sat for a while. Finally, Billy Frye and some of the other people realized that we ought to get into networking, and they did authorize a major expansion, and then we did finally move rapidly into computing, into networking, actually ahead of most universities. We had been very far ahead of other universities in recognizing the need for networking; even with the three-year delay, we were still well ahead of people. The administration had gotten a WangNet, and we watched that and realized that that was not the way to go. They were a very proprietary network which only used Wang computers, and that was clearly not the culture at Michigan. At Michigan, on the academic side, we recognized that people would forever be getting whatever they wanted, based largely on what was important in their own disciplines. Physics was getting DEC Vax's, and somebody else was getting some Primes, and so on, and no way could we control what people would be getting, and so as we had with terminals, always accommodated all kinds of terminals we said better be able to accommodate anything on our network. And so we took a more general view of networks, and even with that delay we were among the first, and developed some

important innovations. For example, what we called a Data Concentrator was developed at Michigan as the front end to interface between the mainframe and the networks and personal computers and so on. That was developed by Dave Mills, in particular, at the Computing Center. That kind of innovation gave a lot of expertise in networking, and so we were ready to go when the University finally put the mo ney in, and it became very important. And today, Michigan is probably networked as well as or better than any campus in the world. And that led, eventually, to the ability to compete successfully to be the manager of the NSFNet backbone, which we are now.

Now, getting back to the workstations and personal computers, we eventually did install a lot of those - later than some people would have liked - but we now have .... I think we are second in the number of Macintoshes in the world, but we have as many personal computers around for public use and in the residence halls and so on, so that I think we are more deeply involved with the use of computers on this campus than probably any other place.

Now, back to the history of it. Finerman took over in 1978, and it was during that period that we acquired Amdahl computers - I think actually we acquired the Amdahl computer a few years before that. We were well into Amdahl computers at that time. One of the things Amdahl, at one point, offered us, I think it was in the early 1980s, a second computer because they weren't ready to deliver the next big one that we wanted, and so we said, "Okay. We will take a second computer, and we will, because of the government regulations, we have to charge everybody the same rates ..." And this hurt the students, because we had to charge the students as much as we charged the outside people, and although the University provided money for the students, we had to ration it a lot, because we were charging a lot for the use of the computer. When Amdahl offered us this second computer, we said we would set it up as a second computer, primarily for students, and we would change the rules on that one and make it much cheaper for students to use. In addition, we said we would provide a certain amount of money for every student automatically, so that maybe we would have to charge a fee for it, but we would provide some money for every student to use on the mainframe instead of rationing it out only when they had a course that needed it or whatever. And we were just starting up that idea when Finerman retired as director, and we actually implemented it shortly thereafter. The second computer system was called UB, the first one UM - probably for maize and blue - I've never been sure - and they remained that way, as two systems, even later when we finally consolidated everything into one computer, we

implemented it as if there were two systems on it. But some time later in this year, 1991, I think we're finally going to combine it back into one system.

Now, I mentioned that we had the Amdahl machines. This was in the late 1970s. Amdahl had been with IBM, Gene Amdahl. He was one of the designers of the 360 system. I think he had some ideas that were not accepted later, and so on, and he left ...

Tape 2/Side 1

B. GALLER: Gene Amdahl had some innovative ideas about how to build a faster computer than IBM had, but he also had the important idea that his computer would be architecturally compatible with IBM's, so that the same software would run. Since he was doing his own engineering, he wasn't violating any of IBM's patents. He did it himself, or if he did violate them, he got permission or license or whatever. In any case, he built a computer he could offer as faster and less expensive than IBM's compatible machine. We were intrigued by the idea that it was a new, exciting development and, in fact, it was a better deal economically than IBM could offer. We were going out for bids, when we needed a new computer, around 1975, and so we got the Amdahl computer. We actually got either number one or number two. I think number two; number one was in their own shop. So it was very new. We were a beta test site. That is, we were a special customer development testing site, and so they put a lot of people with us to help debug any hardware problems. The relationship developed very well so that it was a good thing to have done. Later, when Van Houweling came in over all computing at Michigan, a decision was made to go back to IBM when we needed to get another computer, and bids went out and so on. Our technical people were not entirely convinced that the IBM machine was better, but I think the price was right, and I think Van Houweling wanted to go back to a good relationship with IBM, which, in fact, has developed over the years and been beneficial to Michigan.

E. GALLER: How did the Computing Center change when Professor Bartels retired and Aaron Finerman became the director?

B. GALLER: Well, as I said before, Bartels had almost no structure. We didn't have that big a staff, so everybody was pretty well equal, and we divided up our responsibilities. We had a philosophy where each person was responsible for huge chunks of the operating system, the programming, and so on. We probably didn't have enough overlap in knowledge, but it worked out okay. The systems were not that complex; we each knew more or less what everybody was doing. But as the systems grew and more and more people came in, that was not necessarily the best way to organize a big operation which, on the one hand had development activity, but on the other hand was running a production system for the campus. When Finerman came in, he recognized that we did need more structure, and he made some people ... we had what he called his Committee A - I'm not sure we ever had a Committee B, but Committee A consisted of about 10 or 12 of his senior people, and we made policy decisions with Aaron and provided a kind of a managerial structure, not a tight structure, but it was more organized than under Bartels. And this continued until Finerman stopped being director, and at that time Van Houweling had come in over all of computing on the campus. He took on as his role the consolidation of the academic computing, which we represented; the administrative computing at the Data Systems Center; and the telephone network, which was becoming -- by the late '80s (he came in 1984, approximately) most universities were beginning to consolidate these three activities, mainly because the telephone network was becoming so important in data communication. So then Van Houweling was brought in from Carnegie-Mellon to organize the Information Technology Division. I think he is a Vice Provost in the University, and not too long after Finerman stopped being director, he undertook to reorganize all three into a more coherent organization because it was clear that there were overlapping functions going on in all these different parts. So it is now reorganized as a division, with several functional units under it. And unfortunately, there is no Computing Center any more, which for some of us was a break with nostalgia.

Now, with regard to networking on the campus, many years ago - maybe 20 years ago - with Eric Aupperle's leadership, we started the Merit network, which was a state-supported network for the state of Michigan. This was partly funded by the state, partly some NSF money. It was a consortium of primarily Michigan, Michigan State and Wayne State to develop a computer network. It turned out that this was one of the oldest and one of the most effective state networks in the country, and has now spread to I think all of the state universities and many government offices, with connection points all over the state. I am able, from Sutton's Bay here, to dial a local

number into a connection point at Traverse City to the network. And it is connected to the National Science Foundation backbone which crosses the country, so that we are able to connect to any computer that is on the Internet, the main network of the country. And when the National Science Foundation decided to contract out the management of the backbone which crosses the country of the National Science Foundation's NSFNet, largely due to Van Houweling's energy, Michigan, IBM, and MCI, (the communications company), very feverishly worked over the summer and produced a very good proposal which won the competition, and so the Merit network operates the NSFNet backbone with sponsorship and cooperation and hardware largely from IBM and MCI, and it is managed out of what was a Computing Center building ... it is still called the Computing Center building, but it is in fact the NSFNet backbone management center. And a later result of all of this activity, which has now placed Michigan at the center of most of the network activity in the country, when IBM, Merit and MCI formed a non-profit organization called ANS (Advanced Network Systems, I think) to commercially sell network services, using the actual hardware of the NSFNet with NSF blessing, the underpinning of that operation of that new company was the fact that they were operating the NSFNet backbone already.

E. GALLER: Now let's move on to talk about your research activities. You've already talked about some of the things you were involved in, such as the operating systems and MAD and timesharing and virtual storage. But could you tell something about the Ford Foundation project?

B. GALLER: Yes. The Ford Foundation project ran from 1959 to 1961. It was a project headed by Professor Donald Katz of our Chemical Engineering Department. The idea for which he got almost $1 million from the Ford Foundation, which is a lot more than $1 million now, was to upgrade the use of computers in undergraduate engineering education. And the money was used to provide released time for faculty at Michigan and other places, many of whom came to Michigan for the purpose, to develop computer programs and applications to use in their courses to teach undergraduates. Computers were really quite new, and it was recognized that they were very useful in engineering, but they hadn't really gotten into the curricula. I know people who told me that in their opinion, this project advanced the use of computers by at least five years throughout the country. We concentrated heavily on documenting everything that was done. Don used to say that the only value after this is over, the only value of what

we have done, will be in what we documented, and so a lot of documentation was produced and distributed all over the country. One document that was produced - fortunately - was my first book. We were developing courses in programming as well. One of the courses I in fact started at that time was a non-credit course, which we offered three times before we actually changed it to be a credit course in programming, and this attracted a lot of students and faculty to try the computer. And that was the basis of my writing my book, and the project provided the typing to get the book into shape, and that was the first book, called "The Language of Computers," published by McGraw-Hill in 1962, shortly after the project was done. It was interesting that when I was offering these non-credit courses, I had a number of faculty come to me who were embarrassed to be seen taking the course with the students, and they asked me: "Would I please give them a separate course?" And so I used to meet on Saturday mo rnings with the faculty because they were too embarrassed to be seen with the students. The students were in many ways ahead of the faculty in this regard, and every once in a while a faculty member somewhere in engineering would call me up and say, "I've made some assignments in my course, and some of the students want to use the computer for it. Do you think it's okay? And how do I do it?" and so on. And that's how it spread. That's one of the ways in which it all spread through the campus. Eventually that course became one of our programming courses.

Speaking of Donald Katz, a little later he chaired a committee for the University to assess whether the University needed time sharing, and it was largely on the basis of the results of that committee's work that we were able to work with IBM on the Model 67 and so forth, because we had the University behind us, because that committee interviewed many people on the campus and ascertained that if in fact we did have a good time-sharing system, it would be used.

I remember one funny incident, though. They went in to interview a faculty member who had no idea what time sharing was about, but after they talked to him for a little while, they came away reporting that he would use 11 terminals if he had them. So in some ways, the report was an exaggeration, because they were talking to people who had no idea how to use it if it were there. But it helped, and of course, the rest is history.

I'll talk a little bit about another kind of research that I was involved in, and that is with Martin Piszczalski. Martin

was an undergraduate in psychology when I was chairman of the Computer Science Department, or CCS, which we will come to later. He came in to ask if he could use some of our equipment. He was interested in analyzing speech sounds, and maybe musical sounds. And as we talked, I realized that he was a very bright person and that it might be very interesting to look at this analysis of musical sound. I didn't know that anyone had done that. It turned out, in fact, almost nobody had. I let him, therefore, use the spectrogram machines and so forth in the department, but I suggested to him that we should jointly propose a project to the National Institute for the Humanities because it would be useful for humanities people if we could analyze musical sound and show them what the musical score was that they were listening to. We did, in fact, get a grant from NIH, and later on a second grant from NSF when it turned out to be a little more technical than Humanities-oriented. The work that primarily Martin did, with my overseeing and conversations and support and so on - it really was basically his work - turned out to be ahead of anybody else in the world. There was a group in Sweden that was working on some of the same stuff, and some of it was going on at Stanford. Martin's work was really the best of all, and we published a number of papers together, and finally, he gathered up all those papers and put some glue around it, and that was his doctoral thesis, and he got his Ph.D. out of it. It was very interesting work. He used the computer on the roof of the East Engineering Building, with the support of Bill Williams, who had a Bio-engineering laboratory there, and it was very successful work. And at one point, he produced a videotape, actually a movie, showing graphically the results of his analysis, with the music playing behind it - it was synchronized. And we actually presented that as a paper at the AAAS meeting and got a lot of very interested people aware of what we were doing. It was an enjoyable period for me.

The area in which I am most currently involved is IVHS, the Intelligent Vehicle Highway Systems. This is what people would normally recognize as the smart car-smart highway. Basically, it is using communications with computing facilities in the car, and in the roadside, to work toward a number of objectives, largely safety, on the highway - better use of energy, driver convenience, a whole bunch of objectives. Michigan is one of the three main academic centers involved. We are specializing in what is called ATIS, Advanced Traveler Information Systems, which is largely to do with advice to drivers on congestion on the highways, what to do about it, and coordination, ultimately with traffic signal systems. Berkeley people are worried about very far out Advanced Vehicle Control Systems, AVCS, where they are trying to ultimately have platoons of cars traveling 100 miles an hour one foot apart

down the road, totally under the control of the computers in the cars. There are lots of interesting technical and very important legal considerations if anything goes wrong, and so on. So they are worried about that kind of system. Texas A&M are specializing in ATMS, Advanced Traffic Management Systems, which is mainly concerned with traffic signal control over wide areas, and of course, that interfaces strongly with what we're doing, because if we do advise drivers to get off the highways because of some congestion and so on, they are immediately going to flood the neighborhoods with traffic, and that's where the traffic signal management comes in and they have to be coordinated. So these are very interesting research questions, very complex systems, because not only are there combinations of communications and computers, but also legal and sociological questions involved with driver compliance with suggestions, human factors - how you present the information to the driver in the car, questions of local political jurisdiction over roads, and all kinds of questions, and compatibility across international boundaries. We are cooperating with some people in Canada, but there are also all kinds of things going on in Europe and Japan, and we want as much as possible to be able to standardize things. So I am on a national committee -- an organization has been formed called IVHS AMERICA, very broad representation from government, industry, academia and the public, so broad that we qualified to be what the government calls a "utilized organization," which really means an organization that the government is free to ask advice from because they are not a lobbying organization because there is such wide representation. So we are an advisory group to the Federal Highway Administration and I am on one of the technical committees. I am on the steering committee for IVHS AMERICA System Architecture. And this is one of the core committees in the organization, and Michigan is one of the more active organizations nationally, as I have been saying. I am in charge of running a conference in October, on system architecture, with Professor Kan Chen, who really is the key guy in Michigan. In fact, Kan invented the term IVHS, which is now used internationally. It is a very interesting and active field of research, and when I plan to retire from the University, as we'll talk about, maybe two years from now, one University activity that I really do want to continue is this IVHS activity.

Some of the people who are involved with me in this IVHS activity - for communications, we have Marlin Ristenbatt. I've just been looking at a book on spread spectrum communications, and I noticed references to papers of his from the late 1970s in this area. He and I are working together on an interesting proposal for an in-vehicle computer and communications unit. I've mentioned Kan Chen. Bob Ervin from UMTRI, University of Michigan Transportation

Research Institute, is very involved. He and Kan Chen really organized the project at Michigan. And we have other people, for example, John Nystuen from Architecture - Urban Planning, that is, and Bob Marans from Survey Research Center, and so on.

E. GALLER: As a professor, you have had so many opportunities to travel in other parts of the world and be very interested in international activities. Tell about your first trip to Europe in 1962.

B. GALLER: I was invited to be on a panel about operating systems at the IFIP international congress. IFIP is International Federation of Information Processing societies. IFIP has had every three years an international meeting, and in 1962 it was in Munich. I arranged to fly to Paris and join Dorli and Alan Mayerson who were planning to be there at that time, and drive through France, through the wine country, the Loire Valley, and on into Switzerland, because they were going to visit Dorli's mother in Zurich. We did that. It was a most interesting and pleasant trip through France. The chateaus we visited were beautiful and interesting. What I think I remember most was drinking so much wine I would fall asleep in the car every afternoon while they were driving. They spoke French, of course, beautifully, so we had no trouble in the rural areas of France. We ended up in Zurich with a nice visit to her mother, and I went on from there to Munich. It was an important meeting for me personally, because I met many of the European leaders in computing, which made it possible to have interactions with many of them afterwards. It was very helpful later when I was president of the ACM to know all these people in other countries.

E. GALLER: Tell now about the sabbaticals we had. Our first sabbatical, in 1965.

B. GALLER: Yes. I think that was probably the next foreign travel that we did. I had a sabbatical coming up and Perlis and I wanted to write a book together. We had been in good touch over many areas of computing, and we thought it was time to write a book. So we wrote to the people in Israel, said "Would you like us to come?" He also knew van Wijngaarten in Holland and wrote to him. But we were hoping to go to Israel. We really wanted to be in Jerusalem to do this work. Unfortunately, the Israelis told me later, they didn't know what they would do with us if we came. Of course, all we wanted -- we told them all we wanted was a desk to work at, but they took so long to

answer us that we gave up on them and wrote to Holland and said, "Would you like us to come?" And they were much quicker about it, and so we got invited to Holland. And we went to the Mathematisch Centrum on Tweede Boorhavestraat, I remember, which was a building that was a part of the Amstel brewery, and the strong smell of hops was ... I don't know if it was invigorating, but it sure was there. It was a little mathematics center. They had built some computers there, and they were interested in computers. Every morning and every afternoon, ladies would come around and offer us tea in our offices. It was very relaxed, although the rooms were cold. We had to wear sweaters all the time. They were interested in our work. We gave some lectures to them. I think we influenced Algol 68. There turned out to be an extensible language facility in Algol 68, largely because of our lectures, based on the work on MAD. And then Perlis and I wrote a book together. And our style was that he would outline a chapter and I would write it. And we worked well together. The one thing that was unfortunate, the final chapter that we were planning, on binding time, would have been very important, but Al never quite got around to doing it, so it never got written. Finally, I gave up waiting, and we went to publishing it. And so it was published in 1971. That was a long delay after the 1965-66 feverish and very productive writing. It was a wonderful experience. During that whole time, we had our whole family with us, and we lived for a couple of weeks in Wassenaar, but then we moved into a beautiful home in Hilversum, and I commuted, bicycled to the Hilversum train station. I parked the bike in a garage for a nickel a day, and took the train into Amsterdam, and then the tram to the mathematics center. It was a lovely experience. It was probably the most enjoyable year of our lives to have been in Holland, to get to know the Dutch people. While we were there, Enid and I made several trips to other countries, leaving the kids with very fine babysitters, young Dutch girls who knew English and were interested in doing it. It was during that year that, over the Christmas period, we made a trip to Italy, but also primarily to Israel, during which time I met a number of the Israeli computing people. Perlis gave a lecture to the Israeli computer society and I was there and made some important contacts, which turned out to be very helpful later on.

At the end of that year, I was invited to make a trip through England to give a series of lectures at a number of universities.

E. GALLER: This was June of 1966.

B. GALLER: June of '66, and that's when I mentioned previously that I made the important contact with Newcastle, Ewen Page and Bryan Randell and Elizabeth Barraclough and Jim Eve, all of whom turned out to be very important for MTS and virtual storage on the one hand, and later on for our second sabbatical over there in 1973. It was a very happy and productive year, that sabbatical academic year 1965-66.

The next international contacts, I think, came when I was invited to lecture in a NATO summer school in Lyngby, Denmark, just outside of Copenhagen. Enid and I spent that week there and also went out to Stockholm. I shared the NATO lectures with Don Knuth. I gave, I think, half of them and Don gave the other half. There may have been one other person who did a little bit, but that's when I got to know Don and it was a very interesting and useful, I thought, experience.

That was 1967. The following year, in 1968, was the beginning, I think, really, of my involvement with Israel computing. Frank Moser was visiting in Ann Arbor. Frank got his Ph.D. in geology at Michigan. I was on his doctoral committee as the outside person because he was involved with the computer. He then emigrated to Israel again and ...

E. GALLER: He and his wife had lived in Israel for many years before that.

B. GALLER: Right. But he came back to America to study, and then he went back to Israel. When he later was visiting in Ann Arbor, we had an occasion to talk about how we could help Israel improve its computing knowledge and expertise. And I mentioned to him that I was doing these Engineering Summer Conference courses that we talked about before, that there was the beginning course, a two-week course, and the advanced course, a two-week course, that these were given concurrently at Michigan, but it could be useful for Israel if we offered the same courses end to end and just took some of their people right straight through for four weeks, from beginning to advanced. He went back and talked to Aharon Ge rtz, who was a very important person heading up a government computing agency, and Dov Chevion, and they eventually found a way for the government to sponsor these courses. I guess they charged

some fee to come, but it was largely underwritten by the government. I arranged for exactly the courses that we were offering that summer, all of the lecturers - Andy van Dam and Bob Rosin and Bob Barton and who knows - everybody we had, to simply come to Israel and do the same thing again. So we used the same notes. It was a big package deal. I arranged to bring my whole family with me, and we stayed in the big National Palace Hotel, which was in East Jerusalem, just outside the old city. Very interesting experience.

E. GALLER: This was just one year after the '67 war.

B. GALLER: One year after the '67 war. I think that was probably the year in which the relations between the Arabs and the Jews were the best they have been. I don't think they were great, but it's been down hill ever since. That year, we were able to walk freely in the old city, and we stayed in the hotel in East Jerusalem. It was an Arab hotel, and it was a very good experience. I learned later that it had an important effect on Israel computing as well. I heard some time later that the people that - I think it's Bezek, the aircraft company there - introduced time sharing into their computer facilities as a result of our lectures, as a result of having sent people to those lectures, which improved their productivity quite a bit. A large number of the people in the courses were from the military. They were heavy users of computers. One person who was there who ended up on our faculty at Michigan, was Gideon Frieder,and he used to tell people that I taught him computing because he was at that original session. He was in charge of a group in the army. He and several of his people came to those lectures. He was a physicist, but they were using computers, and those lectures helped him to understand what computing was all about. He ended up a professor and chairman, and now dean, of a computer science department, dean of the School at Syracuse. So those lectures by all of our experts had a profound effect on Israel computing. We were invited back year after year to do the same thing with the same courses that were being given at Michigan. So we did it in 1969, 1970 and maybe two or three other times after that. Eventually, of course, the state of computing in Israel was such that they didn't need us to do that any more, they did it themselves. They established computer science departments at the Technion, at the Hebrew University, and Tel Aviv University, and Ben Gurion and they are one of the advanced countries now. So I can't say we started computing in Israel, because of course, we didn't. There was the WEIZAC computer and the Golom that were built at Rehovoth, at the Weizmann, but that represented, as in many places in America in the early days, a few technical

talented people building a one-of-a-kind computer, and what we brought to them was how to do computing on a large scale with many people, in a productive way, and I think it helped not only, as I have mentioned, the aircraft industry and the army, but it helped the industrial development of a computing industry in Israel.

Another important series of events in Israel computing came in 1971, actually in 1970. Israel had for some years what they called economic conferences. They brought in industrial leaders, primarily from America and Europe, to advise Israeli leaders on how to improve the economy, how to encourage investment in Israel, and so on. In 1970, they established a number of world-wide committees in different disciplines - textiles, furniture, computers and so on - to really continue between the economic conferences this kind of activity. I was invited to join the North American Computer Committee of the economic conferences. I think the chairman was Frank Lautenberg, who is now the senator from New Jersey. He, in his role as president of ADP, a very important computing company, was leading this activity. We met and decided that the best thing we could do for Israel would be to establish a Jerusalem Conference on Information Technology, and this was, in fact, held in 1971. The idea was to bring in American experts and to give papers, but also to ... the theme of that particular conference was developing countries, so we invited people from primarily African countries - we had representatives from 13 different countries - to not only learn from American and European lecturers, but in fact to learn how developed Israel was so that Israel would become a local developed country and be a supplier of technology to these African countries. It was very effective, lots of good contacts were made in Nigeria and Kenya and other countries, and we immediately made plans to do another one three years later in 1974. The 1974 conference was held, but it followed the 1973 Yom Kippur war, after which there was essentially no contact between Israel and Africa. It fell apart. We tried the developing country thing again, but nobody came. So this series of conferences which was intended to be every three years, is now lengthened to six years. We had conferences in 1971, 1974, 1978, 1984 and we had one in 1990. They have turned out to be important conferences to educate many Israelis what is going on in the world, and to bring a lot of Americans and Europeans, and some Japanese to Israel to know what Israel is doing. And they've been very important. I've been involved in the planning of all of them. I've chaired sessions, I've run entire tracks of sessions in various conferences, and every one of them has been a very pleasant experience and another opportunity for me to go to Israel, which is always a pleasant experience, too. In the process, I have made many friends in the Israel computing community, and it is always

satisfying to feel a part of that community, and to feel that I've done some good to them.

E. GALLER: Tell about your trip to the Technion to help them select a machine.

B. GALLER: The people at the Technion were trying to decide whether to acquire as their next machine an IBM 360/65 or a Control Data 6400 or 6500, and their main committee at the top level was evenly split, four and four, and they couldn't make a decision. It turns out that Herman Finkel, a cousin of ours, was Vice-President for Academic Affairs at the Technion at the time. He was originally a professor of agricultural engineering, and was serving as the vice president at that time, and he suggested that I be brought in as a consultant to give them advice on this topic. I arrived there, and for a week I was in a room at the Technion in which every half hour a different professor would come in to tell me his needs. And one of my vivid memories is that in between every once in a while a man would come in with some grapefruit juice to keep me alive. It was a very intense week. I spent the final day of the week writing my report, and I met with the committee that afternoon and I told them that in terms of the available software, there was no question that the IBM computer would better satisfy the needs of their faculty, that I understood that they could use the Control Data machine as a backup to their military, which had Control Data machines, and that was one of the considerations. I said, "It's up to you which is the more important criterion, whether you want a computer that is better for your academic purposes and your faculty, or whether it is more important to back up the military. I can't comment on that question." I said, "My recommendation would be either one or the other, depending on which you consider more important." They eventually chose the IBM machine, and I think that was a good choice for them. It was interesting that at that meeting what they had in their hands were copies of my handwritten report, which was about 15 pages, and I had made almost no corrections. I just sat down and wrote what I had planned to write. And one of the people at the meeting asked me if I had recopied that report from a previous version because it was so cleanly written. But, in fact, I knew what I wanted to say, and it flowed very well.

On the way home from Israel, on that trip, I stopped at the University of Nijmegen in Holland. It turned out they had exactly the same question: Which computer to get? I don't think it was IBM-Control Data. Maybe it was. But it was clear, and they asked me to make the same kind of recommendation. The military wasn't involved there. It was clear

again, that the IBM machine would be much better for their faculty's use, and I made the same recommendation to them.

Tape 2/Side 2

B. GALLER: Another important international trip was in 1968 as president of the ACM, when I travelled to Greece to inaugurate the Greek chapter of the ACM. That was a very pleasant trip. I met Andreas Drimiotis, who was working for Unisys in Athens, and, incidentally, through him, Doxiadis, the famous architect, who was in his building and associated with his office. Angeleos - these are wonderful people who helped us see Greece. The following year, or maybe 1970, I went to Chile under a trip sponsored by IBM, to give some lectures in Santiago to mainly academic people. And that was very interesting, University of Santiago. I remember talking to their medical faculty and seeing a little bit of Chile, and that was just a few months after Allende was deposed, and they had high hopes for the Pinochet government, which I'm not sure ever really materialized. Jose Pino, who was a graduate student of ours at Michigan, was back in Santiago at that time, and I saw him there, and he told me he was hoping things would get better. I'm not sure they have, and I haven't heard from Jose in some time. I don't know what he is doing.

Another trip, I believe in 1970, that was very interesting for both Enid and me, was to Australia. I was invited to be the Overseas Visitor that year. We stopped in New Zealand for a few days on the way to Australia, and I was invited to give an all-day seminar and an evening lecture in each of six [Australian] cities. I was mainly lecturing on time sharing, and virtual- storage ideas that were making time sharing more accessible. And I gave lectures in Sydney and Melbourne, Brisbane, Newcastle, Perth, Adelaide. And it was interesting that wherever I went, the telephone company there sent a team of two people to give their side of the story, because they were not anxious at that time to have people connecting into the telephone system -- just as in America, American companies tried to head off some of that by requiring a special adapter between any terminal and the wall jack, they claimed to make sure it didn't damage the network, and so on. Eventually they had to give up on that. I think it was the Carterphone decision of the Supreme Court that made them back off that. But at that time, that had already happened in America by the time I was in Australia, the Australian telephone company was still trying to do that, and so they had their truth squad

following me all over the country, making speeches, declaring that what I was advocating wasn't really right, and anyone who wanted to use a terminal on their system had to give them the terminal to inspect first to make sure it was okay, and so on and so on.

Another interesting thing that happened there was that I tried to give a demonstration while I was in Perth, of our system, long distance, to show people what it was like, but there were no tariffs from Australia to America, or at least to Ann Arbor. There was no way to actually make a connection. They offered to do something to London, and that didn't help me out very much. But now, of course, it is routine.

E. GALLER: Talk now about the sabbatical that we took in 1973 to Newcastle-upon-Tyne, England.

B. GALLER: I was invited to Newcastle by Ewen Page and Brian Randell because of our close connection which started in my 1966 lectures there. I spent the summer there, and Enid and the family were there with me for part of the time. Mainly when I got there they asked me to do a study of their undergraduate computer science program, which I did. It was a very interesting study because I realized how much it differed from our own undergraduate program. In England, as in much of Europe, they expect a great deal more preparation in high school, and in effect, the breadth that we now get through our distribution requirements at Michigan, for example, they assume from high school. So in the undergraduate college curriculum, it is very much specialization. The students in computer science take almost nothing but computer science and mathematics. I reviewed the program for them, and I made some recommendations. They had, for example, one professor who had been teaching an introductory course for many, many years. It was really outdated, but they didn't feel they could tell him that, and I recommended that they do something about that course, which made it possible for them to do that. In general, I found their courses really quite good. I remember one interesting thing, though, that happened. I was once sitting at a terminal doing some work, and a student came up to me and said, "You know, this is most unusual. I've never seen a professor actually sit down at a terminal and do work and stuff." Another thing I found was that they in fact discouraged people from going in at night. It just wasn't the way things were done. People didn't work that hard. They have excellent people there, but I think, as in many places outside the United States, they take the quality of life a little bit more seriously and spend more time with

their families and at home, whereas we tend to be workaholics and go in at night and work.

I thoroughly enjoyed my stay in England. I learned a lot about England. After the family left, I moved into an almost empty dormitory, and met a friend there, a person who became a friend, who is still a close friend, and who has taught me a great deal about India. That's Sriram, who that year married his wife Nidya and later moved to Detroit. He was at Wayne State for a while, and now they are in Chicago, and we are still in touch with them. And that was a nice friendship, mainly because there were 11 of us in a 1,000-person dormitory, and we got to know each other pretty well.

One trip to Europe that I haven't mentioned yet was in 1971, to the IFIP Congress then. I gave a paper there, in Lljublana which, of course, is in the news now. It was a paper reporting on a new course that Bruce Arden and I had developed. That was the Advanced System Programming course, where we took a group of students, typically 15 - 20 students, and ran a project as much like an industrial project as we could, putting a student in charge, and organizing them, giving them a partially defined problem, and they had to complete the specifications and the design, and actually implement the project. We did various different kinds of systems along that line. The BASIC interpreter came out of that project. One of the more spectacular things that came out of that project was the CRISP system. That was in 1971. It was implemented in 1972 for the whole campus, as the on-line registration system for the campus, and is now still in use, and came out of a student project that year. So in 1971, I presented a paper describing that course, and I think a number of people found it very interesting.

In 1974, I already mentioned that there was a Jerusalem conference. I was actually invited to give one of the major talks at that conference, and the following week there was the IFIP conference in Stockholm, and so I went directly there from Jerusalem and gave a talk at that conference. So that was an interesting time also, because while I was in Stockholm on that trip, I remember waking up at 2:00 in the morning to listen to President Nixon's resignation speech. Those were interesting times.

The next major foreign travel that I can recall - of course, there was the 1978 Jerusalem conference - but in 1984, I went

to Japan. That was at the invitation of the National Science Foundation. They wanted to have someone organize a Japanese-American seminar on artificial intelligence. They came to me because I was the Editor-in-Chief of the *Annals of the History of Computing*, which we'll get to a little later, and I was asked to plan a seminar. I said I would be happy to do that in Japan, which they agreed to do, in connection with the expo at Tsukuba the following year.

I went to Japan in 1984, in June, to meet with Professor Moto-Oka to coordinate the seminar. He would be the Japanese coordinator, and I would be the American coordinator. It was most interesting. I remember at the first meeting he asked me what I thought about including the people who had begun companies in artificial intelligence, and I knew that there was some strain between the people who had started companies and those who were still doing theoretical work. I wasn't quite sure which side he was on, but I said to him, "Well, that's a very interesting question. What do you think?" And he said, "I don't think we should include those people." And I said, "I think you're right." And we got along fine after that.

E. GALLER: What was his position in Japan?

B. GALLER: He was a professor at the University of Tokyo, I believe, very influential in artificial intelligence, very respected. Unfortunately, when we actually gave the seminar a year later, he was too sick to come, and he died shortly after that. But it was a privilege to have known him and to have worked with him. So I met with him and talked to a few other people, worked through the American Embassy in Tokyo, in particular, and made the plans. Then I went home and during the following year got all the American lecturers lined up and the actual seminar agenda arranged, and the following June, Enid and I went to Japan for the actual seminar. Because of the interesting internal politics in Japan (where the people from MITI, who are the industrial ministry, and the people from Monbushu, the educational ministry, don't get along with each other, and the Tsukuba Expo was a project of MITI and the seminar was really a product of Monbushu because that was the counterpart of the National Science Foundation), they had to cooperate, but they didn't like cooperating, so the opening ceremony was at Tsukuba, and the rest of the conference was at another site. It turned out to be the conference center at the Fujitsu plant near Numazu, and then for the final day and the news conference, we went back to Tsukuba, to hold a news conference

about the results of it, and that was the compromise that we made between the two organizations. It was very interesting. The news conference was interesting, answering the journalists' questions. The seminar itself was productive and useful and brought a lot of American experts together with the Japanese experts. One thing I remember which shows something about the Japanese culture. I ran the whole seminar as the coordinator. At one point I asked a question of the group, and there was no response at all from the Japanese, and finally an American answered something. And later, during the coffee break, one of the Japanese professors in the audience came up to me and said, "It is not surprising that you did not get an answer from any Japanese persons. By nature we do not volunteer answers as individuals. It is not becoming for anyone to look better than his colleagues. If you want an answer from the Japanese, you have to pose the question and then have a coffee break so we can get together and decide on the common answer, and we will pick who will then respond to you after the coffee break." That's very interesting. The seminar was successful. I was invited afterwards to write a preface for the issue of their journal that reported on the seminar. I wrote a preface which essentially said to the AI community that they need to explain better to the world what they are about and what it is that makes something artificial intelligence, because the rest of the world is very skeptical. It turns out, I saw a reference in a paper just this week to something called the "AI winter," that is a recent period of several years now where AI is simply not being funded and is out of favor because they really haven't produced very much of what they have been talking about and promising for so long, and that's partly what I was warning them and others about at that time. But that was a very interesting and productive trip. We spent a week in Kyoto afterwards, enjoying the beautiful Japanese gardens and scenery and culture, and in general I was very pleased to have been invited to undertake that seminar and to carry it through.

E. GALLER: Tell now about the sabbatical that we had in 1988, at a very interesting time in the history of the country of Israel. We went to Jerusalem.

B. GALLER: Yes. We had always thought that if we did go to a foreign country for a sabbatical, after Holland, that we would like to do it in Jerusalem. And so when the opportunity came up, I corresponded with Amnon Barak, who had spent some time at Michigan a few years before, and whose work was very interesting to me. He arranged to invite me to a sabbatical in Israel as a Visiting Professor at the Hebrew University. We arranged to go for half a year,

actually five months, January 1, 1988, to June 1, 1988. We lived in an apartment owned by the university right across the street from the Givat Ram campus, the science campus, which was very underpopulated. At one time it had 16,000 students; now about 2,000, because the humanities and social sciences are now housed on Mount Scopus. I had an office in the Computer Science building, which was next door to the National Library, and my office was next on the one side to Shmuel Peleg, who was involved with networking, on the other side was Amnon Barak. They made me a full member of the department, and I worked with Amnon on an interesting paper concerned with database organization for systems that might scale up to thousands of computers. In the process, I learned a lot about Unix and used Unix the whole time I was there. We subsequently published a paper as a report in the Michigan series and the Hebrew University series. We haven't actually published a paper in any journal, but I think he is working on a revised version with one of his students.

It was a useful sabbatical professionally. I worked with a number of committees of the Hebrew University and other universities on some of their computing problems, and thoroughly enjoyed the time we spent there. As Enid says, it was a very interesting period to be in Jerusalem, because the Intifada had begun three weeks earlier, and while we were there, there was increasing tension, and we were able to be with the Israelis while they agonized over what was happening. The Jerusalem Post reflected a number of views on both sides of the problem, and I think we came away with a much better understanding of what is going on, and with as much hope for resolution of the problem as anybody else in Israel. We visited our families there, and from them we got a number of different views as to what the solutions should be, ranging from the extreme left to the extreme right. So we learned a lot, and it was a professionally satisfying and productive sabbatical.

We made a side trip to Egypt, and while in Cairo, I visited the American University there. It was a little sad. They had received a number of workstations from American companies, and they were all sitting there covered up and covered with dust. Nobody seemed to know how to use them, and nothing was happening. And it was sad to see that, because they have great needs, and so much could be done. I hope that if relations warm up better between Israel and Egypt that some of the Israeli technology can be shared with the Egyptians.

E. GALLER: Talk now about your association with the professional journal, the *Annals of the History of Computing*.

B. GALLER: Okay. Around 1978 I got very involved with the History of Computing Committee, with AFIPS, and got quite involved in general with the history of computing. I was always interested in the history of computing, because I got into the field very early, and I knew some of the pioneers, and I knew that I wasn't one. And history has always interested me. Around 1978, 1979, I was invited by the Publications Committee of AFIPS, actually in the person of Aaron Finerman, to consider starting a journal in the history of computing. They had had a meeting of the committee and wanted to start the journal, and they were going to start a search for Editor-in-Chief, and the next time I saw Aaron, I started to recruit him to come to Michigan, and he started to recruit me to be the Editor-in-Chief. And we were both successful. I decided that it was a very interesting and important thing to do, and I agreed to do that. I was on the West Coast soon after that and met with Aaron Finerman and Hank Tropp and learned more about Hank and how deeply involved he was, and had been, at the Smithsonian. And I went to a meeting of the Publications Committee to discuss starting it, and Nancy Stern was there, and Aaron had suggested to me before that that she would be an excellent person to be the Assistant Editor-in-Chief. And when I met her at that meeting, I decided that he was absolutely right. Nancy is a historian. Actually, she is a very broad person. She and her husband write books on assembly language and Cobol and so on. And she turned out to be a very good partner in the journal.

Our first managing editor was Myrtle Kellington, who came over from the other ACM journals to help. Actually, Myrtle had retired, but she came back to help. After a couple of years, we moved the office from where it was near where she lived to New York, and she couldn't follow us, so we got a new managing editor, Mondy Dana. Mondy - actually Rosamond - who had been a free-lance editor and writer, is a most interesting lady, descended from Henry Longfellow and Richard Dana the writers. She was following in that tradition in her interest in literature and writing. And she stayed with us for eight years, all during my tenure as Editor-in-Chief, which was [altogether] ten years. When I finally stepped down from there, Jan Lee took over. Jan is a professor at Virginia Polytech in Blacksburg, Virginia, and he is the current Editor-in-Chief.

I'll go back and talk a little bit more about the journal, but let me say what has happened to it since. AFIPS got on

hard times about the tenth year of the journal, and we decided that AFIPS could not sponsor us any longer, so we turned it over to Springer-Verlag, and unfortunately, the man who championed us at Springer-Verlag and hired Mondy Dana specifically to help us continue the journal, died about two or three months after we switched over there, and the relationship continued, but with difficulties. Mondy was assigned to work on other things. They tried pretty hard, but it was different. And I, frankly, was glad that I had already turned it over to Jan Lee. It was time for me to get out, and Jan has done a fine job. It has now been moved over from Springer-Verlag to the IEEE Publications, or Press, and that is a very recent thing, but from all signs I think they are going to do a fine job of production and marketing.

Now, the journal itself was intended to be, and I think succeeded in being, a readable account of the history of computing, partly articles written by the people who lived through the history and were the pioneers, some article by historians about computing, a few articles about historiography - that is, how to do history. We have an anecdotes section. We had a little bit of reporting on historical events, history events such as what went on at the Computer Museum, and so on. Book reviews and articles in the field of history. We occasionally reprinted articles. We tried very hard to not have more than 20 percent of our articles be reprints, and usually if we did reprint something it was really unavailable in any other form. Sometimes we had occasion to print very old documents which never were published or available. And in general, I have seen people refer to it as "the" journal in the history of computing. I really was very proud of the journal and I think it was one of the major things I was able to accomplish.

Some interesting things that happened which I might note. One of the articles was a description of the organization and structure and use of the ENIAC, by Arthur Burks (Arthur and Alice Burks; it really was a joint article). When we published it, we sent it out ahead of time to 19 different people who had been involved in one way or another with the ENIAC and asked for their comments to be published in the same issue as the article by the Burkses so that we would have a complete historical review of the topic. Most of the people did respond. We got some very fine comments, some very controversial. It was interesting that there were some controversial remarks in the article about the role of Eckert and Mauchly and Atanasoff and so on, and Mrs. Mauchly subsequently wrote an article for us, pointing out things about her late husband. Pres Eckert called me before publication and said, "If you print that article, I'll sue

you." He was very upset about some of the things the Burks had said. Well, Art Burks and I went to the University attorney and reassured ourselves from him that we would be protected by the University. We went ahead and published it. I invited Eckert to submit an article with his point of view, but he never did. Atanasoff subsequently did. And the debate continues. But I think the coverage in the *Annals* was just right.

We succeeded in getting a number of articles written by the people who created the history, clearly from their point of view. And as we know, and as historians know, there are always many sides to things, but you have to establish the record from which future historians will be able to understand what happened. We'll mention later that one of the honors I got was an IBM Triangle Fellowship from North Carolina, the triangle area of North Carolina. I was invited down there to give a lecture to historians on what it was like to do contemporary history. And I tried to help them understand, first, the nature of contemporary history; that everybody is giving you his side, which they almost always believe is "the" story. But you find different people reporting different things, and it was important to get all the different points of view, but to be sure not to believe any one of them. The ENIAC story still goes on. It was very interesting for me to have participated, for example, in a symposium in honor of Atanasoff in 1983, ten years after the end of the trial in which the Eckert-Mauchly patent was invalidated because of Atanasoff's role. It was interesting at that symposium how vitriolic some of the speakers were against Eckert and Mauchly when as far as I could tell all they had done was claimed too much in their patent. Whether they were frauds and cheats and thieves as some of the speakers at the symposium said, or whether they thought that they were just claiming what was theirs, who will ever know? But I tried at that symposium to smooth the waters a little bit and argue that there was plenty of credit for everybody to go around. If they only hadn't claimed what was clearly his, they could have gotten their patent; he could have gotten his recognition. And actually the fault in that particular case was from Iowa State University, that they never did patent his work when they should have.

The history of computing is a very interesting field. I hope the journal continues and succeeds, and now it is time, and they know it - I'm still on the editorial board, but Jan Lee is the active one, and he knows that it is time to move into later years and cover software more than the hardware. We established the rule from the beginning that we wouldn't accept articles on anything more recent than 15 years before publication, the idea being that that is a

moving target, and as the years go by, more and more parts of the history will become eligible for consideration. Well, 15 years from when we started took us back into the early hardware days, the one-of-a-kind computers. Fifteen years before now takes us into the era of rapid development of software, and we're going to see more of that in the journal from now on.

We also covered events and the history of computing in many other countries. One significant article we had was from one of the Polish mathematicians who was involved in breaking the code on the Enigma during World War II. He wrote an article, and unfortunately, he died soon after. But we have his story in the *Annals*, exactly how he and his colleagues broke the code and later turned it over to the British. It's interesting that in all the stories that finally came out of Britain after they declassified all that material in 1976, none of them really gave the Poles any credit for having done any of this work. They mentioned the Poles, but only in passing, and gave the impression that the British had done everything. It is clear that the Poles had broken the code of at least the early Enigmas, showed the British how to continue that work, and then, of course, when the Germans made the Enigma more complicated, the British invented some of the computers that they used to help break those codes. So both parties contributed. But I'm glad that we had a part in at least recognizing the role that the Polish mathematicians did play in the early days of World War II. That is an interesting side of the history of computing, that it helps us even for fairly recent history to help break down the gaps in our knowledge and really understand what happened.

I should say that I have mentioned Mondy Dana, Jan Lee and Hank Tropp, but I should mention at least a couple of other people who have been very helpful and contributed so much - yes, Nancy Stern as well. But Jean Sammet and Aaron Finerman were always very involved and very helpful in the *Annals*, along with many other people on the editorial board, such as Bernard Cohen from Harvard, Bryan Randell from Newcastle, Wlad Turski from Poland. We had an international editorial board, and everybody contributed. It was very, very satisfying.

Another person who was on the board, but with whom I had much more interaction in a different way was Arthur Norberg. When we, the History of Computing Committee of AFIPS, got involved in helping establish the Charles Babbage Institute, and I should go back and explain that a little bit, Norberg eventually became Director of that

Institute, and I am still a member of his Board of Directors, and we have had a great deal of interaction that way as well.

The Charles Babbage Institute was the inspiration of Erv and Adele Tomash. Erv had become fairly wealthy as president of Data Products Corporation, and he decided, because of his interest in the history of computing, to establish the Charles Babbage Institute, which would be a center for research in the history of computing and an archive for people's papers. About the same time, I should mention, the Computer Museum was started in Boston. There was a little bit of competition between the two organizations until it sort of got settled out that one was a museum and collected artifacts and the other was an archive and a center for research on history. And then things settled down because there was no longer any real competition, they had different roles. The nature of the Charles Babbage Institute was somewhat controversial in the sense that we on the AFIPS History Committee had been planning to establish an archive, and it seemed that there would be real competition between what the AFIPS History Committee wanted to do and Tomash's Charles Babbage Institute. That flared up at a couple of meetings and became a real difficult time. In fact, that was the time when I resigned from the AFIPS History Committee because there was some very poor handling of people, very insulting treatment of Jean Sammet, and I resigned in protest. I came back a couple of years later when it got smoothed over. But the Institute was established. We finally agreed that it shouldn't just be a center for history, it should be the archive. If we weren't going to start the AFIPS one, then the Charles Babbage Institute had to do the archival role. And thus we sort of compromised and said, "Okay, AFIPS will put money into the Charles Babbage Institute, but they've got to do the archival thing that we wanted in the first place." Thus it did evolve. Paul Armer was the first director, getting it started, and then probably - I don't remember too well, but I think it was because of his health problems - he dropped out of that, and there was a search, and Arthur Norberg, who had been very active in the historical archive type thing somewhere in the Berkeley area, was brought in, and there was a big search for where to put the Charles Babbage Institute as well. And it came down to two candidate sites for the establishment of the Charles Babbage Institute - The University of Michigan, partly because I pushed so hard to get it there, and the University of Minnesota. I really had no doubt, once it came down to those two, that it would go to the University of Minnesota because Tomash was an alumnus of Minnesota and felt very close to that university. So it was established there. I was appointed as one of the AFIPS representatives on

the board, and that has remained even though AFIPS has faded out of the picture, and thus became close friends with Arthur Norberg. I think I have contributed to running the Institute, to helping it create an archive, and it is really quite successful now.

Part of the problem that Norberg faced was always raising money. He didn't have a very large staff, but he always had to work very hard to raise the money, which kept him from doing some research in the history field. He wrote some papers and did some good work, but there was always that burden on him to raise money, even though Tomash had contributed quite a bit and was continuing to contribute. Eventually it became clear that it was desirable for the University of Minnesota to really take over the Institute and they have done that. It has become part of the university. They have established a chair in the history of computing, which Norberg fills, and so the role of the Board of Directors has changed, because now we are not an independent organization at arm's length from the university; we are really part of the university. So the Board of Directors has much less influence, but that's okay, because it is a fine university, and we really don't have any problems with it. So that is an ongoing institution. I'm glad I had a part in setting it up, and I have a role still.

Two other people I should mention in this whole context of the history of computing and who were members of my editorial board and very active. One was Walter Carlson, who was for many years a very important person in IBM marketing, and was chairman of the AFIPS History Committee. He did a great deal to get everything moving in the whole history field. I should mention that one of the things that Walter sponsored was well over 200, maybe 250, interviews of pioneers, a joint sponsorship between AFIPS and the Smithsonian. We subsequently had a lot of difficulty getting the Smithsonian to release them for public use, but we finally did that, and these interviews are interesting and available, and many have been transcribed by the Charles Babbage Institute and are in their archives. Walter continues to be very active, although he's retired now.

Another person who was a dear friend and involved in history activities was Carl Hammer. Carl worked for Unisys for many years and is now retired and one of the real gentlemen of the field. He's the one, through his connections with Unisys in Greece, who arranged for me to go to Greece back in 1968 to inaugurate the Greek chapter.

One more person I remember from the editorial board of the *Annals* is Eric Weiss, who worked for Sun Oil Company, also retired now, living in Honolulu. Eric handled for many years the biography section of the *Annals*, and managed to find very interesting biographical stories and memorials, it turns out, for people in the history field.

And still another person is Heinz Zemanek. Zemanek is probably one of the most respected computer pioneers in the world. He was with IBM Austria, head of their Austrian research laboratory, a prolific writer, a gentleman, retired now, but whenever he comes to the United States it is an occasion, and he is honored for it.

E. GALLER: Tell about your connection with ACM.

B. GALLER: I started, like so many other people, attending ACM chapter meetings in Detroit and eventually in Ann Arbor, very early, in the 1960s. I was always interested in it, and felt that ACM was an important organization for sharing information. When the Communications of the ACM started in 1960, Al Perlis was the Editor-in-Chief, and he invited me to be his University News editor. My job was to collect news about what went on at the various universities, and so that was another involvement I had with ACM. ACM actually started in 1947, so it was ongoing before I joined. My membership card indicates that I joined in 1958. So since I got into computing in 1955, 1956, I realized quite early that I should be a professional person in the field, and I joined ACM. So by 1960 I was already helping with the communications, and I eventually became chairman of the Ann Arbor chapter and was elected as a Regional Representative to the ACM council in 1966. I was asked to run for Vice-President ...

Tape 3/Side 1

E. GALLER: You were talking about your participation in ACM and the fact that you were invited in 1966 to be vice-president of the organization.

B. GALLER: Yes. I was subsequently elected to be vice-president, with Tony Oettinger, and two years later I was

elected president of the ACM. This was 1968, also for a two-year term. It was a turbulent time. This was the Vietnam era. There were great upheavals, people arguing that the organization, like many organizations, should take political stands. We felt that we should not take political stands on behalf of the organization. Individuals could. But we had many severe arguments at the council meetings about this. I learned a great deal about how to run a meeting and what *Robert's Rules of Order* are all about, and that has come in very handy in subsequent meetings throughout my life. It was a very fine experience, though. I met, as head of this organization, heads of many other important organizations, and was catapulted into the public eye a great deal more, and I think that had something to do with my invitations in subsequent years to international events and lectures and so on. It was an important contribution, I think, to the professional field of computing, and I am very glad that I had that opportunity. I served two more years on the council as past president, actually a total of 10 years altogether on the council, and since then I have had less to do with the organization, but I have served on some committees, such as I am currently chairman of the Turing Awards committee. I actually had done that about 15 years before as well, but now I'm chairing it. Last year the council changed the award from $1,000 to $25,000, which makes it a little more important, perhaps, who one chooses, although it was always important, because it is the most prestigious award ACM has. There are very fine candidates, we are in the process right now of evaluating. We have a five-person committee.

I think ACM has been a very important organization. When I was president we had about 26,000 members. I think there are perhaps 75,000 members now. And it has always played an important role, especially its publications - Computing Reviews, the Journal of the ACM, Communications of the ACM, Computing Surveys. Computing Surveys was started while I was president. I supported it very strongly, and it has continued to be an excellent journal. I am involved with Computing Reviews as well, as a category editor. I've mentioned that in the mathematics area. In general, I support ACM, I just haven't had a great deal to do with its day-to-day activities in the last few years.

E. GALLER: I'm curious to know what that organization was like when it first began, because computers were almost not around at that time. What was the organization set up to include?

B. GALLER: Well, we've seen the minutes and the proceedings from the very first years. It was a small group of those who knew something about computing, and the first year there maybe were fewer than 100 in the country. They came together to exchange information - at that time, everybody very freely exchanged information - and software. The sale of software as an industry didn't really get going until 1969. Until then people freely gave away their software. It was prestigious if someone thought enough to use your software, that was great. And in the early days, the ACM meetings were always at universities, and it was a very academic organization. It became too large after a while to be hosted at universities, and I think in the late 1950s it began meeting in larger cities in hotels, and then later in convention halls, and so on. The ACM national meetings were places where you could give a paper, there were exhibits, and so on. Actually, in the last few years they have cut out the national meetings because, just like the AFIPS national meetings, there seems to be less of a need for general purpose, very broad meetings. Most meetings these days are very focused on a specific topic, and the national broad meetings simply faded away. ACM does have one national meeting called a Computer Science Meeting, which was, in fact, started during my term as president. The purpose of that meeting is to help people find jobs. It is something like the one where I got my first job at the Mathematics Society, a meeting of the same kind. So that one continues, but the national meeting otherwise has just sort of faded away. There are many special interest groups within ACM, and those are flourishing. In fact, it is sort of a strange arrangement. Each special interest group gets to keep the money it raises from its own activities. Some of the special interest groups are far wealthier than ACM itself, and there have always been regrets that we didn't organize those special interest groups in such a way that their profits funnelled back into the central organization. But it wasn't done that way. In fact, during my tenure as president, that was one of the biggest issues, Bylaw Number 7, which defined special interest groups, was a constant item on our agenda at the council all through my period as president. Jean Sammet, it turns out, was very active on behalf of the SIGs, special interest groups, so that was another time when I got to know Jean very well. In fact, she became ACM president, I think a couple of terms after me. Walter Carlson was my vice-president; he became president and Tony Ralston was his vice-president. The tradition had started that the vice-president would often become the president, and Jean Sammet was Tony's vice-president and became president. So the ACM has a great many good things going, and some of them actually started during my term. It was very interesting.

One other that started then was that we put together a code of - not a code of ethics, but a set of guidelines, ethical guidelines. We felt we had to avoid a code because when an organization has a code of ethics they are expected to enforce it, and that meant, in particular, if someone lost his job because he stood up for an ethical position, like whistle-blowing, the organization had better come to his rescue and support him and so forth, and we weren't in any financial position to do that. So we promulgated a set of guidelines which meant that we weren't in a position to enforce them. A few years ago the ACM adopted a code of ethics, and by implication, I presume, they are now enforcing them and standing behind them.

DATE: 10 August 1991 LOCATION: Ann Arbor, MI

E. GALLER: Could you now tell about your participation in NCAR?

B. GALLER: Well, NCAR is the National Center for Atmospheric Research in Boulder, Colorado. It is a facility of UCAR. UCAR is University Consortium for Atmospheric Research, which is 51 universities collaborating on atmospheric research. One of the reasons such consortia are set up is to enable them to pay a higher scale to their employees than government employees, but they largely do government work. NCAR is a beautiful facility in the mountains just on the edge of Boulder, and they have several projects. One is a project which sends high balloons up to measure atmospheric data. They also have, and the reason I got involved, a very large computing facility which participates with other computing facilities around the world in global projects of various kinds. I was on their Computer Advisory Board, which had the responsibility for allocating time on their computer because they have always had one of the largest computers around. And when the National Science Foundation in particular gives grants to atmospheric scientists, it expects them to use part of the money for time on the NCAR computer. The role of our board was to evaluate the proposals that these scientists made and allocate computer time to them. I was on that for three or four years. I was chairman one year, and it was thoroughly interesting and delightful work. I learned a great deal about atmospheric science, which has stood me in good stead since then. I have met some very interesting people also in the atmospheric science field. And, of course, I got to know Boulder a little bit. That was before the family moved there. But it is a very nice place to visit.

E. GALLER: Tell also about your participation on the Computer Science and Technology Board of the National Academy of Sciences.

B. GALLER: That was a board which actually didn't do very much. We met a few times to discuss the government role in supporting computer science and things like that. I do remember that one panel I was on met to allocate travel funds for people who wanted to go to one of the Jerusalem conferences. We had applications for travel funds, and we had to decide who would get them.

Another group that I participated in was the NASA Computer Science Councils for a couple of computer science institutes that NASA had set up. They had originally set up one at Langley Field, and I was on the committee that helped establish that. They concentrated primarily in numerical analysis techniques to support computational fluid dynamics, which is what NASA people are mostly interested in. Later, I was asked to come back in again and help set up a computer science institute, which would spend a lot more time on computer science. And that was established in Moffet Field at NASA Ames in California, where Peter Denning ended up as Director. And then, a third one ...

E. GALLER: About what years were those?

B. GALLER: The one at Langley Field must be 20 years ago. The one at NASA Ames, about 10 years ago. And starting at that time, I was asked to serve on the advisory panel for that center, along with Bruce Arden and Bill Gear and Tony Hearn and several other people. We set up another center in the Norfolk, Virginia area, and those continued, and we used to visit them a couple of times a year and evaluate their program and so on. Then NASA decided to set up a third one at Goddard Space Flight Center, and I was in fact asked to chair the committee that would organize and plan that center. It was an unusual one in that we made it very closely tied to the University of Maryland. The director would have a half-time appointment at Maryland and half-time at Goddard and they would reach out especially to young assistant professors in other universities and support them in their work at their

universities. It was a very successful plan. We argued it through NASA all the way up to the top and got it established, and I was on the first Executive Committee of that, and we participated in the first awards. Then we recruited a regular director, and I dropped off that panel. But that was a very satisfying exercise in leading a committee primarily of atmospheric scientists and space flight people to establish a center in one of the NASA centers. And it was very successful, still going on.

E. GALLER: What year were you involved in that?

B. GALLER: In that last one, at Goddard, I think that was middle 1980s, approximately.

E. GALLER: Talk now about your involvement with networking.

B. GALLER: The CSNET network was established about 10 years ago with NSF money, and I was invited to help establish that network, and I was not really very deeply into networks at the time, and I declined the offer at that time. Tony Hearn had asked me to do it. But I watched what they were doing - Dave Farber, Larry Landweber, and Tony Hearn were the main movers there. And it was interesting that it was primarily set up to connect up computer science departments, but those which were not in the high-powered elite group that ARPA was supporting (ARPA, the Advanced Research Projects Agency of the Department of Defense). ARPA, now called DARPA, had set up a very innovative national network to support the people they were supporting with very large grants. And they developed packet-switched networks, which are now used everywhere, and they contributed a great deal to the networking capability of the country, but it was for those who were big science, big projects, lots of money - places like MIT and Stanford and Carnegie-Mellon and Berkeley. And we at Michigan had never really felt that we wanted to get that involved with the big money projects. We did have one ARPA grant in the early 1960s to look at interactive computing and time-sharing and that sort of thing, with Frank Westervelt and Bert Herzog running it. It was pretty successful, but not too successful, and we didn't feel the need for getting that involved in ARPA.

CSNET was intended to help those who were not in the inner circle achieve networking capability, and it was very

successful, especially in the sense that NSF gave money for five years, each year less money, with the mandate that CSNET better become self-supporting if it was to continue. And in fact, it became self-supporting, and about the fourth or fifth year, they invited me in again to join the Executive Committee, and I agreed and began to participate. Of course, as NSF money ramped down, they needed a shell organization in which to raise money and run the organization, and rather than form a corporation itself, the CSNET Executive Committee - this was just before I came to join it - and NSF agreed with UCAR, which I mentioned before as running the NCAR facility, agreed with UCAR to host as a project the CSNET network. And so once again, I was travelling to Boulder to the NCAR facility where we kept meeting with the UCAR people, and it was again a very interesting experience. I learned a great deal about networking. After a couple of years, I was in fact elected chairman of the CSNET Executive Committee.

Now, a kind of parallel development took place over the same period of the last ten years, and that was the growth of Bitnet. Bitnet started out as simply a leased line connection between two universities. Ira Fuchs was the main person to do that in Princeton, and he connected Princeton to CUNY, City University of New York, so they could communicate. Ira was not so much computer science as computer administration, and the idea was that the administrative centers, the data processing centers of the universities, would be able to be in touch with each other by networking. It turns out that unlike Michigan, most universities had very separate computing activities. The computer science department was one kind of activity, teaching courses and doing research and so on; providing computational ability to the rest of the campus generally fell to the data processing center or the computing center or whatever of the university. At Michigan, we did separate administrative computing from academic computing. We had two different centers. But we tied the academic computing center very closely to the department activity, largely by joint appointments such as myself. I've always had a joint appointment with the Computing Center and the department. At most universities they were quite separate. They didn't like each other; they didn't talk to each other. I remember one meeting at the NSF when I was making a suggestion how things should work at universities, and they said, "Well, but that's at Michigan. You can do that. Nobody else can." And this was an accepted fact, that there was this great separation at every university.

So while CSNET was connecting up computer science departments, the Bitnet group began to connect up computing

centers, and they had quite different philosophies. CSNET established a central computer, and if people wanted to send messages by electronic mail, they would send them to this central computer, which would then send them out again to the target computer, and everything went through the central computer. On the Bitnet network, pairs of universities got leased lines to connect them, and it spread out like a tree, and the requirement was that if you wanted to join Bitnet, you had to agree that somebody else could connect to you and thus continue the tree. And every computer on that tree-like structure had to agree then to pass along messages which were not intended for it, and they would keep tables indicating who they were connected to and who they could pass messages on to further. So it was a very different technology, a very different group of people. But on the national scene, both represented universities. So when I was chairman of the CSNET Executive Committee, we began discussions about merging Bitnet and CSNET into one organization that could better represent universities, and we made quite explicit the goal of urging and facilitating at universities the bringing together of the two kinds of people on each campus so that networking on the campuses would not boil down to who was the dominant factor, the computer science department or the data processing center. We wanted to try and encourage it, and in fact, we did. After we merged, we said, "From now on we are not going to have a Bitnet representative and a CSNET representative on your campus. You give us one representative who is going to be your contact at the university, and then you get your house in order on your campus." And this has largely happened now, and was a very positive force.

In order to effect the merger, we set up a committee of three people from the CSNET community and three people from the Bitnet community. And I was on it, of course, with Larry Landweber and Dave Farber and Ira Fuchs and Marty Solomon and Ken King. Well, they had changing membership over the year or two that it took. But we did negotiate a merger, and we did merge. Bitnet had, a year or two earlier, in fact, formed a corporation on its own. Because of tax purposes and tax exempt status and everything else, we took that corporation as a shell, in effect threw out the Bitnet bylaws, put in new bylaws and changed the name to form a new corporation, and we call it now, the Corporation for Research and Educational Networking (CREN). And I have been on the board of directors; I have been chairman of the board of directors. Ira Fuchs has been president.

We had a study by some outside consultants about our operation and our structure, so we probably will eliminate my

role, the chairman of the board of directors and put more power and authority in the hands of the president so that things won't fall through the cracks so much, where he was assuming that I would do something, and I was assuming that he would do something. It will be a more streamlined operation. We will hire an executive director of the organization. We have not had any paid staff yet. We've been subcontracting to EDUCOM, and through them, to BBN - Bolt, Beranek and Newman. And that has caused some problems about the relationship between the subcontractor and the contractor. We're trying to straighten all that out. In any case, CREN is now an ongoing organization. We still have the CSNET technology and the Bitnet technology are still quite separate. We have decided to phase out the CSNET type activity, because there are some commercial organizations now - I mentioned ANS, Advanced Network Systems, in connection with Merit and IBM and MCI, a non-profit organization. ANS provides the same kinds of services that CSNET provides, and there are a couple of others that do that as well. So it doesn't pay for CREN to compete with them. We are going to phase out the CSNET-type operation, we are going to beef up the Bitnet-type operation, but probably change the Bitnet technology over to something more like the CSNET technology. In any case, it has been a most interesting experience. I have learned so much about networking, and met a lot of wonderful and interesting people, very dedicated to the work, all volunteer, of course. No one has ever been paid for any of this networking stuff, or any of the professional activities that I take on.

People don't understand this sometimes. They say, "How much do you get paid for all this work you're doing?" And when I tell them that it is all volunteer, people in other professions often don't understand that. They wonder ... they're not used to, except maybe pro bono legal services, but not spending a lot of time in national organizations, which do, in fact, take a lot of time. They often ask in wonderment how come we're doing this. But we all benefit from this, the whole community and discipline, the knowledge of what's going on, understanding, and it is just part of our culture in our field.

E. GALLER: Could you talk about your administrative roles at the University?

B. GALLER: I've had, of course, a joint appointment with the Computing Center and with the academic departments since 1959, so I have had some administrative role in each of those branches. At the Computing Center, I have been

an associate director. Generally, I haven't supervised people, although there was one semester when Bob Bartels was on leave in Munich, and I was acting director. I remember that period especially because there were several things happening at that time. One was the way we allocated money at the Computing Center. We were charging for time on the computer, and the way we allocated money was that the Computing Center would look at past usage by every faculty member, and allocate funds based on how much they had been using. And then, of course, if someone new came in, we would give them an allocation, and after that there would be a past usage arrangement with that person. But we decided right about that time that as the growth of computing on campus increased, we didn't feel we should be making all those decisions as to who should use the computer. We thought the deans and department chairmen should be doing it. And so Bartels and I began to actively lobby with the Vice-President for Research, who was our boss at that time, and others that they should take over this responsibility. We got a lot of resistance from them, because the department chairmen didn't want to have to make that kind of decision, sometimes having to decide between the interests of their own faculty members. But we finally got that done.

E. GALLER: Now, again, what year approximately?

B. GALLER: Well, this was around 1960, and I remembered that it was happening about the time I was acting director, because I was very involved. I was not an associate director at that time, but I was really the only faculty member associated with the Computing Center, other than Bob Bartels, so it was, I guess, natural to ask me to be acting director when he went on sabbatical. Bruce Arden was very involved, but he wasn't a faculty member at that time. That was one aspect of my acting directorship that I remember very strongly.

The other was a kind of an amusing incident. I was approached by someone at Purdue University. Purdue had decided - I guess the Indiana legislature had decided - that they were going to build up their computer science department very rapidly. And they got a great deal of money. Somehow, the number $600,000 come to mind, which, of course, at that time was worth a lot more than it would be now. And I was approached by a senior member at Purdue. I was an assistant professor at the time. I think my salary was about $4,800, and he offered me ...

E. GALLER: Excuse me. You'd already been promoted once, because you came here as an instructor.

B. GALLER: I came here as an instructor, with a salary of $4,500 in 1955. And he approached me with an offer of a full professorship with a salary of $9,000 - about double my salary. And I didn't really want to go to Purdue. I liked Michigan. But it's pretty hard to turn down a full professorship and double your salary. I remember going to George Hay, chairman of the mathematics department at the time, and saying, "I don't really want to go to Purdue, but it's hard to turn this down. What I really want - I don't know if Michigan can match that - but what I really want is some sign that Michigan wants me to stay." And the reason I remember it as concurrent with my acting directorship of the Computing Center was that George Hay was frantically trying to reach Bob Bartels in Munich, and it was Oktoberfest time there, and every time they got an operator on the phone there she sounded drunk, or couldn't find anybody or whatever, and it was hilarious. But Michigan did come back with an increase in salary - not too much - but a promise that they would consider me for associate professor and tenure the following year, which was a lot less than Purdue's full professorship on the spot, but I said, "This is a sign that Michigan does want me." And I stayed, and I am very pleased that I did stay. But it was sort of an amusing illustration of how some universities were trying to get into the computer science business rapidly, by buying people they thought were stars.

I stayed with the Computing Center as associate director, in general heading up various projects or doing studies; in general, acting as an interface to the faculty, and participating in the management, for example, on Finerman's Committee A and so on, until quite recently. In the past couple of years, my participation has gone down quite a bit, especially with the reorganization into various units of ITD, and they asked me if they could cut back the amount of money they were putting into my salary, and so I agreed two years ago that it should go down to one-quarter time instead of half-time, and we concentrated it in the summer, just to keep the appointment clean. And then this year, because of the shrinking budget, they came to me and said, "We really think that we need to save your money. Would you agree to reduce your salary to zero?" And I said, "Well, I'm retiring in two years. I don't think it's unreasonable. I haven't been contributing that much, but I want an office continuing," - which was what I was going to ask for on retirement, anyway - "some secretarial support, and so on," and they said, "Sure. That's fine. We'll do that." And so that's the situation now, that most people at ITD, which was the Computing Center, still regard me as

being part of the operation, and the family, and so forth. I'm not on the payroll any more. We are starting, for example, right now, a major proposal which I initiated for a distributed conferencing system, and I told them I am not going to spend a lot of time on it. I initiated it in the sense that I said, "You guys are doing distributed conferencing in the small. You ought to put a proposal in, and I'll help," and so on. And I am going to be indicated as an adviser to the project, so I'll have my hand in what's going on there, but the formal connection, I think, is now finished, and it's an end of an era, I guess. But it's been nice all along. And I will be retiring - we'll get to that later, I guess - from the University in a couple of years, so it's not so bad.

So that was the one branch of my appointment, the whole Computing Center side. On the academic side, I've already described a little bit how our CCS department was formed and then merged into Engineering and so on. I've always taken an active role in the governance of the department, and I've always been interested in how people organize their affairs, and so I've been on departmental Executive Committees very often. In 1973, when Bruce Arden went to Princeton, and we needed another chairman, I agreed to be the chairman of our department, and I continued that until 1975, when I became Associate Dean of LS&A. It was an interesting time to be chairman, because there were some pressures to be more like a regular computer science department, but I still felt that we had something rather unique in our interdisciplinary structure. Maybe we should have done more in the standard computer science field, but we were a very small department, and we had Holland and Burks who were unique in their interests, and I encouraged them.

In 1975, which was only two years of a longer appointment, I gave up that chairmanship in order to become Associate Dean for Long-Range Planning of LS&A. Billy Frye, who is now the Vice-President for Research at Emory University, was dean at that time, and asked me to join his group after Sam Krimm from the Physics Department finished his term as Associate Dean for Long-Range Planning. I actually had two two-year terms in that role. The main idea for long range planning was to continually monitor the effectiveness of our departments, and so I organized - Sam had started it, but I formalized the process of - departmental review. We set up a structure of first determining who a visiting committee should be, which is not always easy, because you don't want to pack it with friends of the department, but you want to get knowledgeable people. And so we needed to get advice on who the

people were and so on. They would come in and review the department, and very often they would give us very helpful insight, which you can't get except by bringing in experts from more than 50 miles away. And we laid the groundwork for eliminating, for example, the geography department, which ended up controversial later. It was actually accomplished after my term was over, but it was clear in this case that the geography department consisted of seven or eight people whose interests were really in other departments. Geography is important, but the individual professors didn't have geography as their central interest. It was peripheral to their own interests, wherever they came from, and the number of students they had was going down, year after year. And it just didn't seem worthwhile maintaining a department. So that's the kind of thing that we looked at.

I also participated, of course, as a member of the college Executive Committee, which had to do with a great many kinds of problems, ranging from macro problems of departmental structure and departmental effectiveness down to individual promotions and appeals from promotions and allocating positions for recruiting, and most of those were extremely interesting activities. One particular event comes to mind, and that was when Harold Shapiro became Provost and Vice-President for Academic Affairs. He, more than anybody else, paid attention to the kinds of promotions and non-promotions that were made. And he came one day to meet with our Executive Committee, and he said, "I've been studying the recommendations that you have been making, and I'd like you to explain to me why this person is being recommended, and why this one is not." and so on. And that stunned us, because that had never been done before, and yet it tightened things up some.

E. GALLER: Because in the past the recommendations of the Executive Committee had just been accepted by the academic vice-president?

B. GALLER: Pretty much rubber stamped. It wasn't that we weren't very careful, but it doesn't hurt to be asked to defend your decisions and sharpen them up some. And it was a good feeling. I mean, we really appreciated the fact that somebody up there cared and took the time to study them. And I've heard this from other deans and associate deans at other colleges, that they also very much appreciated Harold's efforts in this direction and the fact that he cared so much.

Another thing that we instituted. We found that departments, when they made recommendations, were having a very hard time ever turning down a friend. And they were counting on the college to turn down a promotion recommendation when they knew that it really shouldn't have been recommended because the person wasn't doing enough, but they didn't have the heart to turn them down. We instituted a policy that said if a department recommends not promoting someone to the tenure position, we would guarantee the position back to the department. If they recommended someone and we turned that person down, we would not guarantee that position back to the department. Maybe they would get it back, but it would be thrown into the pool and re-allocated as necessary. That made the departments more willing to make the hard decisions, and nobody really argued about it. They understood why it was done, and it worked very well.

The other thing we did was to give the chairmen the chance to make a separate recommendation from the department because very often the chairman had insight that the others didn't, and yet the chairman sometimes didn't feel able to make a negative comment. I remember one time we promoted someone because the department made a recommendation, and it was borderline, but we promoted the person. Years later, the person was a disaster, and we went to the chairman at the time of the recommendation and said, "Why was this recommendation made?" And the chairman said, "I really didn't think it was a good idea, but I didn't want to go against the department." And we said, "Okay. We need to have that separate recommendation." Now actually, that same kind of thing came up two or three years ago, four years ago, maybe, in the Law School, where the dean made a strong recommendation against the person that the faculty wanted, and the person didn't get tenure. Whether it is a good thing or not, it is the next level up that has to evaluate these two different kinds of recommendations. But we thought it was important to have them separated out.

Now actually, the associate dean role kept me on the Executive Committee as well, which is what I've been talking about. But I started on the Executive Committee, I believe, in about 1968 or 1969, because I actually covered a period of ten years, except when I was chairman of the department in 1973 to 1975. And the early years were, again, during the time of the student unrest and the Vietnam era. On the one hand, I was involved with that kind of politics and

dissent in the ACM, as I've mentioned.

E. GALLER: Excuse me. Why were you appointed to be a member of the Executive Committee of the LS&A?

B. GALLER: Okay. Actually, I became visible, in a sense, to the campus during some of the early student unrest, when the teach-ins started at Michigan. The concept of the teach-in started at The University of Michigan, and of course, it spread across the country.

E. GALLER: This was anti-Vietnam War?

B. GALLER: Anti-Vietnam, right. And so we associate that period with Lyndon Johnson in 1968 not running again, and that was when things began to heat up in Vietnam. And so in 1968, which was probably when the first teach-in started, right around then, I can trace my involvement. At one of the teach-in sessions which I went to, I remember standing up and making some kind of statement. I don't know what it was. Not inflammatory or anything, but I said something.

Tape 3/Side 2

E. GALLER: What did you say at this teach-in meeting? And before you talk really much about what you said, tell what a teach-in is all about. Who was participating in such a thing?

B. GALLER: A teach-in is a kind of protest, but it is an attempt mainly to educate people about the issues. It is a teach-in in the sense that University buildings were used. People would gather in classrooms and ask interested faculty or, I suppose, other people, to explain the issues involved. I doubt that most of the time they were presenting all sides of the issue; they were probably presenting their side. But it had the form of an educational exercise. There wasn't any vandalism involved, there wasn't any trashing of things. It was an effort to convince, but in the process, to elucidate. I think it was a very effective means of calling attention to the issues, and although I doubt if the people

involved would say so, I think it was a form of protest to what was going on, the build-up of the Vietnam War. Now, you asked me what I said at that particular meeting. I just don't remember. I was probably moved to make some comment. But at the end of the meeting, I remember one senior faculty member - I can visualize his face, I don't remember his name - coming up to me and telling me he agreed with me. And he probably didn't say anything at that time, but just a few days later I got a call asking if I would be willing to be on the newly forming Senate Assembly they were creating at the time. They were concerned ... all of the turmoil and unrest, people were looking at how decisions were made and so on, and the Senate was organizing itself as the Senate Assembly, which was about 100 people who would represent the different schools and colleges.

E. GALLER: Was this the whole university or just LS&A?

B. GALLER: No, this was the whole University, and I was asked if I would run for election - I think there was an election; there is now - on the LS&A slate. And I think I came to their attention because this particular faculty member I know was involved in those negotiations and proceedings, and I'm pretty sure he saw me at that meeting and remembered to put my name on the list, then. So I got on the Senate Assembly, and I participated. It was interesting work, especially at that time. There were all kinds of arguments about whether we should take political stands. I remember one very active person on the Assembly was Gloria Marshall, a prominent Black professor, a woman, who later changed her name to Niara Sudarkasa, and she is now president of a university, I don't remember which one - somewhere in the East. But she was very active in that. And I remember sitting next to Giles Boles, who is now the dean of the Medical School. There were very interesting people on that Senate Assembly. I believe Cooperrider from the Law School was there, and Marcus Plant and others. I think, especially at the beginning, they recruited people who had been willing to stand up and make their opinions known in various forums, and that's probably the fact that I said something that got me on there.

At a slightly different time, but still in that era, I might mention how my class was disrupted during a protest. This was during the BAM strike - BAM, for Black Action Movement. I think two or three years later.

E. GALLER: 1970.

B. GALLER: Okay, 1970. Thank you. There was again, as part of the overall unrest, a feeling that there were not enough minority students represented on campus, and they wanted more power and more recognition, and so on. I think various Black student groups formed something they called the Black Action Movement, and a number of more radical white students joined them, and probably some non-students joined them, and they called a strike, and classes stopped meeting, some of them, and some of them met off campus, and it became a rather difficult period, lots of emotions. I took the position that they had some legitimate complaints, but I had an obligation to teach my class. I wasn't going to not teach my class. So I continued, and I remember very strongly one particular incident. I was giving a lecture, probably to 150 students in Auditorium B of Angell Hall, a rather large auditorium, and a group of the protestors - and I say the Black Action Movement - they were almost all white, the ones who came into my group and into my class - they came into the back of the hall, and began beating on tin pans and making a lot of noise, and I tried to continue the class, and they continued to make noise. I remember writing on the blackboard, "Hitler, 1933" and that got them very angry, and somebody ran up to the front of the room and tried to erase it. And after a while there was just so much commotion that I couldn't continue the class. One of the students in my class, Larry Hoard, a graduate student in chemistry who had been an officer in the Navy, came to me and said, "If you're going to prosecute any of those people, I will help you." And he said, "I recognized a graduate student in chemistry there." And I said that I recognized an undergraduate that I knew. And so I filed charges with the University judiciary, against those two particular people. We had a series of hearings ...

E. GALLER: Excuse me. At that time, there were quite a few professors who did have their classes disrupted in the same way. What did they do?

B. GALLER: Oh, I think by and large, once it was disrupted, I think you had to pretty well give up. You couldn't go on teaching the class. I think many of them were afraid to do anything.

E. GALLER: Did any of the other professors file charges?

B. GALLER: A few did. A few did. You had to recognize somebody so you knew whom to file charges against. I think some of the faculty were afraid. I remember getting a phone call from an anonymous caller saying, "Bernie, you don't want to do that, do you?" And I called the police and told them about it, and they said they'd sort of keep an eye on our house. You could be a little afraid at that point. I did go ahead, and we had several hearings. I have a tape of one of those hearings where the students disrupted it so much we couldn't have a hearing.

E. GALLER: Who participated in the hearing? Who was listening?

B. GALLER: Well, there was a panel appointed by - I forget the structure of the University judiciary at the time. There was a structure connected with, I think, Senate Assembly. But we were having a hearing, and we just couldn't continue it. So that particular case ended up that the students asked whether, they said ... well, this was during the summer, and they said, "The people we want as witnesses are gone for the summer. Can we continue this to the fall?" They agreed to do that, and neither student came back in the fall, and that was the end of it.

Another incident in that period (I don't remember the exact time) was the harassment of recruiters from government and industry that came to campus. And I remember taking the lead with a couple of other faculty in raising money. We sent letters out to all the faculty asking for small contributions to produce a full-page ad in the Ann Arbor News with the names of, I think, several hundred faculty that we got some money from, saying that we believe in the right of free speech, and that it is irresponsible of people, and unethical of people - I forget the exact statement - to stop recruiters who are coming to campus and to whom some students want to talk. And we did publish that ad. I remember - what is now First of America was Ann Arbor Bank at the time - the manager of the South University branch offered to keep the money for us as we collected it to keep it safe, and we would turn over the money every night to him, because we had a lot of cash and checks and stuff, until we accumulated enough, in fact, to pay for the ad and to go ahead.

Those were interesting times. Lots of protest, everybody had his own agenda, and everybody, in fact, had to sort out

exactly where he stood on those political issues. But underneath it is the question: "What is a university?" And "Should a university take political stands? Should we deny some people the right to speak because we think we're right?" I remember in 1975 an incident here where the president of Israel, Katsir at the time, came to give a talk on campus. And we were in the Rackham auditorium listening to his talk, and a group of protestors came in and made so much noise he wasn't able to continue his lecture. That, in fact, led to a change in University policy which said that the University will not tolerate the disruption of the right of other people to speak. And I think they have enforced that a couple of times. But there comes a time when you can't let people stop other people from speaking or you are going down a spiral pretty fast, and that is partly the basis of the political correctness problem we have now.

E. GALLER: What do you remember about your membership on the Executive Committee of the LS&A college during these years of the BAM strike and the teach-ins?

B. GALLER: I think the Executive Committee didn't have too strong a role as such. Individuals did. I remember one night, probably the worst night in our unrest on campus, when the National Guard was called out on South University and the police had helicopters flying all over and everything else, and there was great turmoil. I remember calling up Bob Fleming at the president's house and saying, "Is there some way I can help?" He said, "Well, we need a faculty presence out there on South U. to try to calm things down." A very emotional time. And I went out there, did what I could, tried to calm things down. We eventually got through that night without any real violence.

There were all kinds of people involved. I remember on that night a couple of people in the middle of South University, with a big crowd around, having sexual intercourse just to somehow protest against the values that they were protesting against, whatever they were. It was a strange time, a strange kind of emotions going around.

E. GALLER: Were these students?

B. GALLER: Who knows? These kinds of events attracted all kinds of people, and there is no way to know if it was a student or not.

E. GALLER: Were they drawing people from outside of Ann Arbor?

B. GALLER: Probably. I mean, with such chaos and crowds, there is no way to know who was involved.

E. GALLER: At the time of the BAM strike, when the University was actually closed down for the first time in its history, what did the LS&A Executive Committee talk about at that time? Do you remember it?

B. GALLER: Well, we were concerned with how to keep classes going. I really don't think there was any discussion of punishing anybody for participating. We recognized that people had their individual views. On the other hand, they had responsibilities for keeping the classes going. There was probably some talk about not paying people, faculty, teaching fellows, whatever, for those particular days and so on. I don't remember if anything was ever done about that. We recognized that we had to get through that period, and we had to get through it in such a way that afterwards we would be able to get along with each other and with our graduate students, who are our teaching fellows. And whatever people did as individuals, in support of or against what was going on, when we got through it we had to be a community again. So we didn't take a lot of action against people.

You know, we still have problems with TAs. There was a later strike of teaching assistants because in fact, they weren't treated very well. They formed a union, the GEO, the Graduate Employees Organization, and that still represents them. And we had a strike just last term, a one-day strike. Who knows, it may start up again in the fall. It was interesting. I had, last term, two teaching assistants, both of them, in fact, are professional computer programmers who happen to be students. And one of them said he opposed the strike. He said he didn't think it was a good idea, and he actually came to my lecture - they always came to my lectures, but where all teaching assistants were being encouraged to stay away that day, he came to my lecture - he told me it was partly to demonstrate that he was not in sympathy with the strike. The other one was sort of sympathetic, but he felt a strong obligation to his students, and he took great pains to notify his students in his sections that he would meet them off campus some place. So you have these different feelings and conflicts that they have as students, as graduate students, as

employees, and as teachers.

E. GALLER: Could you talk about the experience you had as a member of the search committee for the dean of LS&A at Al Sussman's retirement from being dean?

B. GALLER: Actually, I don't think Sussman was dean. He was an acting dean when they searched for the dean. I was on the search committee, yes.

E. GALLER: What year, approximately, was it?

B. GALLER: In the early 1970s, maybe while I was chairman of CCS, so it would be 1973-1974. It must have been right about then, because ... I remember that we did the usual search with advertising and so on, and we got it down to a short list of people, and we were asked to give our list, without ranking, to the central administration. And I think one of the leading candidates was a woman, a Black woman, who probably would have been - especially because they were trying to advance affirmative action and everything else - probably would have been the choice. She had good credentials. Unfortunately, for whatever their reasons, the biology department, in which she would have had tenure as a faculty member, said they would not give her tenure, based on her publication record, whatever. And this was important because appointments as dean, like chairmen of departments, which in the past had been for very long times, like life tenure, by that time had been changed, like department chairmen appointments, to terms of three to five years, typically five years. And after maybe one renewal, it was pretty standard that people would then not be administrators and become faculty members. And so the department had to decide whether they wanted that person in the department and whether they would accept tenure for that person and so on. It became a little sticky problem, but it suddenly blew up because someone leaked that fact to the Michigan Daily. I think at the time, everybody pretty well believed it was one of the Regents who leaked it in order to put pressure on the other Regents to go ahead, anyway, or to put pressure on the department to change their minds. But it became a very public thing, and of course, she withdrew her name right away, and that was the end of that. We were asked to make another search, and we came up with a short list, and Billy Frye was on that list, and Billy Frye became dean and the rest is history. He

was a wonderful dean, later Vice-President for Academic Affairs, and as I mentioned, is now at Emory University.

DATE: 11 August 1991

E. GALLER: You mentioned that you wanted to say something more about the LS&A Executive Committee.

B. GALLER: Yes. I think of all the committees and boards and groups that I've been on over the years, that committee was the most satisfying and the most dedicated and impartial. Every one of us came from a department. Every one of us had a natural constituency. But I've never seen such integrity displayed with respect to representing the College as a whole. In one or two instances, because of a conflict of interest, one person felt he couldn't handle it properly and he resigned from the committee. When we dealt with promotions, we had a standing rule that if a promotion came up in a department represented by an individual on the committee, that person would answer questions from the others as a resource and then leave, and we had an honest discussion. I always respected that committee. We used to get notebooks on Wednesday afternoon, maybe six inches think. There wasn't a single case where every one of us hadn't read that in time for Thursday's meeting. It was really great.

One funny anecdote. When I first got on that committee, Angus Campbell, who founded the ISR, the Institute for Social Research, was on the committee. He was the senior man. And the first meeting, he took me over to the chair farthest from where the dean sat, and he said, "This is your seat, and as the years go by, you will be able to move up toward the dean." That was really quite funny. He was serious; that was the tradition. But of course, it broke down quite soon. I don't know if I broke it, but it soon didn't make a bit of difference. But there was an awful lot of tradition connected with that committee, and it was a wonderful experience.

E. GALLER: You served on some University-wide committees. Talk about the ...

B. GALLER: The Research Policies Committee, for example. The Senate Assembly had a number of committees which actually served as advisory committees to the vice-presidents. And what might be called the Executive

Committee of the Senate Assembly, which was called SACUA, the Senate Advisory Committee on University Affairs - I think there were six people on that, maybe nine - they were the advisory committee to the president, and I believe that they appointed people to the other committees. The Research Policies Committee was advisory to the Vice-President for Research. I believe it was Charlie Overberger at the time that I was on it. I'm not sure. It was a long time ago. We would discuss policies with respect to, for example, when and how should a center or an institute be created? How do we avoid the problems involved that came up once in a while: one group wanted to start an institute in a particular discipline and were excluding some other people on the campus who naturally belonged in it, and how do we resolve that? And there were other questions about whether some of the overhead money received on grants, especially from the government, should be returned to the people who brought it in in the first place. That is now quite routinely done, but it wasn't always seen as necessary. I think it is a very good idea to give, on the one hand, incentive to people to get grants, and on the other hand, a little bit more freedom to support students and get equipment in between grants, for example. So that's now a standard thing. So that was a rather useful committee. The vice-president would bring questions that were bothering him to the committee. We were also able to initiate questions. And the similar experience I had on the Academic Affairs Advisory Committee, which was advisory to the vice-president for Academic Affairs. Allan Smith was the academic affairs vice-president at the time. He later, a couple of times, was acting president of the University, a very senior and very intelligent professor from the Law School. He didn't always bring questions to us that we might have wanted to discuss, but we were free to raise them, and a whole variety of questions. We almost never got down to anything concerning an individual person on the campus, but a number of policies came up, and it was a useful committee. I'm glad I served on those committees. I got a very wide view of what was happening at the University in both of those, and that stood me in good stead.

I might say that with regard to being an associate dean in LS&A, that really opened my eyes, because we tend to live within our own departments, our own disciplines. We talk to the people that work in the same field that we do. When I became associate dean, LS&A had 55 different units, about 33, I think, departments, then museums and centers and so on. I was able to learn a great deal about other disciplines, other fields of study, the values and expectations in those fields, the quality of work. I learned a lot about the quality of our own departments in the process, and all of those experiences were very broadening and, I think, did me a lot of good. I might mention one

experience.

We had one department where there had been a chairman for 40 years. We were in the transition from lifetime chairmanships to terms. This isn't the one I'm talking about, but for example, Hildebrandt, who hired me, had been chairman for 22 years and he wasn't quite as effective in his later years. And everybody realized it wasn't a good idea. But that had been the tradition. In this other department there was a chairman who did a lot of harm to his department from our point of view on the Executive Committee, because, for one thing, when he hired young people he promised them tenure, no matter what would happen, what they did. He raised their expectations so that it put us in a bad spot in terms of ever being able to turn anybody down. In fact, some of the people who were hired weren't that great. And when they became tenured, then they were able to give others expectations and so on, and the quality of the department simply went down. It was clear from national rankings that they were declining rapidly. We had to take a position that we were not going to promote people, or allow them to hire people who were not absolutely top quality. Every year we had a meeting with the whole department, the Executive Committee and the department, and they wrung their hands and they shouted and they screamed, and we said, "Sorry, we're doing this for your own good." We put a chairman in there who was dedicated to doing that, and he had a terrible time. They disliked him intensely, because he was making it difficult for people to get tenure who really shouldn't have by Michigan standards. Eventually that department turned around, and it is now one of our better departments. So that was an interesting, but difficult, time.

Another committee that I'm still on is what we call PCCAR, the Planning Committee for the Computerized Academic Record. This is a committee which undertook to computerize and automate the academic records of all students, to produce transcripts, to produce on-line, interactive auditing and counselling of students' records so that we could look at the record while we were talking to the students. It turns out that at Michigan, the transcript that you get if you are a student has been cut, literally cut, and pasted with scissors, and pieced together, and if a correction had to be made, it was a correction that was done by hand. The arithmetic to compute the new grade point average was done by hand - lots of mistakes. We were really in the dark ages. In fact, many other schools are way ahead of us on the transcript. About eight or 10 years ago I was asked to be on a committee to see if in fact, there would be financial

savings by computerizing; this was the criterion. And we spent about two years coming up with as good estimates as we could. We said, "Yes, you will in fact save money, because we won't need so many people to work on the transcripts, and there will be fewer errors and all kinds of things." And several years went by and nothing happened. They decided to reactivate that and start yet another committee and asked me to be on it. And I told Billy Frye, who was the dean at that time, that I wasn't going to bother if they weren't serious. It simply wasn't worth my time. And he came back, and he said, "Yes, this time we are serious."

And so I agreed. The committee has been meeting. At one of our first meetings we were asked to estimate the time for the project, and I led the way in estimating three years. Well, it's been about five now. The wheels grind very slowly at our administrative computing center. They do absolutely extreme planning, documentation, testing and so on. The system works at the end, but it takes about twice as long as anything that ever would have been produced at our academic computing center. We are well along. Parts of it have now been programmed, they are running some testing, beta testing on three departments this summer. It's going to come along, and it's going to work. Probably it will take another couple of years until it is pretty well done. The one running battle I have with them right now is that they are really in the dark ages with respect to the user interface. They expect, just because it has always been done, they expect the people who use this to have memorized the codes for all the schools. For example, LS&A is 5010 and so on. They resist greatly, just because they haven't ever done it that way, they resist trying to make it a friendly interface. We're having big discussions on that now, and I raise the issue at every meeting, and I hope that we will eventually get them to improve the interface, mainly because this new system is going to be totally decentralized. People in the departments all over the campus are going to have to enter data, not their small set of permanently hired data entry people who do get used to those numeric codes. They haven't yet come around to understanding that.

Another thing which is sort of a disappointment. As far as the auditing of the transcript, that is, telling the student how far along toward a particular degree program the student has gone, and what has yet to be done, and so on, they bought a program from Miami University in Ohio, to do auditing. The input to that program describes the degree programs. Every degree program is different, so you have to put in the rules so that you can do auditing. And the input to that is a very complex set of tables where the position in the table and the tables relative to each other

constitute the and's and or's in the rules, which is, you know, like 40 years ago. When I first saw this, about four years ago, I said we ought to be able to have a proper input language to describe a degree program; it ought to read like English, like the bulletin that the students read. And I can produce, with my students, I said, a translator from an input language that looks more like the English language, and I can generate the tables that this program needs. So the first thing I did was I had a young lady as an independent study design the language that would be needed to do this, and she did it. It was a fine job, and I showed it to the committee, and they thought it was wonderful, and nothing happened. And so, a year later, in my advanced system programming class, I had the students actually write the translator from that language into the tables needed by this program, and they demonstrated that it worked. And they did a fine job. And nothing happened. The committee said, "Gee, that's nice, but it uses Unix, and we don't know how to use Unix." I offered to have our guys teach them. Tim Howes and Bryan Beecher, who are still around, were in the group that did it. They can teach them how to do it. Nothing is happening. It may be that they never will use it, and that really is a loss. They keep talking about how much it's going to cost to train all those people on the campus to create that input, and they could cut that cost down to one-tenth if they use this program. But that's the way some people are in the computing field.

I mentioned my advanced system programming class, where they did the translator. That was a very interesting class, and I think I mentioned the class earlier when I talked about going to Ljubljana, Yugoslavia in 1971, to the IFIP Congress, I reported in a paper there about that class and the kinds of things we were doing. I may have mentioned at that time that CRISP was one of the main products produced by one of the sessions in that class. I'll say a couple more words about CRISP. CRISP stands for ... well, at that time, it stood for Computerized Registration Involving Student Preferences. Actually, during the class, everybody understood it to be Computerized Registration In Spite of Problems. In fact, the students called it CRISIS - I don't know what it stood for. The idea was that instead of lining up for hours outside the gym, waiting to get in and run around to different desks in the gym picking up punched cards which would represent then that a student was enrolled in a class, they could walk up to a terminal and either they themselves or an operator would call up the student's record, enter the class that they wanted to sign up for, and if there was a place in the class, then they would be registered. UCLA and one or two other places had done similar things. But we couldn't use any of that work because our database on students and classes was home-grown

and very different, and any kind of program like this had to be integrated with the database system. I decided, though, that we would demonstrate that it was feasible. The then director of the Data Systems Center was absolutely paranoid about security of his database, and to him, security meant physical isolation. So there was simply no telephone connection to any part of his computer from outside. Everything was inside the building. Of course, a couple of my students once went to his loading dock and walked into the building and showed that it wasn't that secure, anyway. But that was his hang-up. So he had no use whatsoever for any suggestion of interactive remote computing for registration. I did go to him, and I asked him to send a couple of people or a person to sit in the class and at least keep us on track with respect to interfacing to his system; in case he should decide to use our system we would be not far off and at least the interface would be right. I found out later that he really tried to sabotage the whole thing. When I started the class, it turned out I had his number two man in the class, Lyle Baack. I made him the manager of the project. It was a system programming project, like an industrial project, and there was a student manager. He ran the project, and of course, he was from Data Systems, so he was the interface with them. But he told me later that the director at that time had planned to send two of his most incompetent people, and had instructed them to sabotage the effort. When Lyle heard about this, he decided that he would come himself to help instead. He was wonderful in his help. We had Betsy Munn from the Counselling Office to help us with respect to the counseling interface.

The project was very successful. We got parts of a prototype system running. The director of Data Systems was still lobbying not to use any part of it. I took a portable terminal, which was really quite an innovative thing at that time - this was 1970 - I took a terminal up to Allan Smith's office, who was Vice-President for Academic Affairs at the time, and I showed him how it could be done. I showed him the parts of our prototype that were working, how you added a class or deleted a class from your registration, and so on. He called a meeting of the registrar and the Data Systems people and all the relevant people, and he said, "You will implement this system." And that did it. Actually what they did was they set up a committee chaired by Bruce Arden to really study whether it was a good idea for their system, and so on. They came up independently with saying, "Yes, it should be done." Meanwhile, we hired five students from the class - Gail Lift was one of them, I remember - over the summer to complete the prototype and make it totally working. Even though they were going to have Arden's committee study it and rework it, we wanted

to demonstrate that it really would work. We took all of the data from the previous fall term and ran it through our prototype system, and then - that's for all the students in the whole University - we demonstrated that the results, the actual registration, all the students in every single course in the University came out the same as they had done without the system, and then they were convinced that it could work. They subsequently implemented it, and got it working, except that I had gotten advice from Adrian Harris at UCLA that it was very important that every student have an appointment, when to show up to go through the system, and not to have lines. I told them this, they ignored it, and the first couple of times they ran it, they didn't give out appointments to students, and they had lines. It wasn't until Mrs. Frank Rhodes - Frank was vice-president, and now I remember it was Frank Rhodes to whom I demonstrated with the terminal, and he's the one, not Allan Smith, who told them to go ahead with it. It wasn't until Mrs. Rhodes stood in line for four hours that it got a little bit of attention, and they realized that they had better make appointments. And they do now, and it works fine. And as far as students are concerned, it has been that way forever, and that's the system, and there are no problems with it.

E. GALLER: I think it is interesting that the word CRISP is now part of the University of Michigan student language, and it is used as a noun and a verb.

B. GALLER: "I'm going to CRISP today" - sure. And every time I hear that, I smile to myself, because it is an interesting history, and it was a student project.

E. GALLER: Your consulting has been really very important to you, and very worthwhile to your career.

B. GALLER: Yes, I think it is very enlightened of The University of Michigan - I don't know about other places - people are expected to, or allowed to, encouraged to, spend up to a day a week consulting, and it is understood that this keeps us in touch with the real world. It enriches both our research and our teaching, and I certainly have learned a great deal from everything I've done. One of the first things I did - I think I mentioned that my introduction to computing was through General Motors, and how I became a consultant there, to get me into the gate more easily, but they treated me as a consultant, and that lasted about 20 years. I helped develop some interesting compilers for

them and evaluated their work for them, and made recommendations, and I used to go there, at the beginning, once a week. After a while it became once a month, and then toward the end, it was less often. But it was a fine relationship. I was recently invited back to a 25th anniversary of a very innovative system that they developed for an interaction between a person and a computer for drawing parts of a car and so forth. They had actually developed some hardware for this purpose with IBM, which was the forerunner of some early workstations, and I had been involved quite deeply in that project. When they had the 25th anniversary, I was treated like one of the family, and it was very, very nice. In fact, I think I was the only outside person there, at the reunion. And that was very satisfying, the relationship with General Motors. It was partly through that that I got involved in my more recent work as an expert witness in litigation, but I will mention that later, when we get to it.

I also helped General Motors evaluate the very large Control Data computer that they almost got. And that was interesting. I chaired a committee of outside people, and we studied the Star computer that Control Data was going to deliver. We decided it would be good for General Motors, but we cautioned that we'd better run benchmarks and make sure it runs properly. In fact, Control Data had made some errors in the design of the machine. They'd overlooked some things.

Tape 4/Side 1

E. GALLER: You were talking about GM's decision to get the Control Data Star machine.

B. GALLER: Yes. There was the Star 100, a very, very fast and powerful machine, but as I mentioned, Control Data had made some mistakes in designing the machine. They had to rip out parts of the machine. They had hard-wired everything, thinking it would make it faster, but it made it much more difficult to correct the mistake. They eventually put in microcode, which makes it easier to make corrections. It took them so long to finish that process that by that time GM and some other people did not get the machine, and it caused them a lot of problems.

Another interesting thing I remember about consulting for GM was that on one of their machines, IBM had made

what we call an *intelligent channel*. A channel is an input-output path into the computer, and *intelligent* meant that in fact, it was a computer in its own right, but with a limited set of instructions that it could do - a little bit of arithmetic, like adding one or counting, retrying for errors, and so on. So the idea was that the channel, as a little computer, could relieve the main computer of having to worry about whether the data was coming in correctly; it could make sure it was correct. And make sure that the right amount of data would be coming in, and so on. So, in order to write the appropriate programs for that little computer, we invented a language, and we wrote a compiler. I was very involved in that. We weren't quite sure it would work, so we called it Maybe, and the Maybe compiler did, in fact, work. On the other hand, it slowed down the operation of the channel to try to do all of this checking and everything. IBM soon gave it up and never built that kind of intelligent channel again. But it was an interesting experience. I still have good relationships with General Motors, but I almost never go there any more.

Another important early consulting relationship that I had that lasted, again, about 20 or 25 years, was with the Booth Newspapers. The Booth family owned eight newspapers all over Michigan outside of Detroit. All of the second-line cities - Muskegon, Grand Rapids, Ann Arbor, Ypsilanti, Flint, and others - and they had the idea that maybe they could tie them together with a computer network and centralize some of the operations. They came to me one day; they had asked me to have lunch. I'm not sure how they got to me, but we had lunch, and they said, "We have this idea. What do you think of it?" And I said, "Well, it really sounds like a fairly low-level operation, but it can be done." And they never let me forget how low-level it was. We went on for 20 years building that system, better and better. One useful thing that came out of that was that we worked with Control Data. I recommended that the little computer that Control Data was selling at the time, the 160A, was the best for their job, and they later went on to Control Data 3300 systems, and so on. And that has stood me in good stead, because now when I do a lot of expert witness stuff for IBM, I am able to say, "Look, I didn't always favor IBM. I was as impartial as I could be in looking for the best possible computer. I recommended Control Data." Which was true, but it is helpful to have that in my background.

In any case, we developed the network system, which worked pretty well, but it was really from scratch. They had not a single computer person in their employ. I had to help them hire their first computer person, Gordon Carlson,

and then he and I together hired the next fellow, Ed Lucas, and so on, and we built up a staff. We developed applications, not only the business side of the newspaper group, but the production side. That is, all of the other papers would send raw text that was keyed in by an operator, and would send it over telephone lines into the central computer. All of the typesetting information, the hyphenation and justification and all of the control needed to drive their linecasters, was prepared in the central computer and sent back out to their machines, where it was punched on paper tape and used as direct input to their linecasting machines. That was very innovative, and it worked fine. It was interesting that whereas in many applications you have to be absolutely careful that every single bit is correct as it goes over telephone lines, they said, "Our standards don't have to be that high. If we get a bad letter in a word every once in a while in a news story, our proofreader will catch it and fix it, because we still have to do the proofreading. So we can be a little more relaxed on the actual error rate on the network." And that was helpful. It cut the cost down to some extent. We ended up having computers at each plant, at each newspaper in each city, because after a while we introduced classified ad input. People would key in classified ads. They would be hyphenated, justified, proofread, and so on, and go right back out to the linecasters; so in fact, the composing room, which used to be a large number of people sitting at those linecasters, keying in character by character, essentially was eliminated. And of course, that could lead to union problems. In some newspapers, this was a terrible problem. In West Palm Beach in Florida, they fired all their union people and had strikes all over the place. In Toronto, they had a strike that went on for 20 years. Mostly because of Jim Sauter, who was their labor relations person - in fact, the person who first brought me into the process and later was president of the newspaper group, but at the time he was labor relations - he had a way with the union people. He negotiated with them, he convinced them that this was the way of the future, that they weren't going to lay anybody off. They might not replace people when they left. They would have an opportunity to learn this stuff if they wanted to. The Booth newspapers never had any problems whatsoever with the union. It was an interesting lesson in human relations, too.

There was, at one point, a demand by the union that they should have control of every word from input to output, from the beginning to the end of the process, and therefore they wanted to take over the computers, and run the computers, and so on. Sauter said to them, "Well, if your people can learn it well enough, we'll see what we can do." So they sent some of their people, some of the union people, to a school. I think it was supposed to be an eight-week

school. I told them that they're not going to learn enough in an eight-week school. They dropped out after two weeks. They just couldn't handle it. They hadn't had the background or the education. They couldn't deal in abstractions enough. They came back and advised the union that they'd better lay off, that they weren't going to be able to handle it, and that was the end of it. And the computers stayed within the management sphere, but the composing room was union, and Sauter negotiated the proper relationships, and it worked out fine.

So it was not a very complicated operation, but we added more and more things. The wire services would come in already in computer form. It had to be stripped of all the standard wire service stuff, and then reformatted for the Booth Newspaper style, but they wrote programs to do that. And they were very innovative about handling what they called display ads, the very complicated different sizes of text and combinations of things that go in normal newspaper advertising that we take so much for granted. All of that had to be done on the computer, very innovative stuff. I learned a lot about the newspaper business. It was very satisfying for about 20 or 25 years, until the Newhouse newspaper chain bought out the whole Booth system. They began to bring in some of their own computer people, to replace the systems we put in with more modern systems, and they didn't need me any more, so that was the end of it. But a very satisfying and educational experience for me.

Another important consulting relationship that I had was with NCSS. NCSS was a company started by Dick Orenstein and one or two other people. Orenstein had been a student at Michigan. He got a bachelor's degree here, went to MIT, where I think he got a master's degree. Then he and his friends realized that the time-sharing business was growing. I think this was in the late 1960s. And they saw an opportunity. IBM had developed, as I mentioned, the TSO system, and the CMS system. TSO was the very slow, cumbersome one that IBM stuck on top of their standard operating system. CMS was a - I didn't say this before, but it was a kind of moonlighting job. Some of the people in IBM did it on their own time, and then the corporation realized that this was a good thing and began to market it. And I mentioned earlier that we evaluated CMS and decided that MTS was better, so we never picked up CMS for our own use. But CMS wasn't very bad. It was a pretty good time-sharing system, an interactive system, and it was on standard IBM hardware and interfaced with standard IBM systems. Orenstein and his friends knew the value of being compatible with IBM. They obtained the CMS system. They gutted it entirely as far as the internal

code, and rewrote it so that it had the external shell and user interface of CMS, but they made it very efficient. IBM was never very good at making an efficient system - a lot of overhead, slow, and so on. So Orenstein and his friends did it well. Now, they had an IBM- compatible, very efficient, interactive time-sharing system. They began to market services. People could use their system on NCSS. Their company now, had the mainframe, and people could get into it buying only terminals, and have access to a very good time-sharing system. Much later, they carried over a version of that system to small mini-computers, and as people outgrew the time-sharing system service that NCSS was providing, they could buy their own mini-computer and move the system over to it and be in business on their own. So NCSS grew, and was very successful. At some point, when they had I think about a $30 million a year revenue, they realized that they had nobody on their board with a technical background, and the board was not really able to communicate too well with their own technical people. Orenstein said, "I've got a professor back at Michigan who might be okay. Let me go and talk to him." So he flew out to Ann Arbor to talk to me. I remember it was 1975. It was, in fact, the day that President Katzir of Israel tried to give a lecture and had it disrupted, because Orenstein was with me at that lecture. He discussed with me the possibility of my joining their board of directors. I thought it would be very interesting. I hadn't had that experience yet, so here was yet another opportunity. A few weeks later I flew to New York and to Boston, and met with various directors, some in New York, some in Boston. They were looking me over, and shortly after that I was invited to join the board of directors. It was very interesting. There was some salary connected with it that helped us put Elaine through Harvard. It was exactly those years, 1975 to 1979, that she needed ... she was going to Harvard and the tuition was going up, and it was very helpful. I was on the Audit Committee of the board, and had to meet with the auditors and make sure that they were, in fact, doing a good job of auditing the systems so that there couldn't be any fraud, either internal or external, at the various branches, and so on. I remember that because of my experience with time-sharing, I was able to suggest ways in which people could embezzle money from the system, from the company, that the auditors hadn't dreamed of because they had no background in computing, really. And so I felt I served a good function on that, that they closed all those loopholes. And we made a very fine set of friends through that board also. Bernie Goldstein, who is probably the premiere person in mergers and acquisitions in the computer industry; Bob Weissman, who went on to become president of Dun and Bradstreet, where he is now; and Dick Orenstein, of course, who - I'll explain this - in 1979, we sold the company to Dun and Bradstreet. Bernie Goldstein had negotiated such a good deal that we could not in our

conscience and our fiduciary responsibility turn it down. None of us wanted to give up the company. It was a growing company, over $100 million revenue at that time. It was doing well, which is, of course, why Dun and Bradstreet wanted to buy the company. And he negotiated such a good price that we said, "Unfortunately, this is the end of it for us, but we can't help it. On behalf of the shareholders, we have to do it." So the company was sold to Dun and Bradstreet, and that's how Bob Weissman began to move up in their hierarchy. And so that was the end of that consulting. Orenstein, at the time had a great deal of stock in the company, and felt that it would be beneficial to him for tax purposes and otherwise to donate a lot of that stock to The University of Michigan. In fact, he donated enough to establish the Orenstein Fellowships. We have two fellowships now, in our department, in our division, called the Orenstein Fellowships, based on the income from his donation to the University at that time. So he is now, in fact, a member of the Presidents' Club at The University of Michigan because of that. And I was very grateful to him. He designated me as the sort of trustee of his fellowship. That is, the funds can't be used in any other way unless I approve. And that's very nice, to be recognized as trustworthy of his confidence in that fellowship. So that chapter ended rather abruptly with the sale of the company to Dun and Bradstreet.

E. GALLER: But you learned so much about how a corporation board of directors has to work.

B. GALLER: Oh, absolutely. How a board of directors has to work, how a corporation works. One thing that came out of that experience and some of my other consulting is that I do have a healthy respect for people who run corporations. I mean, we hear a lot about fraud and mismanagement, and so on. At least the people I have worked with have always been very dedicated, careful in the management, a strong feeling of fiduciary responsibility, never a suggestion of dishonesty, always carefully running every opportunity and decision past the accountants, the lawyers, to make sure that everything was proper, and I came away with a very good feeling about corporate America, although with the skepticism that we do see a great deal of fraud, especially in the aerospace and other military-type things, where there is too much opportunity for it. The people I have known have been honorable and dedicated people.

One other consulting arrangement that is still going on is with the NCR Corporation. I'm on their Science Advisory

Committee, which is organized by Mike Flynn, who is a long-time consultant for them, apparently. This is a committee of about six people, and we have been traveling to different NCR plants around the country about three times a year, looking over their operation, making recommendations and getting to know the company better. It's interesting; now that AT&T has acquired NCR, I was wondering whether the committee would even be in existence. In fact, though, we're meeting in a couple of weeks in Dayton, the company headquarters, and I guess I'll find out more about whether the committee will continue. Again, it's a learning experience. Some of the members on the committee are very involved with chip production, how to lay out and wire chips, others in system architecture, and I'm on there representing language and software. And so we have a broad spectrum of interest, and we talk about a great many things with NCR, and I learn from all the others about the state of the art. NCR is a very big company. They have no debt, essentially. They are trying to move into the Unix world. They have established a very strong goal in the last couple of years of being number one in certain areas, and they're trying to get it. And I think AT&T acquired them partly because AT&T had made a mess of the computer business and saw that NCR was successful. We just hope that AT&T will leave NCR alone and let them do their thing. So often when companies are acquired, the acquiring company puts their own people in, and they run them into the ground, where the reason they got the company was because it was successful. If they leave them alone, hopefully everything will be okay. I did talk to some AT&T people recently, and they said, "Oh, we're going to leave them alone. We're going to leave their management in." It remains to be seen whether they can keep their hands off and let them be good.

DATE: 16 August 1991

E. GALLER: You had other kinds of consulting, which were expert witness experiences. Tell about that.

B. GALLER: I've been very fortunate in being recommended by various lawyers to others, and so I've built up a considerable amount of experience now. It started, I think, back in the 1970s, in the Government v. IBM trial. I was slated to be a witness with regard to IBM's development of the 360/67, the one I mentioned earlier that was done specifically for Michigan. I actually never got deposed there. They settled the case. But because I was on the list there, and because Don Hart at General Motors had been deposed -- in fact, I guess he had testified at the trial -- I

believe that that led to Cravath, Swain and Moore, which may not have been their name at that time, to ask me to be a witness in the IBM v. Greyhound trial. I was deposed there. I got to know Bob Mullen from Cravath, who taught me a great deal in getting ready for that deposition. Incidentally, Elaine went with me to one of his training sessions, and he still asks about her whenever I see him.

I was deposed in the Greyhound case. It was not a very extensive deposition. It was by a local person in Ann Arbor, but it was good experience for me. That case was also settled, so I never got to testify there. By that time, I was known to Cravath. The main case that I really got started in was the Fairchild v. Data General case on the West Coast. Jack Brown, who is by far the world expert lawyer on copyright cases, used to be with IBM. Now he's with Brown and Bain, his own law firm in Phoenix. But he knew all the people with Cravath, and I guess they referred him to me, and so he asked me to testify in the Fairchild case. That was a major undertaking. It turned out that I was able to lecture directly to the jury for four hours, with all kinds of visual aids, and so forth at the beginning of the trial. The trial lasted about three months, but I was the first witness to educate the jury about computers. It was interesting, because the judge had removed from possible jury participation anyone who knew anything about computers or even had a friend who knew about computers. Very restrictive. That case was also pretty unusual because the judge, Judge Orrick, in the 9th Circuit in Federal District Court in San Francisco, insisted that all direct testimony be written ahead of time, and made available to the other side and to him, so a great deal of planning had to go into that. We even rehearsed my testimony with a group of people off the street to see if it was effective. In the process, I got to know Jack Brown very well. That was an interesting case in that the jury decided for us, then the judge decided that no jury should have been able to reach that decision, so he reversed it, and the appeals court absolutely slapped him down in a very strong manner, and we won the case. They settled for damages of $52 million in that case. It could have been $300 million, but in any case, that taught me a great deal about testifying. I learned a lot about copyright, about the law, and since then, I have been referred from one place to another in a number of very important trials. Jack Brown has been a champion of mine all along. I was a witness for Lotus in the Lotus v. Paperback case, which we won. I was a witness with Jack Brown in the Intel v. NEC case. There, I didn't actually testify for a variety of reasons, but I was deposed twice. But I believe I helped out a great deal in that case, and we did win the part of the case that decided that microcode was copyrightable, which was the important part for

intellectual property law. I am now involved with Jack again, in the Apple v. Microsoft and Hewlett-Packard case, a very important case on the so-called *look and feel* of the user interface. All of these have been important milestone cases. I've learned a lot. Along the way, I've been involved in a number of other anti-trust cases -- Fairchild was an anti-trust case -- cases involving non-performance of computer systems, patent cases, trade secrets, both plaintiff and defense. It's been very, very interesting.

Out of this came a new development. Because of my interest in intellectual property, it happened that I was in a long limousine ride after some meeting with a man named Brian Kahin, who is an adjunct professor at Harvard and very involved in intellectual property law. And he alerted me to the fact that the Patent & Trademark Office was issuing many software patents, mainly because they didn't know anything about software, and they had no prior art against which to compare applications, because there had not been any software patents issued, or at most a handful, before 1981, when there was an important Supreme Court decision about it. So the Patent Office began issuing patents because they didn't have the prior art to turn them down with, and they didn't have the expertise to decide something that might have been obvious. Their definition of *obvious* is "a small step beyond the prior art." And if there is no prior art, everything looks like it is a big step. And their examiners have almost no programming background, and it has just led to thousands of patents being issued, most of which anyone in the computer field can recognize as obvious or "I did it 20 years ago," or whatever. The Patent Office admits they have a problem. Jerry Goldberg, who is the head of the group that handles software patents, is asking for help, but meanwhile, they keep coming out.

E. GALLER: You mean patents keep being ...

B. GALLER: Patents keep coming out from the Patent Office approved, and there is a big uproar in the computer industry because, while there hasn't been a lot of litigation about infringement yet - everything is too new - the potential is there, the potential for abuse is there, and we can expect to see a lot of it in the next few years. There is a lot of uproar; there is the League for Programming Freedom, headed by a guy at MIT, which is lobbying very visibly for getting rid of patents for software, period. There is a commission, a government commission, on reform of the patent law, which is much broader than software. They are looking at all of the questions, but their main problem is

clearly software, and their request for comments and advice stresses that they need help in the software area. I have talked to some of the people on the commission and given them my ideas; and other people are. But that is long-range stuff. What was needed, clearly, was a way to stop the flow of patents which shouldn't have been issued. And I have taken the position that I think that there is room for some patents, really legitimate ones, but that most of the ones that they are issuing are not well-advised. So I decided to see if I could take some action in this regard. I prepared a proposal just about a year ago, last summer, the summer of 1990. I wrote up a proposal, about six or eight pages, for the establishment of a Software Patent Institute. Actually, the proposal was for a meeting to discuss the feasibility of the institute, but in the proposal, I described what the institute would do. The idea was to establish an organization which would solicit prior art contributions and establish a database of prior art. It would include all the patents being issued - they are prior art now - but also solicit from practitioners in the field and academics and software vendors, whatever they are willing to disclose as prior art, at least so the Patent Office could, if they wished, search it and know what prior art is there. Potential applicants could search it and decide that they shouldn't even apply, which would help cut down the number of patents issued, and anyone who was sued for possible infringement of some patent could look in the database and see if there was anything in there to help fight the patent, or challenge it, or whatever. So I wrote the proposal, and showed it to Brian Kahin, who had suggested the problem to me. He was enthusiastic. He showed it to Jerry Goldberg of the Patent Office, who became enthusiastic about it. And word about it began to spread. I solicited the help of my friend Ken King at EDUCOM, which is a consortium of universities.

E. GALLER: Who was especially interested in this? Lawyers or scientists?

B. GALLER: Well, as the word spread, everybody. Everybody, because everybody knew there was a problem. Nobody knew what to do about it. Fortunately, everybody, without exception; even the League for Programming Freedom didn't attack my proposal. They didn't come outright and support it, but they didn't attack it; they didn't say it wasn't a good idea. Every single person who's seen it has said: "This is very important, it should be done, good idea, and so on," because no matter what their long-term ideas are about what should happen to software patents, everybody recognized the need for a prior art database. So I asked Ken King of EDUCOM to support it, also,

because I wanted the blessing and the endorsement of this consortium of universities, and we formed a kind of informal steering committee -- which became more formal later -- of Ken King and two of his colleagues at EDUCOM; Jerry Goldberg of the Patent Office; Vint Cerf, who is a very distinguished computer scientist who happens to have a special interest in high-quality, high-speed Fax, which we saw as important in disseminating patent descriptions down the road. And it was suggested to me that - of course, I wanted to establish it in Ann Arbor. I thought that if we started an institute, it would be good for Michigan, good for Ann Arbor, to have it here. I didn't really want it within the University. The University is not the most hospitable home for an institute which is a service-type thing, not a research thing. But I was pointed toward Roland Cole at ITI, the Industrial Technology Institute, as a person who might be very interested in this, and I saw ITI as a possible home for it, as a non-profit institute involved in computer technology. I talked to Rollie Cole, and it turned out that he at one time had been president of the PC Users Association, so he was knowledgeable about the political scene in computing, the computing industry. He is a lawyer, he is the legal counsel for ITI. He had all of the qualifications needed to be an active person, and he happens to be a very nice man to work with, very enthusiastic about everything. So he became a member of our steering committee, also.

The problem became whether I would get money from anyone to run the planning meeting. I had listed in my proposal 20 or 30 possible people to invite, all of the important people in the field that I knew, people like Jack Brown and Susan Nycum, who is one of the most important copyright lawyers around; several patent lawyer friends of mine that I respected; people from computer science like Jean Sammet and Arthur Norberg, and from the Patent Office. I had listed a whole bunch of people, clearly saying that most of these had not even been talked to yet, but these are the kinds of people I would invite if we had the meeting. At the suggestion of Brian Kahin and Jerry Goldberg, I sent the proposal to a number of industrial people. Unfortunately, sometimes they got into the wrong hands within a company, and there was no interest. I sent a letter, for example, to Ken Olsen, who is president of DEC, and it got shunted off to his public relations people who said "No, we can't do this." DEC people later told me I should have sent it to their corporate counsel. Well, I didn't know their corporate counsel. I knew Ken Olsen. But one of the people I sent it to was Tony Clappes at IBM, their lead copyright lawyer, whom I had worked with on a number of occasions. Tony called me back three days later and said, "I've talked to Roger Smith, our chief patent counsel, and

not only are we going to put in $10,000 toward your planning meeting, but we will support the institute when it is established." That was very helpful. I figured I needed between $20,000 and $30,000 to run the meeting. The reason I needed that kind of money is because I planned the meeting after one I had been to, in fact, the one I had been to from which I was riding with Brian Kahin and got a start on this. The structure of such a meeting is a two-day meeting, or one and two-thirds-day meeting, where four or five people are commissioned ahead of time to write commission papers. You have to carefully divide the overall topic into four or five equally important, equally difficult question areas. Then you ask people to write position papers on those, and those form the basis for small-group discussions at the meeting, from which you come out with answers to the kinds of questions the meeting is supposed to answer. In order to get responsible people to write such papers, I needed money to pay them. And so that was part of the budget I proposed. Another part of the budget was to support people whom I wanted to invite who didn't have a lot of corporate support and would need help in their travel expenses. And then we wanted to have a dinner and pay the office of the University Conferences and Seminars for mailing and logistical support and arrangements, and so on. So I needed .... It turned out I did, in fact, raise $26,000. IBM came in first. Jack Brown, because he has represented Apple, and does represent Apple now, introduced me to one of their lawyers, and he wrote a number of letters to Apple on my behalf, etc., and nothing ever came of that.

The Software Publishers Association heard about this from Jerry Goldberg. For some reason, some of their people were meeting with Goldberg in Washington about something, and he told them how enthusiastic he was about this, and told them they really ought to support this. And then he told me that he had told them that, so I got on the phone and sent them the proposal. And I heard from the chairman of their Government Affairs Committee that they were interested in supporting it, and I would hear from Tom Lemberg, who is the counsel for Lotus, whom I knew because of my involvement with the Lotus case. It was interesting that Tom called me and said, "Yes, we're going to support you. We're going to put in $5,000." And I said, "Tom, you guys are so cheap. You know, you've got multi-million dollar companies. I need $10,000. I haven't got time to fool around. You're either in it, or you're not." He said, "I'll get back to you." Two days later, he called me and said, "Okay, $10,000." I mean, I had to take a firm position. I was getting fed up already with these guys who have all kinds of money and who were trying to - I don't know - play on the low end of it, which wasn't worth my time. Anyway, they put up that money. It happened that I was, because of this proposal, getting some visibility. I was invited to two meetings to talk about the thing. One was

in the Detroit Renaissance Center, a committee of the AIPLA, which is the Association of Intellectual Property Lawyers. Or, I guess it is the American Intellectual Property Lawyers Association, their committee on intellectual property, on patents and copyrights. And I gave my talk to them, and got some very enthusiastic support there. And the other talk was in Washington, at a conference sponsored by the Annenberg Association, on intellectual property.

At that meeting ... I'm sorry. There was a third meeting. It wasn't the Annenberg meeting, although there I met again some very important people who came, in fact, to the planning meeting. There was a third meeting. It was a Congressional task force, staff people, who were ... it was the Office of Technology Assessment ... who were having a series of meetings to help the Commission on Patent Law Reform, and I was invited to be on one meeting, one panel, of computer scientists. They had had a previous one of lawyers, and so on. At the OTA meeting, I met Dave Nagel, who is a vice president of Apple, who was there at the same time. At the meeting, I was asked to explain my Software Patent Institute proposal. And so I explained it, and afterwards I went up to Dave Nagel, and I said, "I don't know why Apple has not been a supporter of this. Jack Brown has been lobbying with Apple for it," I said, "and your comments after my talk, you seem to support this. I want some money from Apple. That's all I need now to have enough to run the meeting." He said, "Call me in a few days." When I called him, he said, "Well, we'll put in some money. What do you need?" I said, "Well, at this point, I need money to cover the expenses and a possible honorarium for a dinner speaker, and I need some contingency money in case we go over the budget." "I'll call you back," he said. And he called me back a few days later and said, "Well, we'll give you some money for what you want, but we want a seat on the board." I said, "There is no board at the present time, and I'm not about to promise anything." I had to fight off efforts of various groups to control this thing, and this was one instance of it. Incidentally, I am planning to propose a person from Apple for the executive committee, but I wasn't about to give in to that kind of thing at that time. Anyway, he did offer to support the speaker at the dinner and $5,000 of contingency, and at that point I had enough money to run the meeting.

We sent out a lot of invitations. Most of the people I invited came. It was very successful in that sense. But another incident happened. The Software Publishers Association people asked me for a list of who was being

invited, about two weeks before the meeting. I sent them the list, and I got a call, I guess it was from Tom Lemberg again, who was acting as secretary of the Government Affairs Committee. And he said, "We'd like you to uninvite two of the people you have asked to come." It turns out that the two people he was talking about were people who were fairly activist in the sense of long-range not wanting to have any software patents at all. Now, the Software Publishers Association represented software vendors in the industry - Microsoft, maybe Apple -- I'm not sure -- WordPerfect, and Lotus, and so on. They were supporting all of this because they had patents and they want their patents to be effective, they don't want too many patents. They were supporting what I'm doing, but they don't have any sympathy for those who want to eliminate patents altogether. So he said, "I want you to uninvite two people." And I said, "No way am I going to uninvite people. First, it would be an insult to them, and second, you have to have faith in me to make sure that this meeting talks about the Software Patent Institute and doesn't get involved in controversies about long-term things." And he backed down. And it was another incident where I had to retain control of this thing, and not let these guys who were putting money in push me around just because they were putting money in.

Well, we had the meeting. It was very successful. I opened the meeting by saying, "Everybody here has his own agenda. Everybody here has his own reasons why you're coming here. You all have different opinions about the long-range future of ...

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... and we're just not going to discuss any part of the longer-range controversies, or I'm going to rule people out of order." Then we started the meeting, and Jerry Goldberg talked about his problem, and a lady from the Patent Office, Margaret Moskowitz, involved with the Biotechnology Institute, which is a similar kind of thing that I learned about after I made my proposal, but is within the Patent Office. It has some problems. She talked about lessons leamed from that. Bill Kefauver, who is a retired, I think, patent counsel for AT&T, but who is on the reform task force, talked about their work. And then I proposed a mission statement for the Institute, which I had developed, and it was accepted with a few suggestions for changes. Then I proposed about half a dozen questions which this meeting had

to answer, such as: "Is the Software Patent Institute a viable thing? Where should it be located?" and so on. Then I asked the authors of the papers to give ten-minute highlights of their papers. I had asked a very prominent patent lawyer of IBM, Pryor Garnett, to prepare a paper, and Vic Rosenberg, a close friend here in Ann Arbor, who is an entrepreneur of a small company. Both of them prepared papers on what kinds of incentives would be needed to get people to put contributions of prior art into the database, and questions of overall feasibility and so on - Garnett from the point of view of IBM, a large corporation, and Vic Rosenberg for a small company. Both were very, very enjoyable. All of them were very good, and raised a number of points which were very helpful. The second paper had to do with the technical questions of how one would organize the database, search strategies, and so on. I had asked Bob Korfhage, who is now the chairman of the Information Sciences Department at Pittsburgh, to write a paper on that. He did an excellent job, and talked a little bit about some of his own research in this area, which is very interesting, and the paper was very successful. The fourth paper was by John Sayler on the topic of what kind of training the people in the Patent Office should be getting, and that was well-received. In fact, in the final report of the conference, they lifted a list of his suggestions right from his paper to put into the report.

And the final paper had to do with how to establish the institute, the governance, the budget and fund raising, and so on, which had been prepared by Roland Cole and his colleague Bill McUmber of ITI, and that was adopted essentially without change. So before lunch, then, on the first day, the highlights of these papers were presented. In the afternoon we broke up into small groups for each of these major topics. It was interesting that I didn't assign people to be participants. We had about 45 people there. I didn't assign them to the groups. I said to them, "Let's have a show of hands which ones you are interested in, and we'll see if I have to encourage anyone to go to something else because of an imbalance." The groups turned out to be almost equal in size, and it was great. The afternoon was spent, then ... I had prepared a separate list of questions for each of these break-out groups to guide their discussion, and they went off and had their discussions and broke up about 5:00, some groups longer than others. We then convened ... I should mention that I had assigned ahead of time a facilitator and a scribe for each of these break-out sessions, a scribe to record the main points that were in agreement and so on, the facilitator to lead the discussion. That night, those two people for each of the break-out sessions were assigned the job of summarizing what went on for presentation the following morning. I was the scribe, in fact, for the final session, the

governance and budget session, which Rollie Cole had written the paper for, and I forget at this point who was the facilitator, but it was very well done.

We had a dinner at the Michigan League in the Hussey Room, a very nice dinner, and the speaker was Elmer Galbi, who had retired from IBM as a patent lawyer, but was a consultant now, who lives in Lake Oswego out in Oregon, and he talked about some things that were going on in Lake Oswego from which he drew lessons about software patents; it was a good talk.

The following morning we met to have the facilitators and scribes present the summaries of the separate sessions. We had about 45 minutes for each one. We discussed the conclusions they came to, and then we broke for lunch, during which Rollie Cole and I prepared a series of resolutions that we wished the meeting to vote on in the afternoon session as the final report of the meeting. After lunch, we took up this list of resolutions and went down the line. Most of them we just got agreement on. I ran the meeting in a way which facilitated rapid movement when there was agreement, and ... for example, on each one, when there was clearly agreement, I would say, "Okay, any votes against? Any abstentions? Done." And that was it. And they appreciated that because it kept moving.

As soon as there was discussion on something, and it was clear that there might be some controversy, I would say, "Fine. Now we will go back to Robert's Rules of Order and we will ask for a motion and amendments, and so on." And we had an orderly change, and we modified a number of things. There was one interesting thing where I had indicated that I would help people challenge existing patents. There was quite a bit of argument against that, based on, again, the idea that some of these companies didn't want their patents challenged, really. They did not want an advocacy role at this institute. They wanted it to be totally neutral. When I saw that this ran into trouble, I backed off. I said, "Fine. We will disseminate abstracts of new patents to everyone over e-mail across the country. We'll disseminate them rapidly, and we won't interpret them in computer language," which was my original idea. Because the patent lawyers that were there kept arguing that you can't interpret patents -- that's misleading --and so on, I had to back off and say, "Okay, we'll disseminate the abstracts so that people can read them, and we won't encourage people to challenge a patent, but we'll make sure they know about them." And privately, one of my goals is to still

issue guidelines as to how to go about challenging things if you need to. But the way that people might challenge a patent is to come up with the prior art that is needed to challenge it with, and from my point of view, that is prior art that is going to go directly into the database, so we will, in the process of disseminating new information, continue to build a database and make it possible for people, if they wish, to challenge patents. And I said exactly that at the meeting, and everybody was satisfied.

It was interesting afterwards. Pryor Garnett, the lawyer from IBM who presented one of the papers and is very sharp, and has been nominated by a number of people to be on the executive committee, came up to me afterwards and told me that he thought I ran a very good meeting, and he said, "You also knew how to be pragmatic; you knew when to back down." And that was very nice.

So, the meeting was successful. We agreed on .... The important thing that I had to get out of this whole meeting was what to do next. What were the next steps? And toward the end of the meeting I proposed, and people agreed, that the next steps were that there would be two weeks for people to nominate people for an executive committee of about eight people who would, once we got them agreed on, which I will explain, the executive committee would expand itself into a full board with another six or eight people, but meanwhile the executive committee would develop a business plan for the Institute, and choose the host institution. We discussed ITI, and there was general agreement that it would be a good idea, but the executive committee has to do what they call "due diligence" to make sure that it is, in fact, a viable organization, and that the appropriate relationship would be negotiated between the Institute and the host institution, and so on. The board would then generate a number of committees - fund raising, technical committees, and so on, and start the work.

The process for choosing the executive committee is interesting, because how do you start from nowhere? We agreed that within two weeks, everybody at the meeting would nominate people for the executive committee, and indicate whether they, themselves were available or not. Within the next two weeks, we will send all of those nominations out to all the participants for their comments - not their votes, but their comments. That input will go in to the original steering committee, which is the only real group in existence at this present time, and the steering

committee will appoint the first executive committee. At that point, the executive committee takes over and generates all the things I said, plus bylaws for the organization, and so on. There was a description of the executive committee that it should have broad representation from industry, government, academia, and so forth. So the process is started, and we sent out a complete draft report to everybody who was there -- of all of these resolutions, and so on. We have been getting some comments back. The main comments for change came from Jerry Goldberg of the Patent Office. He showed it to their lawyers, and they began to say, "Well, you have to delete this, and you have to change that," and so on. He called me up, and it was interesting. One of the resolutions that came out of this really didn't pertain to the Software Patent Institute. One of the resolutions adopted at the meeting was that the Patent Office must begin to hire computer science people and treat them appropriately, commensurate with all of the engineering types that they have been hiring. Jerry said his lawyers want that deleted; they can't take a position on that. And I said, "No way, Jerry. No way are we going to rewrite history. That resolution came out of the conference. This was the report of the conference. The best I'll do for you," I said, "is I'll put in a disclaimer that you can't take an official position on that as a representative of the Patent Office." So he agreed to send me and Rollie Cole the wording of the appropriate disclaimer. But I had to take, again, a position with respect to his lawyers, the same kind of position I took with the Software Publishers Association - we must be an independent organization. We cannot accept direction from any group, whether it is the government or the industry. Another thing, I got a call from Tom Lemberg again. They were concerned about some of the resolutions. They were concerned about how we were going to get people to contribute stuff, and so on. Mainly, he was concerned about when all of this would go to the media. What would happen when we go to the media? And he said something like, "How will people understand? This is only the first meeting." I said, "Tom, wait a minute. This is not the first meeting. This is the last meeting - of that group. Once we set up an executive committee, we've got a whole new ball game. The executive committee is going to determine everything. And the report of that meeting is input to the executive committee; it's not binding at all. Of course, most of the things that came out of there are what we want to do, but what we are going to give to the media is the report that says: "Here is what this group of people who came together said. That's it." And he was satisfied. And I put in again, into this report of the meeting, the statement that this is good input to the executive committee, but not binding, and so on. It is very interesting how everybody has his own axe to grind, his own agenda - not necessarily a hidden agenda, but his own reasons for supporting this, and concerns about what will happen, and so

on. I did get a call a couple of days ago from a reporter from *InfoWorld* who had heard about this. He had called Jerry Goldberg, he had called Rollie Cole, and they both pointed him to me. And I think that's what is going to happen. Everybody is going to say, "Call Galler." And we'll get a consistent story.

I was interviewed by this reporter. It was interesting that he was concerned about the secrecy about all of this. I said, "There's no secrecy. As soon as we get a report that everybody agrees on as representing what happened at the meeting, it will be available. I'll send it to you." "Oh - that's wonderful." There is all kinds of distrust everywhere. And the only way to get across what's really going to happen is to be open about it.

That reminds me about one little incident just before the meeting. I was talking to a lawyer about the Lotus case, in New York, who was not involved in this meeting at all, but he said, "There are all kinds of agitation in the legal community here about your proposal. People are wondering what's in it for Galler? Why's he doing this? What are his motives?" And that's the kind of environment we've got to deal with, everything is political. Of course, there's a great deal of money involved here. There are millions of dollars at stake in all of this stuff, and I can understand it. But everybody is looking for hidden motives, and I keep telling people that once this thing is set up, I'm going to pull out of it. I have no interest in it; I have no desire to build my career on it or anything else. I want to see it get started because it is needed.

E. GALLER: Through the years, you've received some very nice honors. Talk about those.

B. GALLER: Well, we've already talked about some of them, like the trip to Australia as their Overseas Visitor. There were two that I haven't mentioned, I think. I received in 1980 the Distinguished Service Award from ACM. That was, I think, triggered by the establishment of the *Annals of the History of Computing*. I was the Editor-in-Chief. It was established in 1978-79. But I think the ACM was recognizing at the same time the fact that I had been a president of the ACM and very involved.

The second award was the Distinguished Service Award for AFIPS, which is the American Federation for Information

Processing Societies, of which ACM is one member. And in 1984, I got their Distinguished Service Award, I guess mainly for the *Annals* again, which by that time had become established as *the* journal in the history of computing. I had also actually been on the AFIPS Board of Directors while I was ACM president, so I had some involvement along those lines. I remember the AFIPS presentation was in Las Vegas at a major annual meeting. AFIPS at that time was running among the largest conferences ever held in the United States, I think between 80,000 and 100,000 people. And Enid was with me at that time, and some of our children, I'm not sure ...

E. GALLER: Bruce and Grace were there. I think Glenn was also there.

B. GALLER: Right. And that was a very nice occasion. Actually, I remember at that meeting, Enid and I had breakfast with John Akers, who was president of IBM. And that was interesting. Those were very nice honors to have received.

E. GALLER: Now you are looking forward to retirement in two years. Talk about what your retirement plans are.

B. GALLER: Well, people sometimes ask: "How come you are retiring, and what are you going to do?" and so on. The reason that I have told the University officially that I will retire in two years is because I have so many other outside activities, such as the Software Patent Institute, which is going to start taking time. The Intelligent Vehicle-Highway thing is part of the University, and I intend to find a way to continue that activity as a consultant or whatever. The main reason that I have decided to retire at age 65 is to have more flexible time to do all these other things. I think I have contributed enough to university committees. I would just as soon pass that on to other people. And while I enjoy the teaching, it gets difficult because of all these other activities to say I will be, like this fall, teaching a class at 12:30 Monday, Wednesday, and Friday every week. And it just makes it difficult to do all these other things that I am involved in. So I will retire from the University. I will find a way to continue the Intelligent Vehicle stuff and everything else I'm doing. In no sense am I retiring to stop doing things. And I only hope that everything keeps going at full speed the way it is now. I've got more than I can handle right now, and I'm looking forward to a very active retirement. It's too bad I have to call it retirement. It's just a change, that's all. And I

guess it will give us more flexibility so we'll be able to spend larger blocks of time up north at Sutton's Bay. We'll be able to travel more because I can probably schedule commitments from all these outside activities more flexibly. But that's about the only change that should happen, and I'm feeling very good.

I just said I'm feeling good. I might mention that I really appreciated a compliment a young lady paid me at an airline ticket office recently when I wanted to buy some senior coupons, which are available if you are 62 or over. She asked me for proof of my age because she didn't think I was 62. I thought that was just wonderful.

One other activity I haven't mentioned, and I hope I finish it before I retire, is the book I have started, which is, I would say, 80 percent finished. I just haven't had a chance to finish it off, and that is a book on computers and copyrights and patents. I've learned so much about the law in intellectual property that I thought I could contribute by writing something that would explain what's going on to lawyers who are not involved in intellectual property, and computer people, on the other hand, as to just what is happening in copyrights and patents and intellectual property from my point of view, which is different from a lawyer's point of view. And much of the book is written. I keep updating it, because things are developing, especially as we have just seen in the patent area. There are also some interesting things happening in the copyright area. The recent Supreme Court decision in Feist v. Rural Telephone Company broke some new ground with respect to the copyright of compilations of names and addresses in the white pages, which has lots of implications for other kinds of copyright, and I've just incorporated that into the book. I need to find a block of time to finish it. I hope I finish it before I retire. If not, certainly then I will do it. But I think I should be able to do it before then.

E. GALLER: Even though you are not yet an emeritus professor of the University of Michigan, could you now, looking back, not counting the two years ahead of you, but looking back on your career as it so far has been spent, was it a good choice to become a professor here? Do you regret not becoming some other professional? Do you regret not being a lawyer or something else?

B. GALLER: No, I have no regrets. First, I've enjoyed being a professor. I have enjoyed the teaching; I've enjoyed

working with students. I've enjoyed the position of a professor in our society. It is a respected position. One reason I have been successful as an expert witness is because of my credibility because I'm a professor. I've enjoyed The University of Michigan. It is a wonderful university. I think we have far less politics in the University than many other places. People are honest and have integrity, and it has been excellent.

Now, would I do it again? I've often thought, now that I know so much about the law, that if I hadn't gone into computing, that it might be interesting to be a lawyer, to go through a legal career. But I've got the best of both worlds now. I'm very involved in the law in a role which gives me a chance as an expert to give my opinions; a lawyer working on a case can't inject his own opinion into the case necessarily. So I have all the fun and none of the routine work that goes into preparing a legal case. But I love the law. I find it challenging. I find the adversarial spirit of it a real challenge. The strategy, the logic ... it's fascinating to me. And so I think I've found the best combination one could possibly have. I've participated in some very interesting research over the years in computing. I've been involved in the history of computing. I think I've carved out for myself, step by step - not with an overall plan, but inadvertently, in a sense, but by taking advantage of opportunities, I carved out a most interesting career. I wouldn't really change anything.

END OF INTERVIEW