An Interview with

ARTHUR L. NORBERG

OH 379

Conducted by William Aspray and Jeffrey Yost

on

20 January 2006

Chicago, Illinois

And

9 February 2006

Minneapolis, Minnesota

Charles Babbage Institute
Center for the History of Information Processing
University of Minnesota, Minneapolis

Arthur L. Norberg Interview

20 January 2006

Oral History 379

Abstract

This oral history briefly addresses Norberg's education and early career at the University of California-Berkeley's Bancroft Library and the National Science Foundation before shifting to provide detailed information on the origins and first two and a half decades of the Charles Babbage Institute (CBI) and Charles Babbage Foundation (CBF). Norberg was the founding director and longtime leader of the Charles Babbage Institute and discusses his early priorities with CBI, and its development as the premier international historical research center and archives repository focused on the history of computing. This includes discussion of personnel, projects, strategies, publications, relationships to other institutions (University of Minnesota, American Federation of Information Processing Societies, CBF, etc.), and other topics.

Subjects: History of Computing, Historiography of Computing, Charles Babbage Institute (CBI), Charles Babbage Foundation (CBF), University of Minnesota, Research, Archives Collection Development, Engineering Research Associates, the Defense Advanced Research Projects Agency's (DARPA) Information Processing Techniques Office (IPTO), Bancroft Library, and National Science Foundation.

TAPE 1 (Side A)

Aspray: Today is the 20th of January 2006. This is an oral history interview with Arthur Norberg. The interviewers are Jeffrey Yost and Bill Aspray.

Arthur, could you begin by telling us about your early life, your parents, brothers and sisters and so on.

Norberg: I came from a working class background. My cousin, who is eight years older than me, and I were the first in the family to go to college. I graduated from LaSalle Academy (in Providence, RI) in 1955; it was the period when physics was the popular science and everybody was either excited positively or negatively about atomic bomb building. I wanted to study physics. My cousin had done the same thing. He had gone into physics as well and had graduated eight years before me. So I had this model in my mind of what you do when you go to college, by watching him. We lived in the same building, but not in the same flat. He was a very interesting young man and I looked up to him quite a lot. We're still close. I applied to join the Naval ROTC program at Brown University. I did not get in to the Naval ROTC program and they didn't tell you why, so I had no idea. I assumed the other applicants were more capable than me. But when I was accepted to Brown and when the time came for putting in the reservation money for the space my father said, "I can't afford to send you to Brown. We just don't have that kind of money." And that was the end of the conversation. I wasn't bright enough to go to Brown, which was only a few blocks away, and find out whether there was any financial aid. It was just not…it was not as big a deal then as it is now. So I didn't and I went to Providence College instead, which my cousin attended also.

Aspray: You were brought up in Providence, is that right?

Norberg: Yes, I was raised in Providence. We lived in what is called Fox Point. It is still a thriving neighborhood although the ethnicity has changed over the period of the last fifty years from what was predominantly Irish Catholic—and we were Catholics too—to Portuguese, for reasons I can't explain. But the Portuguese came over on these very, very small sloops, probably twenty at a time. It must have been a harrowing voyage for them across the Atlantic. They would dock at Rhode Island and they would then settle in Providence. In fact, as the neighborhood had begun to run down, they spruced it up. They made it look nice and it still does. I've been there a few times since and it still does look very nice. The Irish just simply moved out. They moved into the suburbs and they moved into other places as they became more prosperous.

Aspray: Were you a good student?

Norberg: Yes and no. I was a good student in the things I liked, like history and English literature. But not so good in other areas. I went to a private academy as a scholarship student for my high school years. In those days the elementary school that I went to, which was a Catholic school, had nine grades so you went from kindergarten up through the ninth grade and then you went to the tenth, eleventh, and twelfth in high school. Now of course it is a little different, it is eight grades and then four in high school. I was second in the class, there was a young woman ahead of me. She and I both received scholarships to go to separate academies, one for boys one for girls. Mine was a Catholic school run by the Christian Brothers. It was across town. I lived at home. I continued to work. I worked regularly from the time I was about twelve and no one asked me to do anything with the money. It was my money. So there was no necessity for an allowance or anything like that. I went to college and the first year was not a great success. I didn't fail anything, but it was not a great success. And at that time if you entered a scientific program there was remedial mathematics to be taken. You didn't just march into calculus. You had to go back and do algebra and trigonometry again. I got bored with that, so I wasn't doing great. I think I received a D for the math class, two semesters in a row. In the second semester, I would guess, there were forty people or forty-five people in this class, and the instructor went down the rows during the final exam and picked people and said, "I'll give you a C if you just drop this course and drop the science courses you are taking." And he picked the two guys on either side of me. They weren't doing any worse than I was, or any better to be sure. But he didn't pick me and I thought sure my turn was next. It has been one upward climb since then. I learned a valuable lesson from that; luck has a lot to do with what happens. So whatever he saw, whatever evaluation he made, it worked to my benefit. Well, I had done poorly that year.

I was looking for a job for the summer and I wanted to follow in my cousin's footsteps again and work for the city parks department. My family was politically connected. I had an uncle in politics at the time. He was very close to the governor, who had been mayor of Providence. He was going to try and get me in there. My father told him, “Don’t bother I have a job for him.” My father was a person who helped to salvage companies, companies that were going under. There were two partners and one man was the man in the office who took care of the finances and contracting and all that and my father was the man on the floor. He ran the staff basically. There was a heavy emphasis on jewelry, costume jewelry. Balfour, for example, a college ring company, was in the same town. The two men had taken on a small company in that town, Attleboro, MA, that I would guess had twenty people working for it, but it was floundering. And they turned it around. They would make it profitable. Then they left and the owners took over again. But my father took me to that place and he put me working on the floor. I was working on soldering machines. I was working on punch-press machines. He didn’t put me into electro-plating; I suspect he was afraid I’d get hurt. Added to those tasks, I worked in shipping and various other things and he’d go off to lunch with his partner and they would leave me there to deal with the rest of the staff. The rest of the people didn’t know how to deal with me. I was the boss’s son so what do you do with this kid. You don’t want to say anything he’ll take back to his father. So I learned what I would consider now to be the most valuable lesson in my life: that I didn’t want to be in a dead-end job like that. And boy, the next three years of college were just wonderful. Then I decided I was going to go to graduate school. I applied to a number of places and the one that gave me money, enough money to be able to live on at that time, $1,800 a year, was the University of Vermont. They had a second-level Physics department. They had two very good people one in optics and one in chemical physics. I went to work with the man in chemical physics, not that I liked chemistry, but I liked the problems in physics that applied knowledge to the study of surfaces. What happens on the surface when a gas hits it or when two solids come together, and that sort of thing. I was sort of fascinated with that, so I decided to study with him. The optics professor was one author of two of what was then the famous textbook in optics. So he was good to be around too.

Aspray: Now let me understand something.

Norberg: Yes, sure.

Aspray: You'd said that in public school or high school you'd been interested in history and English?

Norberg: English literature.

Aspray: Literature. I didn't hear anything about science. Were you interested in physics at all or were you just pursuing this because it was the family…

Norberg: That is a good question Bill, and I'm not sure I can give an informative answer about that. What I would say is that I was interested in it in the way you are interested in an important topic. Physics was important. I was fairly good at it, at least at the lower levels. I thought that I would be interested in theoretical physics. It turned out I wasn't capable for theoretical physics and even in experimental work I wasn't all that great, but I managed to get a masters degree and a publication out of it. So I had some capability and therefore it wasn't the sort of thing where you'd say, "Oh well I'd really rather be doing history and I'll just go off and do that." To be somebody was to be a scientist, so that's why I was there. I didn't particularly like chemistry but I did like electronics and I took all the mathematics I could. We were at that time taking seven courses a semester, if you can imagine it. In part that was because the head of the Physics Department, as he said, was concerned that if any of us went on to graduate school we wouldn't have the same sort of background that somebody coming out of Princeton or MIT would have. He was insistent that we took more courses, especially the related ones like Mathematical Physics, which was very common in those days. But we had to take all these other courses too. I had to take two years of philosophy and four years of theology. It was a Catholic school run by the Dominicans. We studied every bloody work by Thomas Aquinas that was ever written, including those great big volumes of *Summa contra gentiles* and the *Summa Theologica*. We studied Thomistic psychology, not psychology that was useful. So I learned about the autonomic nervous system and the sympathetic nervous system and things like that, but where the hell they were I had no idea. I did like the history and the English literature courses. Those were the two things that I excelled in I think its fair to say. But they were avocations; they weren't vocations. Physics was a vocation and I was serious about it.

Aspray: And was your expectation to be an academic?

Norberg: Yes, always. Always to be an academic. Very early I had planned to go on to graduate school to get a Ph.D. By my second year, I think, in physics I was already dreaming about that. I wouldn't say I had a good plan for it but I was already dreaming about that and hoping that it would come to pass. After college I got married and we started having children, so there was a necessity to earn money to keep the family together. So when I received my master's degree in physics from Vermont, I was grateful that I immediately was offered a job. I didn't search for it. I was immediately offered a job at a small men's college close by the University of Vermont. So I took the job and I was teaching physical science.

Aspray: What was the place?

Norberg: St. Michael's College. It's an Edmonite college, another Catholic school.

Yost: In your physics program were you introduced to computers?

Norberg: No not as an undergraduate, that was too early. In fact the only contact I had with computers was to look in, from the outside, into one of the service bureaus that IBM was running not far from our house. I was watching the sorting machines and that sort of thing. It was very fascinating but I never went in so I never learned anything about that. When I first encountered computing was in graduate school in mathematics. There was a man in the Mathematics Department who was interested in computing and taught two courses on the subject in 1959, 1960. And I took both those courses. They are not the kind of things you'd take now in computer science. One of them was determinants and matrices, to know how to do algorithms and how to do various kinds of manipulation of information using a matrix structure. The other course was programming, believe it or not. In which we did four different languages during the course of a semester, so we did FORTRAN and we did…Cobol wasn't around yet...we did SNOBOL and we did a machine language… I forgot what the other one was. We had problems to do and he taught us how to do these things. So that was when I decided on my topic for my master's thesis, again still thinking about this theoretical idea that would allow me to become a theoretical physicist. I convinced my research advisor that I wanted to do a theoretical problem using the computer. The university had just obtained an IBM 1620 and they were teaching a number of courses. I did my problems in that second semester on that machine. My advisor had an idea, which I found rather fascinating. There was an experimentalist at Princeton who was doing experimental work on the adsorption of gases on metal surfaces. So my advisor handed me the papers in which the experimental results were listed. I went off and looked at various things and tried to understand how I would do this. The principal theoretical assumption of the adsorption of gases on metals at the time was a thing called the Langmuir isotherm. The presupposition behind the isotherm was that when a gas is absorbed on a metal surface there was a molecular bond transfer so that you only got one molecule of gas per molecule of surface. No second layer of gas molecules would appear until all the first layer sites were occupied. I built a model using FORTRAN to explain this and did a whole series of calculations on the machine of what a theoretical curve would look like for the particular surface and the different gases at different temperatures. So I had a whole series of curves that I had plotted on the computer and I superimposed the experimental data on them. And it was a wonderful fit. So he liked it and we published it. Six, seven years later I ran into him at an American Physical Society meeting, and we were chatting and he said, "Oh by the way, all that work you did is now not looked upon as correct because it has since been shown that the Langmuir isotherm doesn't work. There is a buildup of various levels of molecules at the same site before filling all sites at lower level." So that was the end of that. Of course, by that time, I was out of physics. So I continued teaching.

Aspray: How did you find out about this job? How did it come about that you got this job?

Norberg: Interesting question. The former head of the physics department at the University of Vermont who was then long retired, took a liking to me. He used to come in and run labs just to keep active in the field and I was a T.A. and I was working in those labs with him. He was asked to teach a course over at St. Michaels and he said well he didn't really want to commit himself to that but he had a young man that he thought would do the job for them. It was teaching physical science not physics. And so he brought me over there and I met with the dean and before long I had an offer. Private school, didn't have to answer to anybody, didn't have to advertise; they needed a body and they got one. So I went there. I was very good friends with Dr. Holmes and his wife. He had a forty foot yawl in Maine and I went up one summer, which was a lot of fun. As people in their eighties, they shied away from small children. It wasn't my children they disliked, they just didn't warm up to small children so I didn't go up very often. So anyway, I continued to teach and continued in the Vermont Ph.D. program.

I had talked to the head of physics at Brown when he was on a visit to St. Michael's funded by an American Physical Society program to have an important member of the profession visit smaller physics programs. We might have worked out some sort of a deal. I never followed it up because I got out of physics as you know. I continued teaching and continued raising our two children. Well, over time, I would say over the two years when I was taking courses half-time, I was getting more and more frustrated with physics. I took a course called classical electrodynamics. Alan Shapiro and I occasionally talked about this course, because he took the same course with the same text book.

The difficulty I was having was with my approach not with my mathematical ability it seems, because I wanted to see a physical situation that I was trying to solve like a Newtonian problem and in classical electrodynamics that's not what happens. You're talking about many dimensional spaces, many different kinds of interactions, which are multidimensional. Since I couldn't get a good picture of this in my mind I couldn't solve the problems. So I went to the professor and I said, "I'm not doing well here. I can't do it." So they got me through, he helped me a lot. He was a theoretical physicist himself. And then I started talking about being a theoretical physicist and they very kindly, very politely told me that they didn't think I was ever going to make that. Fair statement. Then I started taking quantum mechanics and that was even worse, because I was still looking for this physical reality using these wave equations and they have no basis in physical reality. So as I said, I was getting more and more frustrated so I decided to leave the field. That would have been in 1963. After two years of teaching, I got a job at Westinghouse in the naval reactors program. After nine months I knew that was not for me. I was not interested in working for industry and I went back to teaching at St. Michaels after fifteen months at Westinghouse. I learned a great deal and I enjoyed some of the problem solutions where we were looking at neutron diffusion studies in working reactors. I had the responsibility of plotting the activity of an active computer in a submarine against the theoretical plot that had already been made about how that reactor was going to perform over the years. If anything ever went wrong then we would know it by comparing the actual operation of the reactor with the theoretical one. At least, we'd have an early indication of potential trouble. So that was my responsibility and I did enjoy that. In those days, the Navy was not going to accept a reactor until somebody at the corporation had actually made it work, made it work for the first time on the boat, as submarines were called, and was attested to be at least safe in the early operations. Four of us went out from the company as a team to a naval construction yard, two experimentalists and two men from the diffusion study group. We worked twelve hours on, twelve hours off while this testing was going on. It wasn't a hard job because the real work was going on down in the boat in the control room and I was upstairs just checking the readings to compare with the plot made at the company. I went on four submarine criticalities, the John Adams, John C. Calhoun, the James Monroe, and the fourth was a hunter submarine the Dace. When we were in the Newport News Shipyard, Newport News Shipbuilding Company in Virginia, I was up on the deck of the submarine where there was a little hut that was just sitting there for the work activities and I was waiting for the information to come up over the speaker about what the rod bump had been. I would plot where it was and see whether it was in the predicted range for that reactor. Back at the plant I had a very easy job, so easy, so easy that I would come in, in the morning and I would do what they call in the Army, "get your display out." I'd open the top drawer of my desk and I'd put my stuff on the top that I'd put into the drawer the night before and then I'd go off and have coffee. At 11 o'clock one of the other guys and I would go swimming at the YMCA in Keysport, Pennsylvania. Then we would go to the dining room in the Hotel Keysport and we'd have lunch, very cheap. Then we'd go back to work. And in the summer time, at 3:30 we'd be out playing golf. I said to myself, "What kind of life is this? I can't do this. This is not for me, I need to work." So that's when I decided to go back into teaching. So I went back and the question was should I resume the Ph. D. program. I was still determined to get a Ph. D. and it just wasn't working, so I quit, I just left it.

Aspray: You did get a master's degree?

Norberg: I did get a master's, yes. I had already had that before I was even in the Ph.D. program. In fact I don't think they had a Ph.D. program when I went there. It was in the works so that if it hadn't been approved I would have had to go somewhere else. There were twelve of us in that graduating class from Providence College, twelve physics majors, about four of whom were veterans from the Korean War. So it was a lively group and an interesting group but only two of us went on to graduate school. One went to Rensselaer and I went to Vermont. Similarly, that other fellow didn't get a Ph.D. in physics either; in fact I don't know what he got the degree in. So there were the twelve of us going out into work and education. When I got to the University of Vermont there was only one other student and he was having emotional problems. He was on antidepressant drugs. Oh, it was a real trial to share an office with him. Then somebody else came in to make three. It was not a good group. In fact they dismissed the third guy. Then the problem fellow went back to a group home at some point. It was tragic but… so I was really by myself. There was no way to talk to anybody about the problems I was doing. It was a struggle I have to say, so I was glad when it ended. I battled around for a year; I was teaching, of course and family life was taking up some time. By that time, we had a third child, my oldest boy, and so three kids to be handled. But I was still determined that I wanted this Ph.D. degree. I had a close friend in the philosophy department who was interested in the history of philosophy and there was a fellow in the history department who had come from Cornell and had taken some courses from Henry Guerlac and I've forgotten who the biology person was then…an older man. And so I would have lunch with these two men practically every day. So I mentioned that I was interested in history and the historian said, "Well, it seems to me that you would have a good background to be able to do this through science." Ok. And then my friend in the philosophy department encouraged me. He used to come over to our house for dinner and we would talk after dinner while Joan put the children to bed. They convinced me that this was an option. Well, what to do? I was reading H.G. Wells, *The Time Machine* and on page 3 or page 2 whatever it is, there's a comment about the famous astronomer Simon Newcomb and something he had said about time and motion. And I thought who this Simon Newcomb? I'd never heard of him. I knew about Chandrasekhar and Schwarzschild and I had heard of Russell and a few more astronomers, but I'd never heard of Simon Newcomb. Who was that? So I started doing some background reading. And I found out that Newcomb published 500 papers and six books and had a whole series of mathematics education books for elementary school and so on. Really a very accomplished astronomer, he was interested in the old astronomy, not astrophysics. So I decided I'm going to write an

*Revised 2014-01-10 to correct spelling of Henry Guerlac, p.15.

article on this fellow and I'm going to send it to the *Scientific American*. Talk about over confidence. So I wrote this draft of the piece and fortunately I had the presence of mind to give it to my two friends to read first. And I got no answer. They didn't say anything. We were having lunch everyday. There was no reason why it should be held back. So finally I brought the subject up and they said, "Well, we've been a little reluctant. We didn't quite know what to say. There are problems with your manuscript." "O.K., what are they?" "Well, first of all this is not a paper in history. You're judging this man by your standards not by those of his time." Newcomb had died in 1909 and this was about 60 years later. So they thought that I needed to get those skills about methods and approaches and analysis in historical themes. So I said, "Well, fine, how do I get that?" They said, "Well, there are two ways you can get it. You can start reading, we'll give you a list of titles and you can start reading, but the most efficient way is to go back to graduate school." So back to graduate school. I applied to three places. I applied to Harvard; I applied to Cornell—I had an in at Cornell through this fellow who was in the history department at St. Michael's—and to Wisconsin. And the three were chosen because of the faculty there. One of the other men in the history department was a man by the name of Edward Phiffer, who had worked with Donald Fleming. He gave me an introduction to Fleming. Then my friend from Cornell sent an introduction to Pierce Williams. The only other place that had anybody working in an area that was close to what I wanted to do was Erwin Hiebert at Wisconsin. So those were the three places that I applied. Well, I went for an interview at Harvard; it was a very contentious interview. Fleming and I did not get along. When he suggested things that I ought to do, I said, "No, I don't want to do that I'd rather do this," and so on. Well I didn't get a favorable recommendation so that was the end of Harvard. I went to Cornell to see Pierce Williams and we had a very good conversation. We got along well. He's just like me, he's aggressive and approaches things in an open manner, says what he thinks and I admired that. We had a conversation and again he said you know there are more important things to do before you do Newcomb. He said, "I won't try to talk you out of doing a study of Newcomb and the old astronomy but it seems to me that somebody needs to look at Peirce beforehand." Because Newcomb was working with Peirce and was working on the same sort of problems with the same methods that Benjamin Peirce had developed and so maybe it would be a good idea to study Peirce first. Well, what frightened me about Peirce, and I told him this, was that Peirce was more involved with mathematics, theoretical mathematics than he was with applied and I'd already had this bad experience with theoretical physics, so I was gun-shy at that point. I really didn't want to take that on. He said, "Well, o.k." So then we talked about finances. Since this meeting was taking place in August, I think, he said, "There's no money now. It's all been dispensed but if you can come on your own for the first year—and you do well—I'll see to it that you get three more years of support. I'll work your ass off but you'll be out of here in four years." I thought that was a wonderful idea but I didn't have the money, so crossed Cornell off the list. Then I went to see the people at Wisconsin and that was a mixed interview I would say. It wasn't clear what would happen, but there was a job close by at the University of Wisconsin, Whitewater, 50 miles away, teaching physical science and astronomy. And I got the job so I moved to Madison. I was teaching there full time for the first year, just teaching, and then I applied to graduate school as an in-state student and I paid my own way. I worked full time for the first three years and took courses half time and applied to the NSF for a faculty fellowship and received it. So I was maturing, I was getting better, I was able to argue my positions better and I was learning history of science so that it was working out. And then Hiebert left; he received a call to Harvard. And that really deflated me. I liked Erwin a great deal. I really did want to work with him, he was interested in Newcomb. In one of his classes, we had looked at J. B. Stallo's *The Concepts and Theories of Modern Physics* and Newcomb's criticism of Stallo. Well then Dan Siegel came to Wisconsin. Siegel was younger than I was because by that time I was thirty. He was younger than me. He had only worked with Martin Klein at Yale for a year. He was a physicist. But he turned out to be quite good and he was very helpful to me in the end. So I had misjudged him in the beginning. The way he set out—he claimed to know little about astronomy and that was especially true about the old astronomy. What he knew was in astrophysics. He'd worked on Maxwell and had worked on problems in electrodynamics and so on. He had this knowledge of some nineteenth century activities. So when it came time to organize my thoughts about my dissertation and the research problem, he said, "Well I think it might be good if you could give me some lectures about astronomy, about the background to what you're going to do." I thought, "God, this is going to take me even more time." But it was a godsend, he was right. Whether his reasoning to me or his statements to me were what he really believed or not, I don't know, but it was a wonderful exercise. By the time I got through lecturing to him about the old astronomy, I knew enough to go on and look at the nineteenth century figures. At that point the dissertation problem both grew and shrank at the same time. It grew in the sense that it went beyond Newcomb and looked at nineteenth century planetary prediction theory. I looked at the astronomers in England, in France, in

Germany, as well as in the United States, but it shrank in that I had planned always to do an intellectual biography of Newcomb, which ultimately I never did. So I dropped all those things by the wayside and Bert Moyer, who followed me in that program, quite serendipitously took on some of those topics and worked with Dan Siegel on Newcomb and others. So Siegel's first two students were working on Newcomb and related figures. I got my degree and I applied for various jobs. One of them was at Berkeley. I thought had no hope of getting it. I didn't think that was going to be a possibility. The position was not on the faculty, which was the only reason I applied. I don't think I would ever have had the gall to apply to the faculty. I didn't feel that strong. I interviewed at the AAAS meeting in Washington in 1972 with John Heilbron and Roger Hahn. Heilbron and I were facing each other and Roger Hahn was sitting over behind me to the right so I couldn't see him, occasionally he'd ask a question. That was very disconcerting. It just so happened that when the meeting was over and everybody was returning to their home base, Heilbron and Siegel were on the same plane together to Chicago. Then you couldn't fly cross-country you had to stop in Chicago or Dallas if you were going from East Coast to West Coast. And so they were on the same plane together and there was a delay in Chicago, neither one of them could get out. And Dan told me afterwards that in the course of those eight hours they were together the subject kept coming back to me and John Heilbron would ask more questions. I didn't know what to make of that. I was still at the Smithsonian on a fellowship and so I was flying back and forth to Madison and staying twenty days/twenty-one days in Washington and ten days back in Madison. And doing research at the Library of Congress, that was the reason for going to the Smithsonian in the first place, Newcomb's papers were at the Library of Congress. And I was making reasonable progress I would say. It wasn't swift, but it was reasonable. I did get the degree in five years start to finish but the first three years were half time and I was teaching full time. The fourth year I was at the Smithsonian on a fellowship, so I was working on my dissertation problem full time. And then when I went to Berkeley I hadn't gotten my degree yet and I was working full time there, so I was just doing the dissertation at night. So it was a five year program but I worked four years in that time.

I was called for an interview to Berkeley and of course I was thrilled but I was equally nervous at the same time. I went out there and Roger Hahn picked me up at the airport and was cluing me in as to what was going to happen. And when we got to the hotel I said I want to change my clothes I don't want to go to visit the director of the Library this way. So I changed my clothes, he was there and we were talking through the doorway of the bathroom I guess. And he said, "By the way, this job is yours to accept or reject and so in a sense you're interviewing us, it's not the other way around. We've decided." Well it turned out I wasn't the first candidate. The first candidate that they wanted was an historian of science at Montreal. What was his name? I can't remember; it'll come to me. He was and is an historian of physics and Heilbron liked him and they got along very well and so on. But he didn't want to take a non-faculty position so he dropped out and that left me second in the line. So that's what Roger Hahn was telling me. So I went and I met the head of the library, I don't remember meeting any other staff members in the library, but I met the head of the library who was a professor of English. He had been vice chancellor under Glenn Seaborg, when Seaborg was chancellor, so he had lots of connections on the campus. He was a wonderful fundraiser and taught me quite a number of things about fundraising in the course of my five years there. And he was well connected in San Francisco and Berkeley. His wife was a member of the MJB coffee company family and his father had been a silk importer from the Far East, so they had plenty of money.

Aspray: What was his name?

Norberg: James D. Hart. Hart wrote a very famous book in American literature which is now in its eighth edition. Hart had done work on Melville. I was really taken with Jim Hart. I really did like him. He was not a likeable fellow, but I liked him. That might say something about my personality. And he was always good to me though he demanded a lot. He was a very firm taskmaster. He was always editing my work, which made me a little annoyed at first, but it was always better when he got through with it. So I took that job. It was a three-year contract. There were no guarantees that when the three years were up there would be any money. The money had come from Hewlett and Packard as individuals, not the Corporation. They had only one objective in mind and that was to get an interview of Fred Terman into the archives. That's all they cared about as long as that was done, Berkeley could do anything else with the money they wanted in the project. So I did that. That was the second oral history interview that I did in my life and I think it was a decent one. Terman was a very cagey fellow, and, like Glenn Seaborg, whom I interviewed later, these people didn't betray anything that they didn't want you to know. They were very good and very careful. So I didn't learn a lot from either one of them in terms of the techniques of oral history. I did learn a great deal while interviewing others.

Aspray: Before we go any further, how good a student were you at Wisconsin?

Norberg: I would say very good. I had learned a great deal of history in the interim between my undergraduate and graduate years. I would like to think I had a flare for the field, but that may not be a fair judgment for me to make at all. It may have to be made by others. It probably does have to be made by others. So I really, I don't know, but one of the things that I do know is that the assignments that I had to do and the dissertation itself were reasonably creative. For example, I took a course in American intellectual thought, a two-semester course in the History Department at Wisconsin. And the man was an American intellectual historian, a very accomplished one, Paul Conkin. I was taken with his books. If you read his book *Puritans and Pragmatists* and read our DARPA book you can see the influence of Paul's writing on mine. I took him as a mentor and he seemed pleased with that. He made all these original assignments; they were just marvelous. When we studied the puritans at the outset of the class—(There were some interesting people in the seminar and I'll come back to them in a minute) when we studied the puritans, he asked us as the first assignment to write a puritan sermon. I just loved that; I thought that was a great idea. I wrote an essay, a sermon, entitled the objectification of evil is the only evil and carried out an argument that I thought was reasonable. He gave me an A so I guess he thought it was too. The second assignment was to evaluate the Constitution—because we had been through the revolutionary war period—and decide whether it was a good document, if it was, why was it and if it wasn't, how would you change it. I got off on the wrong track right from the very beginning of the assignment: I assumed it had to be changed. So I rewrote the constitution with all sorts of new mechanisms and so on, changing terms of people in the House of Representatives and changing the relationship with the Supreme Court all that. I turned my evaluation in; I was very frustrated in doing it; I was up all the night before trying to finish it. I threw drafts away, but I worked hard at it and that was the difference between the undergraduate and the graduate experience. I worked at it; I worked at it until I was satisfied with it. I turned it in and I got it back. He went through point by point and showed me that you didn't have to change the Constitution to achieve those results. You could do it by the general law making process. Oh, I was deflated. Nancy Cartwright was in that class. Do you know Nancy?

Aspray: I know who she is.

Norberg: She wrote an essay that it didn't need to be changed and of course she was right. We talked about the evaluations in class and Paul had her lead the discussion. Jacobs…what was Jacobs first name…was in there. He was a passionate observer of the UFO movement. After he got his degree in the History Department, he joined the Northwestern history department. He's written several books on the UFO movement since, and he was a consultant on…The Third Kind…What was the title of that movie?

Yost: *Close Encounters of the Third Kind*

Norberg: *Close Encounters of the Third Kind*, yes. His book was used as background for some of the scenes in that movie. There were really good people in that seminar. I did enjoy that but the third assignment and I'll stop with that, I won't go any further. The third assignment was to write Paul a letter on why we were or why we were not a pragmatist. I described the theory of evolution and how I came upon it and what I had thought the implications of it were and how it attacked a number of my earlier assumptions and so on. I still have the essay, those two essays. I didn't save the constitution one. Every once and a while I come across them in my files and I would read one or the other of them. They were very well thought out I have to say. I enjoyed doing them and I enjoyed doing my dissertation. I enjoyed the research. I finished the research and went on to defend it. I learned another valuable lesson during the defense. Paul spoke up after the questioning period had been going on for a while—Paul spoke up and he said to the other members in my hearing, "I can't judge whether this is a good dissertation or not because it's too highly technical for me." So he said, "I need to hear opinions of you people from HST as to whether you think this is creditable job." Which was a really devastating question in a defense, because if anybody else had spoken up in a negative fashion who knows what would have gone on. And so, there was dead silence in the room. Nobody spoke. Which made me think nobody read it, except Dan Seigel maybe. Well Dan being the advisor didn't speak up as is customary and appropriate. So finally Bill Stahlman—who was an historian of classical astronomy and somebody whose work I admired spoke up and said, "Yes, I read it and it's wonderful work, it's ideal. It's an important dissertation." And I thought, "You hypocrite, you never read it." But I wasn't going to criticize Bill, he was getting me off the hook! [Laughs] And so that was the end, they voted and that was the end of it. I got my degree. What I learned from that was that I probably wasn't a very good judge because I didn't speak up and say, well yes if you look at this and this and this that really is what's going on. And at the end of the dissertation was the long disposition about the professional community in the United States at the time and about their arguments. I was now criticizing some of the arguments of people in their own time, not in mine. I'd learned that from the earlier Newcomb business, but even I didn't speak up. And I didn't speak up it turns out because I wasn't sure it was right either. I thought I'm never going to let this happen to me again and so that's why I think that my five years spent at Berkeley was my coming of age as an historian. Working with Heilbron, working with a couple of other people like Charles Kittel in solid state physics, and doing the interviews taught me how to do history.

TAPE 1 (Side B)

Aspray: Other than Paul Conkin and Dan Seigel were there other people on faculty there or actually, other historians other places, or scientists other places who were shaping influences on your career?

Norberg: Yes, there was at least one other faculty member in Wisconsin and that was Robert Stauffer, the historian of Darwinism and evolution. I took his seminars and even gave some thought at one point to doing some work in the history of evolution. By the time I finished my dissertation and saw who else was working in that area I decided that I couldn't really contribute anything without having to do more biology study. But I liked Stauffer's approach to the study of the biological sciences and I learned there that the definition of a biological problem is different than the definition of a physical problem. I had not known that before, because up until that time I had never studied any biology. Now you can't get out of high school without a course in biology, but then you could. So my first introduction to biological topics and evolutionary theory was through Stauffer. And, of course, I was married to Ann at the time who was a microbiologist, so we had a lot of discussions about biological problems, but I didn't understand how to do it historically. It was Stauffer that taught me this difference.

There was Nathan Reingold, of course, whom I went to work with at the Smithsonian, when I received a fellowship to go there to do research at the Library of Congress. So I spent ten months with Nathan. I actually shared his office because they didn't have enough rooms to put me somewhere else and he had this great big room in the castle. He gave me a desk at one end and there was only one condition, if he asked me to leave the room when he was on the telephone or visiting with someone I would have to go. And that was fine, it only happened twice in ten months. As I said, having watched Paul Conkin and seen how Paul did his thinking before writing, I looked for Nate's style. Nate had an entirely different approach. Conkin was a straightforward researcher; he did his research, did his thinking about the argument and then wrote the materials and revised and revised and revised until he had it in the right form. Nate didn't do that. Nate did his reading and I would see him; he'd be getting books all the time from the Library of Congress. He was reading articles and looking at original materials, since he was working on Joseph Henry for the most part at the time. They had all the Henry Papers at the Smithsonian, and he'd be doing research in those, either the papers themselves or the microfilm copies that they were making. And then he'd wander the halls—walk up and down and up and down—and I used to think, "My god, he's wasting an awful lot of time." Then he would sit down and he would dictate a manuscript. Or, he'd go home at night and he would dictate and bring the tape in the next morning for the secretary. And it was done. It was done! And I thought, "My god, I can't do that. That's not my style." But I ended up mixing the two because I saw how Heilbron worked after that, and Heilbron was even different from both of them. Heilbron could write much faster I think and that was a lesson to me as well because I can't write fast, I'm very slow. But when I've finished the first draft, the second draft is easy, easier anyway. Whereas with Conkin he was really still laboring over the third and fourth draft to get it just the way he wanted it. But I mixed these two approaches, so you'll see me walking around the suite or building, walking outside, taking a walk across the campus. So I found that reflecting on these topics was useful in the way that Nate did it, but I couldn't then sit down and dictate the result. I had to do it Paul's way. Sometimes I have to go over and over the materials, the original materials as we're doing now (at CBI) with the IBM project. I'd read some of those *Rochester News* issues four times, and each time I look at them I will find something new that I want to use. I have to do that. I'm not that fast at writing. So these were the influences on me. It was mostly observation as opposed to training. When I got to Berkeley, the expectation there was that I would do the interviews and that I would do them in the history of physics. It was that I would interview all the Nobel Prize winners in physics on the Berkeley staff and then whatever else seemed to be appropriate, bring in collections just like we do at the Babbage. And then do this interview with Terman. Well, the first thing I noted was, I'm tired of doing physics, tired of doing astronomy. It had been a long haul for ten years and I was sort of worn out with it. But I had to get some publications out. I didn't feel I could just sit there and not do anything because three years was going to be up very fast. If I didn't have any publications or at least something on the way I wasn't going to get another job in academia. So I started working on a piece for *Isis* and I started working on a piece for *Technology and Culture* (T&C). The T&C piece never was published. It was never published for two reasons. One, when the reviews came back, Mel Kranzberg was the editor then, when the reviews came back, one reviewer wanted me to enlarge the piece, double the size of this, "Norberg knows more about this than anybody else, blah, blah, blah." I thought, "Well, OK that means more work but…I let that go. Then I read the second one and the second one was, "This is too long, it should be cut in half, emphasize this, but drop that" and so on. I was bewildered so I talked to Mel on the phone and I told him about the diametrically opposed opinions of the two reviewers. He said, "Well, just do what you can with it and send it back and we'll see what happens." Well, I stalled and stalled and stalled. In the meantime Hugh Aitken got on to the topic and he wrote *The Continuous Wave* on the subject. One chapter includes what I was doing and, indeed, he used some of my materials, with agreement. He did it far better than I could I thought. I don't think that right now, but at the time I believed that. He had taken an interview that I had done with an electrical engineer (Leonard Fuller) and he got more out of it than I thought was in it and did a wonderful job on this man's contributions to the field. So I just left my manuscript in the drawer and that was the end of it. The other day I threw it away when I was cleaning out my office in computer science. However, the United States Bicentennial brought another opportunity for publication. For the celebration, the IEEE wanted an issue in its education discussing hallmarks in the development of electrical and electronic

*Revised 2014-01-10 to correct spelling of Hugh Aitken, p.28.

engineering. I prepared the article in my CV on electronics on the West Coast, before the computer.

In the meantime, I prepared the piece for *Isis*. It was the second chapter of my dissertation. I sent it in, the reviews came back. Same problem. "There are two ideas in here, it should be split up into two papers, maybe the second one shouldn't even be published." I thought…what am I going to do now? But it was all right. There were two themes in the work and I didn't see it. It works fine for a chapter in a dissertation but not as a publication. So I just excised one half of it and took it out of there altogether. The other half was published in *Isis*. Both of those pieces were submitted in 1976, one of them never appeared as I said, the other one appeared in 1978 by the time the review process was over.

In 1976, the American Chemical Society was celebrating its hundredth anniversary. Glenn Seaborg was the incoming president and I'd already done fourteen hours of interviewing with Seaborg. He called me—and I learned later he had already talked to Jim Hart—he called me to see if I would participate in the preparations for their celebration. They had a celebration in Philadelphia, which is where the society was founded during the Centennial Exhibition and did a lot with Priestly and Franklin and those people. Glenn had the idea the society ought to celebrate the anniversary on both coasts, because he wanted to brag about Berkeley and California chemistry. The incoming president that followed him, several other chemists, and I were on the committee to do something to be opened at a dinner in Berkeley. We generated an exhibit that was both interactive as well as historically informative. The exhibit was mounted in the Lawrence Hall of Science for something like six months. I wrote all the companion pieces to it: a booklet that was given out at the dinner, a brochure that was given to people who came to see the exhibit. We had a postcard that they could use as a bookmark that had a picture of G. N. Lewis on it, which the chemists loved. So it was a good experience. But then *Chemical and Engineering News* approached me and asked if I would prepare an article on this subject of chemistry in California and by the way we need it in thirty days. So it was a last minute thing for them. Well, I'd already done these brochures and we'd done the storyline for the exhibit so I thought, "Well, yes, I should be able to do that." Well, I finished it on the very last day. I was taking my children to Yosemite and they were waiting outside for me to take them off in the car while I was finishing this piece. This article on chemistry activities west appeared in August of 1976. Thus by the time the Berkeley position was to be up. Jim Hart said—and I don't want to say that this [was] a conclusion of mine—but Jim Hart said to me that I had done such a good job at getting interviews done and getting collections in, and it pleased the Nobel Prize winners so much, especially Seaborg, that he had gone to the chancellor to get my position firmed up. So I was offered a continuous appointment at Berkeley in the library, no teaching. I wasn't asked ever to teach and I didn't have much interaction with the faculty anyway except for the history of science people and the people in the nuclear sciences. So I went out and hired somebody else to do the physics and I went on to electronics, that's how I got into the electronics game, and published that piece, again in 1976, on the development of the electronics industry on the Pacific Coast. If you look at those four, there's one in astronomy, there's one in chemistry, there's one in electronics, and then later on there's a couple of other small pieces on other activities in astronomy. It's a very disparate set of publications so that by the time I was thinking I wanted to get on the market again and I was trying to leave Berkeley, I really didn't have a lot to offer a history program elsewhere. In spite of having I think five publications at the time and one or two more in the works, it was not an easy thing to contemplate. "Well, what shall I do? Shall I advertise myself as an historian of astronomy? Shall I advertise myself as an historian of electronics, electrical engineering, or what?" But fortunately I didn't have to answer these questions. I didn't have to apply because I went on leave. I went to the National Science Foundation and then I took a job at the National Science Foundation as a program officer, and later I applied for the job in Minneapolis. And while I was, I would say mildly interested in the job at first, I wasn't sold. I applied because the Reagan people had come into office and Bill Bennett had it out for me and he and his people at the Heritage Foundation wanted my NSF program eliminated.

Aspray: You personally or your program?

Norberg: The program, but maybe a tinge personal. He had telephoned me a few years before when I was first program officer and wanted NSF to give money to the Humanities Center in North Carolina. And I said, "We can't do that. NSF has a policy that we make the choices on who gets fellowships. We don't give money so that you can make a choice." Well, Bennett argued with me on the phone. I said, "I don't have any control over this. This is not my doing." Well, he wouldn't take that as an explanation. When the Heritage Foundation put out their report for the transition committee for the Reagan administration there was a list of programs that should be eliminated, mine was among them.

Aspray: You should say a word or two about what you were doing at NSF, what the program was.

Norberg: The program was called Ethics and Values in Science in Technology. This was the period in which technology assessment had come to the fore. Risk assessment was a hot topic at the time. There were ethical problems that scientists were facing such as those arising in the DNA controversies of the period. And there were problems for engineers, like the BART engineers who knew there was a manual override problem on the cars and nobody would listen to them. Whistle blowers were beginning to be more frequent, they weren't very frequent but they were beginning to be more frequent. The Congress, not the Foundation, and particularly Ted Kennedy, were interested in helping these people understand what the ethical issues were and how they could approach their work by learning something about what ethics is and what values are and how you incorporate them into your thinking. The fellow who had been running that program William Blanpied was promoted in NSF so the job was available. Ann wanted to stay in Washington anyway, so I applied for the job and got it. Just as I arrived, Kennedy had put another bill through Congress for yet another program in ethics and values. The Foundation was not interested in this program. They didn't want the program. One of the principal thinkers behind the program in its original structure with Blanpied was Gerald Holton. There was the original program; there was this new aspect of fellowships that Kennedy and his group had put together with the help of the people at Georgetown University; and then there was a new interactive program with the NEH people. I had no

*Revised 2014-01-10 to correct spelling of William Blanpied, pp. 32, 35.

help with all of this. I had to run all three of these aspects, which meant there were five deadlines every year, two, two, and one for each part of the program. There were ten panels a year that had to be put together. I had a secretary who was not incompetent, but was not dedicated and half the time didn't even show up for work. So I was working from morning until night just administrating awards, I couldn't do anything else. There was no idea that I would do any historical research when I went home. I'd get home about eight o'clock after going in at eight in the morning, and we had dinner and I helped with getting Gregory to bed. So it really wasn't very comfortable, but I did like it. And until I ran out of names for the panels I was doing OK. I liked working with the NEH people.

Aspray: What had brought you to Washington originally?

Norberg: Ann became a Congressional Fellow for the Society for Microbiology. She was leaving for a year and since I had been the one talking up Washington in the beginning I wanted to go with her. In 1976, I tried to get into the Carter administration. Mel Kranzberg was a close friend of Jack Watson who was the head of the transition team and so Mel gave Watson my CV and we talked about…I told him what I wanted, they didn't tell me what they wanted. I thought I might be a good commissioner for the NRC, Nuclear Regulatory Commission, which was a bit arrogant because the people they named were far better than me. I was willing to take a second level position there but that didn't work out because the new appointees brought their own people with them to serve as underlings. And it died, so I never got anywhere with them. But I was still angling towards Washington. Ann caught the bug and she got the fellowship. So I thought I can't let this happen to me. I'm not going to sit back here and take care of the kid and she said, "Then come to Washington, too." So I went to Washington on History of Science Society business and I talked to Diana Menkes. Do you remember Diane Menkes?

Aspray: I don't think so.

Norberg: At the time, she was the managing editor of *Isis* that was before Francis Coulborn Kohler. Diana had the post for a long time. She and I were meeting about the journal, since I was treasurer at the time for the History of Science Society. When we got to personal talk, I told her about this and she said well you know Josh is interested in having somebody in the history of science—her husband—to come for a year in his program in risk assessment, risk and technology assessment. So the next morning I had an appointment with him and before I knew it he had arranged for me to go on an intergovernmental assignment. I asked for leave at Berkeley and they gave me the leave and I went to Washington and we had a comfortable life for a year there. When we were coming up to the end of the year I was assuming we were headed back to Berkeley. One day, Ann asked if I'd like to have lunch with her at the Supreme Court, which was then open to the public. As we left the dining room, she took me on a little tour of the Supreme Court hearing room and so on. Then she said, "Can we sit down for a few minutes?" We sat on a seat in the foyer and she seemed very nervous but I didn't think anything about it. She said, "I'm not going back to Berkeley." Just like that, I'm not going back to Berkeley. And I said, "Why not?" She said, "I have another job." I didn't

*Revised 2014-01-10 to correct spelling of Francis Coulborn Kohler, p.34.

even know she was interviewing. Well, that shows you what was happening in that marriage. She told me it was to work for Monsanto. So I was very deflated with this, that she had not taken me into her confidence. So the question was do I go back to Berkeley or do I stay in Washington? And that's when I actively started looking for a position in the National Science Foundation. As I said Blanpied had been promoted and the job was open and I got it. So then we spent two years more living in Washington. We bought a house in Chevy Chase, MD. It was a really very nice house; we liked it a great deal. The two years went by very, very quickly. Then the Reagan people came in. Of course, I'm not sympathetic to Republican ideology and so I didn't know what I was going to do. I didn't really see four, eight years in Washington with the Reagan crowd around. I'd hate to be in Washington now, for the same reason. So I was thinking what shall I do, shall I leave NSF. At NSF, we were on a rollercoaster, a budget rollercoaster with this attempt to get rid of the program. I'd come in one day and there would be no money. I'd come in the next day and we'd have half of it back. I'd come in the next day it was down to a third. And my boss was angry with me because just like my writing I was slow in getting the grant awards out of NSF. We had a time schedule and I always met the time schedule but he wanted me to get two grants done today, two done tomorrow, two done the next day and so on until you got to the end of the period and then everything was done. Well I always had it at the end of the period; they were all done at the same time. Well, we got caught. We almost lost the money for these grants that had already been approved. So he was angry about that. When my two years were up, or about to be up, and the Reagan people had come in as I keep saying, Roger Stuewer called me and he said the deadline for applications for the Babbage job is coming up very quickly. He called me somewhere around the beginning of February, said the deadline is the twenty-eight of February and we cannot accept any applications after that so I'm encouraging you to apply. I said, "Well, I don't know what I'm going to do Roger. I'm going on vacation. I'll call you when I come back."

Aspray: Did you know Roger?

Norberg: Yes, he was secretary of the History of Science Society when I first became treasurer and Sally Kohlstedt became his successor. Sally wasn't at Minnesota yet. That's how I knew all these people. I was on the Council and I was among the officers in the Executive Committee. So he said, "All right but please don't delay. Call me. I want you to apply." So we went to the Caribbean. We were there ten days and came back and then there was this bloody rollercoaster. I was so tired of it after two weeks that I decided, "Well, ok I'll apply to Minnesota." Academics being how they are it'll take three or four months to get this thing organized and so I'll have plenty of time to decide. And if things calm down here, I'll try to stay—because there was no guarantee of the job at NSF. If they don't, then if I get the job in Minnesota, I'll go to Minnesota. I had not bargained on Stuewer. The application was in right about the twenty-eighth, twenty-seventh, twenty-eighth, it took me a little time to put it all together. March 10th they had me for an interview. I thought it would be the end of April or something. I looked at the other applicants, the other people they were interviewing and I thought, "Oh this looks like a set up to me. They don't want these other two." The other two applicants were not historians of science. One was a policy person, Seymour Goodman, the energy policy computer scientist, and Charles Dollar from the National Archives. Dollar went on to be a really good thinker about digital archives. He's still at the archives in management. The Minnesota faculty, I assumed, favored somebody in the history of science. I thought these other two candidates can't be it. A faculty position could be had in the library—Minnesota still had a library school then—and the other one is clearly going to be in computer science. I was sure Stuewer didn't want that. So I thought, "Oh boy, this is going to go faster than I thought." Well by the first of April I had an offer and it was a good one. I was pleased with it. It was a fifteen percent increase in salary over what I was getting at the Foundation, which was a supplemented salary for living in a high cost of living area. So it was higher than my Berkeley salary and they then put fifteen percent on top of that and gave me some nice perks, as well. There were no-start up funds at that time that was just not done yet. So suddenly I had to make a choice. I went in to see my boss at NSF. Let's see what was the chain of command, there was my boss, who was a program director, there was his boss, who was an assistant director, which was a presidential appointment, and then there was the deputy director and the director. So that's the chain of command we're talking about here. So I went to talk to him about this and he said, "Before you start I have something to tell you." And he hands me a letter, offering me a job at the National Science Foundation, a permanent position. He didn't want to let me go in spite of the fact that he didn't care for some of my methods.

Aspray: And was this just coincidental or did he know?

Norberg: Yes.

Aspray: It was coincidental.

Norberg: Well, I don't think he knew anything about Minnesota because I hadn't said anything to anybody. So if he learned it, he learned it some other way. But he knew I was coming to the end. I would have had to get out of there at the first of September so what was he going to do with the program? So I'm sure he was thinking that he wanted to keep the person he had rather than take on a pig in a poke. He was an economist, a rather good one I think, and his boss was an economist so they couldn't just easily take this program on. They had to have somebody who had some knowledge about the area. So I don't think it was coincidental; it was serendipitous. I think it fit at the right time because then I had a choice and with a choice I could make a good decision I thought. It still took me until the end of June to make a decision and part of that had to do with my personal relationship with Ann. She wanted to stay at Monsanto, which led to a major disagreement. When I accepted the job at Minnesota, the personal problem was still unresolved. I should have taken care of the personal problem first, but I was being pressured to answer. So I took the job at Minnesota and left Washington and I have not been sorry.

Aspray: Did you consider going back to Berkeley?

Norberg: I didn't tell that part of the story. I had leave for a year and when I was…I was asked to stay at NSF for another year. This was before I had the NSF offer of a job. I was asked to stay another year in a governmental assignment position, which was not uncommon, and I went back and had lunch with Jim Hart and asked him if I could have another year's leave and Jim said no. So I was undecided whether I was going to go back to Berkeley or not. I could have gone back to the library. Salaries were still low then. It was only after I left that salaries began to climb and I think my salary there, if I was still there, would never be as good as it has been at Minnesota, but then of course I wouldn't have the same responsibility either. So I just bided my time. I already knew at that point that Ann was going to stay in Washington. And I just simply resigned from Berkeley so there was no going back. I could have gone back earlier, but not at that point. So when I went to Minnesota that was it, that was the bridge that was open because I rejected the NSF post.

Aspray: So tell us about Minnesota.

Norberg: Well, what is it you want to know about Minnesota? One of the things I've always believed and practiced is to not get in the way of either people who are working with me or for me, but also not getting in the way of other people who are doing the job that I used to do. For example, the treasurer's position at the History of Science Society is a two-year term and I was going out of office after two terms. My successor was Seymour Mauskopf. He was going on leave for a year and asked if they would accept me for an additional year. So I was a treasurer for five years. But when I left it was Sy's job. I didn't go on the finance committee. I didn't go on the council I refused to run for office

*Revised 2014-01-10 to correct spelling of Seymour Mauskopf, p.39.

for the council and I've never been asked again. In sum, I left because I felt Sy should do the job.

When I left the Babbage the first time in 1993, before Bob came aboard Judy O'Neill became the interim director and I left her to her own desires, activities, and decisions. When Bob came, same thing, I didn't hang around. I didn't offer advice. I gave advice if it was asked, but I just don't feel right budding in. I won't do it with Tom Misa when he takes over either. He can ask for help if he wants, but if he doesn't, I won't be offended by it. So when I took on these jobs — when I took on the job at Berkeley, when I took on the job in CBI — I didn't want the people who hired me around either. But how do you tell them? They're colleagues and so on. I was fortunate that the year I went to Berkeley, Roger Hahn went on sabbatical to Europe. By the time he came back a year later I had solidified my position so he was not going to get in between me and Jim Hart. And when I came here Roger went to Germany on sabbatical and so I didn't have to contend with Roger. By the time he came back, I had solidified this position too. So I was fortunate in that sense. Both of those men are very hands on and I believe that they think they're helping and maybe they are, but I just didn't want to have to answer to them. I had other superiors to answer to so I wasn't going to answer to them. So I just simply got out of the way. When I came there was only LaVonne Molde working in the Institute, period. So the first thing we had to do was start organizing and hiring a staff. And knowing the academic procedure, I knew it was going to take a while, that when you hired someone they can't automatically come for whatever commitments that they have to the institution that they are at. I was expecting a year or two. You remember, Bill, you showed up only eighteen months later I think in late 1983.

Aspray: Not your first choice.

Norberg: Well, that's one way to interpret the result, that's true. But I was being pushed by the History of Science people, they wanted somebody else. I wasn't so set on a candidate that I did not go along but it wasn't rejection at that point…yet, yet. The one person I did reject you know well. He started arguing with me during the interview and I thought well I'm not going to be able to work with this fellow. So he was off the list very quickly. Anyway we set out to do this hiring and in order to make it appear at least that the Institute was doing something I hired ten graduate students because I had the money. I had them working on bibliographies and on background to interview candidates, on assembling materials, published materials primarily and so on. And I put out a couple of documents in the history of computing area, and the community laughed. One was a bibliography in history of computing and you may remember that one of the graduate students helped me put it together. And I remember that Bell, what was her name?

Aspray: Gwen Bell?

Norberg: Gwen Bell just made a fool out of me in public about those CBI documents. To her the bibliography was naïve; it was not very helpful and so on. Maybe it wasn't, I don't know, I haven't looked at it in years. But I wasn't getting off on the right foot with that community. We knew later on, of course, that she saw us as a competitive threat for raising money and she was bad mouthing us out in the community to potential funders.

So I understood things a bit better later on. She was buddy-buddy with Jean Sammet, so both of them were giving me a lot of heat in public. Nevertheless, I went to my first board of directors meeting as the new director of the institute; I had no role in the foundation at that point. I put together a strategy document, which I think you saw later on when you came to the institute, Bill. And it had a program outline and I expected CBI was going to do everything from collecting oral histories, something that we have done over the last twenty-five years, to policy work, to historical studies and so on. You know I was really being inventive, I guess would be the word. Not thinking that all of them would get done, at least not in the beginning, but that we'd pick them seriatim and we would get most of these things done. I was going to try to build a historical database of economic data, until I found out what happens to annual reports when mergers take place and that was the end of that idea. Well the directors were in two camps in reaction to that strategy report. There was Erwin Tomash and the man who worked for the *New York Times*, he was their data man who set up all of their information data bases. I can see his face but I can't remember…he died at that meeting as a matter of fact, at the meeting. Anyway, he had some comments to make about that too and they were in opposition. So there were those people who criticized it. It was too much. You can't do this. We need to get archives. They wanted me to do archives and I reminded Erwin that I had told him and the Dean before I accepted the post, if all they wanted was for me to replicate the Berkeley job, I didn't want to do it that's why I left Berkeley. There had to be historical work. There had to be research and publication. And other things that would go along with what a professional historian does or else I didn't want to take the job and I was very clear about that. Since I had this other letter in my pocket I didn't have to worry about having the job. And they all said, "Oh no, that's not what we're going to do." Well in this first board meeting that's exactly what everyone was saying, "We want you to get the archives in." The AFIPS representatives, Jean Sammet…

Aspray: Aaron Finerman

Norberg: Aaron Finerman and I forgot the man from Pennsylvania…Garner?

Aspray: Saul Gorn?

Norberg: No, it was Garner. I've forgotten his first name. He was at the Moore School. They were the three representatives, and, of course, Sammet and Finerman were just up my back all the time. Indeed, it angered Erwin a great deal, because they were pushing the AFIPS agenda and saying that we set out to support this because it was going to build an archive and we want to see the archive built, blah, blah, blah,. Erwin was trying to say you're not here as a representative of AFIPS, you're here to help this organization. That's what a good board member does. So they were getting into all sorts of fights at the time. So I saw the handwriting on the wall, it was very clear to me that a number of the things that I suggested were either unworkable like the historical database, or unsupportable, for example, the economic database. There were things this group was not going to support such as the policy work. They didn't want me doing policy work. In fact Birkenstock from IBM was the person who was most adamant that we shouldn't do that. So there were these two camps: there was the AFIPS camp that wanted us to do what they wanted and wanted us to stick to the archival program only, don't do the rest, and there were the industrial people—I don't remember the historians being involved in that argument—who thought we should sort of scale this, should do the archives first, then do the history, and then do whatever else we can after we have a program established. And so I went away from the meeting feeling disheartened because I didn't get my way but also understood that these people had good ideas and they weren't bashful about offering them and so I should take them seriously.

Aspray: Just to be clear, this was the CBF [Charles Babbage Foundation] board?

Norberg: That's correct.

Aspray: Because later many of those AFIPS representatives were on the Program Committee rather than on the board.

Norberg: Well, that was when AFIPS stopped sending money. AFIPS agreed to give $50,000 a year for five years and that was a third of CBF's contribution. So they felt they had some say in what was going on. But once that money commitment ended, indeed we had…it was not clear we were going to get the last payment even, because that's when the national computer conferences were beginning to have trouble, and AFIPS didn't have as much loose cash, marginal cash, because the societies were demanding their share off the top and if there was any money left for AFIPS management, they could use it as they wished, but if there wasn't, tough luck. So it wasn't clear toward the end, toward the fourth and fifth year that we were going to get the money from them. Well Steven Yao came to believe they had a commitment and they were going to keep it even though he didn't believe they should give the money to CBI. When we got the fifth year, Erwin decided we don't need these people anymore. But by that time Galler was on the board and he could influence Finerman. Sammet was gone and Michael Harrison from Berkeley was nominated to the board as an AFIPS representative and we liked him so we kept him as a CBF member. And there was another person that came along…oh yes, the man from Norway, Per Holst came from AFIPS, too, and he was very helpful in Europe. So we kept representatives, some on the Program Committee, and some on the Trustees. There was no board of directors then, all trustees went to directors meeting if they so chose. The group was not large. There was only forty at the time, so twenty would show up and it would be a respectable meeting. Once they began enlarging that group, then it separated into a board of directors and a trustee group and by that time I was the executive director and running the operation for them out of CBI. Things were much more structured by that time. But that was three or four years into the task. When we lost their money in 1986, that was the year the industry had still not recovered from the 1982 recession. The companies had all exhausted their agreements—three years from CDC, three years from AT&T and so on. And they didn't re-up. Bill Norris asked me when we were going to become self-sufficient. What do you mean academics self-sufficient? What are you talking about? I didn't even understand the remark. So we had to do something else and that's when the discussions began about giving CBI to the university and three years elapsed by the time it was done.

Yost: Can you discuss the history of computing as a new area of inquiry and did you feel historical research was essential to effectively collecting in this new area?

Norberg: I will answer the second part of that question first because there's a very straight forward answer. The answer is yes. I felt that we had to conduct historical research for three reasons. One of them is we had to justify our existence, that we were thoughtful people in the institute…I was the only professional at first, but then when Bill and Bruce came it was clear that we had the professional authority to publish in the field. So we had to do it for justification for the institute. We had to have an endorsement from a larger community that had no interest in CBI. The second thing is I believe that it…my experience at Berkeley had shown me that if you know a lot about a field you know whose papers are worth going after. And indeed that's the way I operated at Berkeley. I had separate projects in my mind—they were not formally organized—separate projects in my mind. There was the physics area, nuclear physics, and as I said, I hired somebody to do that part of the task. And then I had another thing going on in the nuclear sciences, particularly in chemistry and I was interviewing chemists and physiologists. I didn't do the medical side, I hired somebody else subsequently, Sally Hughes, to do that. Then I had this electronics business going on down on the peninsula and so there were lots of things being juggled at the same time. I had good graduate students who were developing questions for these interviews and so on. So when I came to CBI I thought that doing historical research of the sort, of the investigatory kind, not the interpretive kind, would lead to good collections. The American Institute of Physics (AIP) had demonstrated that you had to do surveys of people and find out what they had and what their accomplishments were. They have a file full of these survey documents that are like CVs from physicists and related people like astrophysicists. I thought yes, we had to do that and when Bill came he was convinced of the same thing. And the third thing I thought about was professional development. I didn't do much publishing during the Berkeley project. There were three articles from that project. Nothing came out in physics. The scientists at the Berkeley Radiation Laboratory wanted me or somebody in the project to do a history of the Berkeley lab. Seidel and Heilbron did some of that after I left. But they expected me to do that and I didn't, I never got it done. I never even made any attempt to try. So when I left there I felt that I had learned a great deal, but I really didn't have much to present to the outside world yet. Professional development was going to require historical research and interpretation. Bill and I designed a program that we would do certain things separately and he would get more publishing done than I would and he did. Whenever anybody on the board of directors or any of the trustees said, "You know you're really going to have to get more archives. You shouldn't be doing so much historical research." I just simply never agreed with them and would never back off. Never backed off and felt it imperative for the staff at least—you know I was a tenured faculty member so if I never got promoted or I never published anything it really wasn't a catastrophe for CBI. It would have been for me. I would never have been able to hold my head up I think if I hadn't done the publishing. But as I said before about the writing, I 'm very slow. The DARPA book took seven years from start to finish. Whether it would have been done faster if Bill had not ascended to the directorship of the IEEE History Center, I don't know, it might have and it might not have. Who knows what we would have done. Judy O'Neill was helpful in the areas that she was knowledgeable about but she wasn't helpful in the project overall. Her training was still too narrow and her outlook on history was insufficient. She was about in the same state of knowledge I was at when I got my degree. And that may be the case with many people and so she needed time to mature in order to be able to participate fully in that book. She did a good job, but to participate fully she would have needed more maturity and we didn't have the time for that.

Aspray: I know this is Arthur's interview but I want to interject with a comment here.

Norberg: Go ahead.

Aspray: When I was there Arthur was not only very supportive of my doing historical research, he was very protective of my time to enable me to have a lot of time to do that. He took on essentially all of the administrative detail work that wasn't being done by LaVonne so that I didn't have to learn any of the systems. So that left a lot of time for network building, it left a lot of time for kicking Bruemmer's ass. [laughs] And it left a lot of time for historical research.

Norberg: I think Bruce Bruemmer is very grateful for our pushing him very hard—he was very shy when he came in. I guess, he thought he was running an archive, an archive with nothing in it to speak of at that point. Bill, particularly, was pushing him to get out there and find more collections. Pushing him to get involved with the computing community and the archival community. And he did over the course of five years and he became very good at everything and he's grateful for that now. He knows that we did him a good turn, we weren't just trying to get him to do work, we really were interested in him.

Aspray: I think…I also presented Arthur with a dilemma at one time because it wasn't very long after I'd come there that I was invited to do this big oral history project on the history of mathematics at Princeton. That really didn't have much to do with the mission of CBI. It was clear that he wasn't enthusiastic about it, but Arthur had this feeling that I should be able to pursue my historical research interest and this built on some things I had done before and he let me do this. I got past it and got more back on target with computing.

Norberg: Yes, but it was a good job and it's had a good deal of attention, so it was very worthwhile. Now that CBI gets recognized for things like that even though CBI had nothing to do with it, it helped our reputation as an organization so it was a good idea to let you do it. I'm not so sure that I let you do it, I just didn't object. I don't like confrontation on professional issues. I'll confront somebody on a performance issue and certainly on behavior in the institute, but I never felt that I was competent enough to be able to say we should do A and not B. However, I remember a conversation that we had walking to the office from Dinkytown, which you were not very happy with the results of, because I was saying the history of mathematics is not something we do in this institute at the moment. And I remember him being angry about that. So I quoted the mission to him. I would quote the mission to Bill and he did his own interpretation of that and indeed he went along with it. We only do work for CBI. It isn't until 2000 that I myself moved away from that. That was the year I published the astronomy article returning to an earlier love and pulled that stuff out of my dissertation and did some more research and made it better than it was in the dissertation. But that's the first time I'd ever done anything outside of CBI's interest since I came here.

Aspray: I'd like to go back a little bit and ask some questions. There had been some development activities that were…that went on prior to the actual movement of the organization to Minnesota and you at least had some involvement with those. Would you tell that story?

Norberg: Well, I had not had much involvement in that before 1981. I was involved in a couple of things. When Erwin decided to found an institute—and this in 1975, I think—he was stepping aside as Chairman of the Board of Dataproducts Corporation. He thought he was going to do a history of computing himself—and he had been taking some courses with John Burke at UCLA in the history department. (John was my predecessor as treasurer of the society, so I knew John well.) So John was helping Erwin to understand what the dimensions of such a history would be, how to define it and how to work at it and so on. Somewhere along the line Erwin decided, he said to me later, that this was not his interest. He wasn't going to do all this detailed historical research. He wasn't going to write such a history and then he started talking to the man who was head of the Clark Library at the time…I can't remember his name…and Erwin came up to Berkeley at his suggestion. He talked to me and he talked to Heilbron or Hahn, one of those two. He went over to the Historical Society in San Francisco and talked to people there. Erwin and I hit it off. We met the next time not long after that at the meeting in Los Alamos that produced the book, *A History of Computing in the Twentieth Century*. I asked if I could attend that meeting because I was going there to interview people for my own projects at Berkeley. That was the first time I'd ever encountered somebody like J. Presper Eckert. John Mauchly was there…I've forgotten some of the others…Julian Bigelow was there…

Aspray: Backus.

Norberg: John Backus was there and Herman Goldstine was there. These were new names to me and I learned something from what they had to say. So I attended that meeting. Erwin and Adelle (who were also there) and I, one day, went for lunch together rather than going with the group. They wanted to picnic outside; they weren't interested in eating inside again. So we went to a deli or a food market to get some food and took it out…there's a small lake, artificial lake in the middle of Los Alamos, the town, and we sat there on a picnic table and we talked about this. I tried to sell Erwin on the idea of a museum, because there was no museum in 1976. It was in the works, in the planning stages back at DEC, but I didn't know that and I'm not sure Erwin did either. He might have. But I tried to sell him on the idea of a museum. It would be a mini-Smithsonian. He said, "Well what do you think it would cost?" And I said, "Well about twenty million would be just a guess." "No I can't raise that kind of money and I don't have it myself." So in the next year or so that's when he began putting together the people that became the first trustees. He had talked to Bob Multhauf; he had talked to Walter Bauer; he had talked to Jim Birkenstock, and a number of his other friends like George Ryan, people who had made some money in the industry or were well connected in the museum and historical community like Multhauf and Joshua Lederberg.

TAPE 2 (Side A)

Norberg: At Lederberg's invitation, the trustees met for the first time in 1978 at Rockefeller University, and for the agenda, they had invited three people for show and tell. This was a common activity for children at the time, show and tell, and it became a phenomenon in education and elsewhere—so we were invited to show and tell. It was Spencer Weart, talking about the history of physics and the Center for the History of Physics. There was Morton Fine…was that his name? That doesn't sound right. The fellow who was the editor of the Bell Lab series. He was there to talk about the Bell Labs project and that was an interesting talk, I thought. And then I was invited to talk about the Berkeley project because of the electronic component. The other people in the room were Tomash and Paul Armer. Al Chandler was there. Joshua Lederberg was there. Lederberg was in and out all day long because he had other business there, as incoming president of the university. Jean Sammet was there. James Birkenstock was there. There were a couple of others whose names don't occur to me now. We spent the whole day talking about our projects, the three of us, and then listened to them talk about what they thought they ought to do. Lederberg gave some good advice that day that was very valuable. The people in the room decided it would be a good starting point and they put it into practice very quickly. One piece of advice was not to set up an independent operation, because you have all the costs involved in the independent operation. In terms of facilities, in terms health care plans and benefit plans and so on. You have to do all of that. There's no clear case of continuity that you'll be around ten years from now. It might disappear easily like a small business disappears. The second piece of advice was to organize some sort of arrangement with a university because a university offered three things; first of all it offered this continuity that you could appreciate…that a person that you approached could appreciate that you were going to be around awhile, because the university was not going away, and so if you were successful you'd be around. Secondly, you would have all the intellectual resources in the university: the library, the faculty members, other types of operations, the university campus. And thirdly, you would have help in fund raising because the universities are all set up to do this. And then the third piece of advice he gave was there aren't very many people in the field. And this gets back to your question, Jeff, about what was the history of computing like. It was clear in 1978 that there wasn't a lot going on. *Annals of the History of Computing* had just begun and it was largely the captive of the professional computer science community. The first editor, Bernie Galler, was a very good choice. He was good at promoting, persuading, and proposing. He was able to get the money to run a journal and it ran the first few years in a terrible deficit. And he was good at bringing people together to give and get advice. He is very easy at taking advice, very easy at giving advice and very polite about it. But that was just beginning. The second issue of the first volume was just coming out in 1978. By that time the DEC museum, as it was called then, the Computer Museum of Boston as it became, moved into the wharf area downtown, that was just beginning. They had put together a considerable amount of equipment for display. They had a display area at the DEC headquarters. It was totally funded by DEC at the time and until they moved downtown they were dependent upon DEC for their funding. But that was new. They had no pretension about doing historical research. They never said they were going to do that, unlike the Computer History Museum in California, which has said it over and over again. The collection was relatively small, it was hardware oriented and when they started running into trouble raising money, just as we did in the middle 1980s then they began cooperating or partnering as it's now called in the industry with the local schools to get school groups in. Well that's a whole different kind of operation. So even some of the things they wanted to do in the exhibit development, they weren't able to do. Ultimately they closed and they moved to California in the early 1980s. The Charles Babbage Institute was just beginning at the time. Start-up began in 1978 and established at the University of Minnesota in 1980. There were a few people getting degrees. There was Bill and there was Paul Ceruzzi, and Martin Campbell-Kelly, was beginning to work on historical topics although he was in a computer science department, and Jim Tomako was interested at the time, a software engineer. That's about all I can think of. Mike Mahoney was doing history of mathematics, which would lean toward history of computing. Nancy Stern was doing her work on Eckert and Mauchly and working with Ruth Cowan at Stony Brook. Tom Smith and Kent Redmond were doing their project study of the Radiation Laboratory at MIT and then on to the work on MITRE. There was a young man out at Santa Barbara who was doing a master's degree in the public history program. He did it on the chronology of growth of the computer industry in the world, did a very large survey project. That was funded by Tomash personally. I can't think of anybody else. The third piece of Lederberg's advice had to do with enlarging the number of scholars working in the history of computing.

Aspray: That was pretty much it.

Norberg: Yes, that was it. So what Lederberg was saying was we need more people to do this work and so you have to give out fellowships.

Aspray: Andrew Hodges.

Norberg: Yes, Andrew Hodges had a particular interest in Turing and did that excellent biography of Turing. There was Hyman who was doing the work on Babbage. It was a relatively small community, some of them older people. Hyman was older. Dorothy Stein, who did the work on Ada Lovelace, was older. Neither one of them were historians. Gratton-Guiness was doing some work, but it wasn't directly related to computing; it was more the culture of applied mathematics and applied science in the French revolution period. Bernard Cohen was interested in the Harvard situation. Later, he did two excellent books on events at Harvard.

Aspray: Nathan Reingold in applied mathematics.

Norberg: Reingold in applied mathematics and then the people at IBM, Emerson Pugh and Charles Bashe. But again they were not historians. Bashe and Pugh came out of the development area. Bashe had risen to vice president for development and systems products, I think, by the time he was getting ready to phase into retirement. He took on the technical history project and brought in Jack Palmer and Emerson Pugh and Lyle Johnson into the project. They published two volumes that are really very good.

There you have the field in the early 1980s. I think at CBI we understood at the time, that there were two things missing. First of all, these people, when they were through with those tasks, like Bashe and Pugh and so on, were going to drift off—Pugh didn't but we thought he was going to drift off after that—and not do anymore in the field. So we expected to lose them. The *Annals* clearly in 1981 was doing first person participant accounts, and while they are good primary source material, it's not necessarily history. Paul Ceruzzi took on the job at the Smithsonian at that point, left Clemson University, and got very involved in exhibit development and published the computers and flight book, and then went on to do other things in history of computing. Bill was doing some work with Donald Beaver on advertising and was getting ready to do his…the information concept piece and then the transition of computing to Europe after World War II. We thought Bill would stay in the field; we thought Ceruzzi would stay in the field. I had not done anything in computing at that point in 1980 and 1981. So who knew what I was going to do. I might not do it at all. There was nothing in my contract that said I had to. For all I knew at the time, maybe I wasn't capable of doing it. All those possibilities. I was not part of the community when I took the job at the Babbage and there was some question about my talents in computing, especially among Galler and Isaac Auerbach and some of the other people in AFIPS. Indeed, they asked me a direct question one night in Isaac's car driving to dinner—we were at the DEC meeting in 1981—about what my background was in computing, did I know anything about computing. I think it was very blunt. I related some of the things I'd done: about the nuclear diffusion work and the dissertation work and which machines I'd worked on and so on. And that sort of…At least they felt I was capable of understanding when somebody was talking about computing.

Aspray: I'd like to ask about that DEC meeting because I remember that you had quite a public disagreement with Gordon Bell at that meeting.

Norberg: No, that…

Aspray: Was that later?

Norberg: That was at a computer museum meeting afterwards. That's the time when Sammet and Gwen Bell got after me about the bibliography and Gordon got after me because he thought I didn't know the development of PDPs and the VAX. Since he was one of the principal agents in VAX development, he thought he knew all about it and probably did. And so he went after me publicly, but that was not at the DEC meeting. He was never part of CBF. When he was invited, he declined and it was a good reason for declining, but I don't remember what it was. I think Gwen was sick at that time and he was cutting back and then when she got better again he increased his efforts in computing. They asked him a second time, but by that time he was with the Computer History Museum so he said no again. The difficulty came in at the AFIPS computer history committee meetings about collections of papers. I noticed in the *New York Times* or in some magazine I was reading recently about the twentieth anniversary of the NCC meeting at Disneyland…no, there was a commercial about the twentieth anniversary of Disneyland when they had that celebration, now its fifty years. That was the year NCC was there and that was the year I was on a hot seat for five hours, where Sammet and …the older guy what was his name, Carl Hammer and Finerman and maybe someone else, were really pounding me. Why aren't you doing this? Why aren't you doing that? Do you believe this? Do you believe that? When will you get this done? When will you get that done? I was being very polite, I think, with my response.

Aspray: I was in the room.

Norberg: And Bill and Martin came out afterwards along with Mike Williams and they were appalled at how bad it had been. But I never lost my cool. That's because I had my eye on the prize and the prize wasn't them or their money. I knew that they were going to go away before long and we were going to still be here. Those were difficult times and you remember it was at that meeting that I said to you that we needed a book on computing before computers. I didn't use that title, you dreamed up that title later for the volume. I commented to you that since you were coming to CBI you should do it on CBI time. You put together the group to write the essays and you got the book published through Iowa State University Press. You remember that?

Aspray: I know the book started while I was still at Harvard but I don't remember…but it didn't get finished until I was at CBI. And I don't remember all the incidents of how it got going.

Norberg: Well, that's my recollection of what happened. I was interested in getting you people to do it. I was going to contribute, in fact I was going to edit it at one point. When you came to the Babbage, I just turned it over to you and then I never participated after that. But it was trying to get these things done so that meetings like the one at NCC would never occur again. That we could throw things on the table and say, "Here's what we do, damn it. You're stuck with that; you accept that or not as you choose." So those were very contentious meetings. It was always the outside community, not the directors of CBF. Erwin Tomash was very, very quick at these meetings to correct Finerman when he was overly critical of CBF/CBI activities and attitudes, at least got out of line as far as Erwin was concerned. Erwin wouldn't let those guys take out after me and try to make a fool of me. Erwin just wouldn't allow it to happen and he kept reminding them what they were there for. And Finerman never believed it. So when Finerman was off the board of CBF then things began to change. Sammet was the same way. When she, too, got off things were far better. Then when we managed to get everything working well, when we had a staff, when we ran the meetings for them, when we helped with the fundraising—I guess I really didn't get heavily involved with fundraising, other than to go with Erwin when he visited somebody—until 1986. Before that they were coming up with the cash and they were keeping their commitment and so we didn't raise a lot. But I went to see these people. I remember going with Erwin to see Dick Daley down at Com…it wasn't Comserve but one of those…Comsoft or something…and learning from Erwin his view of the company's building. They'd built a palace for themselves and now had to sell it and lease the space back. I watched Erwin's technique at fundraising. These were people he knew and so he thought he could just approach them and ask them for the money and sometimes he got it and sometimes he didn't. His approach was good but he wasn't a fundraiser either. But in the course of those three years 1982 to 1985 when we got Tom Lindquist aboard—in those three years I set my own strategic agenda for fundraising knowing what I thought I could do and what I couldn't do and when I needed help and when I didn't. And then we proceeded on after that. But it was never a greatly successful fundraising scheme. Bill you know the reason well. It was because I had eighty things to do in a week: teaching, committee work and various other things for the university, plus running the institute, doing…attempting, at least, historical research. We made a number of sponsored project proposals, which were successful in getting money, and then attending meetings of other kinds. Somewhere along in there I became a member of the history committee of NASA and then I became the chairman of that committee. Later, I was asked to join the NASA Advisory Committee. That was four meetings a year of just the main body, let alone other meetings of the History Committee, which were usually two a year, and then any subcommittees I was on which might meet once or twice in the course of a year. So there was a lot of time going into those things. The fundraising wasn't always as effective as I would have liked. Maybe I was relieved by that, I didn't have to do quite so much. Maybe I wasn't, maybe I was distracted, I don't know, but it was just enough so I didn't want to do anymore. Then when we hired John Whitmore, I had less to do with fundraising. John set meetings up and I went along to make the case for funding. But unfortunately Whitmore died suddenly at age forty-one. So we were out of luck with that. That was the breakpoint for me. I knew then that it was going to be a struggle because I was getting no help from the university yet, the Babbage Trustees were gradually drifting away from fundraising. They just really were never good at it. They still aren't as far as I'm concerned. I was glad I didn't have to do any more than I was doing. We managed to keep the place alive with either grants like the DARPA grant that you and I got, Bill, or with private funding that we were able to get from one or more of the foundations like AT&T. Unisys was always generous—Sperry-Rand and then Unisys—they were always generous. Burroughs was always generous. So we managed to keep the budget in the black for the most part. When I left the first time in 1993, it was fifty thousand dollars in the red after twelve years of operation, running a budget around three hundred thousand on average every year. So it wasn't a difficult situation; we kept it running, but it never grew. And I think that was my biggest disappointment; that it never grew, that I was never able to either raise the money or find ways to make it grow inside the university. That I'm disappointed about. I'm certainly very happy I took the Babbage job. I think it's been the best career move for me. A bonus was spending the last twenty-five years at the University of Minnesota, which is why I never left. I've enjoyed working with some of the people at the university. I've certainly enjoyed working with almost all the people at the Babbage Institute.

Yost: You became interested and began a major project, a major historical research project, on Engineering Research Associates. Can you talk about the origin of that?

Norberg: Yes, thank you. When I came to Minnesota and was trying to understand what sort of historical research we should do, people like Erwin and Arnold Cohen and Dick Daley and one or two others kept telling me about the importance of the computer industry in Minnesota. In 1981 and 1982 the dying end of it was still around. CDC was still active, although they began losing money in 1982. Some of the other firms like Analysts International and Lawson Software were just beginning to grow. Analysts had been around since the early 1950s as a public company. At the time, Lawson was still prior to their IPO. So I began reading about the other centers. I knew a little bit about Silicon Valley, but not a lot. I knew about its predecessor activities before it became a computer center. I knew a little bit about Route 128, since I'd grown up near there. I began looking at the literature about those two areas, to see what the comparisons might be with Minnesota. Articles began to appear—*Atlantic Monthly*, *New York Times*, *Wall Street Journal*—about the centers of activity in the computing industry. The centers of activity were listed as being Silicon Valley, Palo Alto, Route 128, i.e. Boston, Dallas, and somewhat, North Carolina. No Minnesota! I thought now wait a minute. The men in Minnesota had a view that there's some computing here and it had a rich history, but nobody elsewhere seemed to think so. And where did North Carolina come from? Research Triangle Park was getting IBM and various other operations down there so they were promoting it, but they didn't have anything to speak of. So when I began looking into Minnesota I crafted the ERA project. By 1983, two years after I'd come to Minnesota, I prepared a proposal to send to NEH and NSF. It was a proposal to study ERA, to study its development, its interaction with the government, its interaction with the military services or at least with the Defense Department. There were half a dozen questions posed that were to be answered through research and oral history. We got the award, roughly $100,000 between the two agencies. I started working on that. Bill, by that time, received an NSF grant for his Von Neumann study, the first award and then he got a second award for the work. So we had things that we thought were going to make an impact on the field. Besides, Bill was continuously publishing in *Annals of the History of Computing*. I was not. So I started the ERA project. Effectively, I started it in 1984 when we got the money. By 1986 Whitmore was dead. The endowment fund drive was supposedly about to be in full swing. A whole series of meetings had been arranged and we certainly wanted to keep them. So I went to the Dean and I said, "I can't do everything. I've got to drop something. Here are my options." And I laid out the options for Jim Infante, our Dean, and the first thing he told me was drop the teaching, which surprised me a little actually. I was only teaching half time then, so I thought that was a little odd but his thinking was to evaluate options that would keep me on the campus. Nothing else will hold you on the campus. Try to continue the research, run the Babbage, and do the fundraising and it'll keep you off the campus when you need to be. OK so I did that. Bill handled things at home and I went out to do that and we raised…I don't know from $270,000…$300,000 in promises. And then the capital campaign came along for the university and that's when Erwin and Bill Drake put together the chair fund proposal and essentially funded the private half of it. Bill Drake was a Regent at the time and there were funds to be matched. Mostly the money came from Bill and Erwin. They put in almost a half a million between them and John Parker put in $50,000 and then there were a few other…quite a few smaller gifts. So about a half a million in all and the university matched that and it went into the ERA-Chair. So the book looked like it was going to be, again, a good capstone to that. Well, I didn't get much research done. Then I asked for administrative leave, let Bill take over as the Director, and I would work on the book. And I was two months off on leave—ten percent. I was back to teaching again and doing all the other things too. Ten percent off, so around March 1st, I was getting back into the research work on ERA and Bill said, "I've got another offer." And Bill left to go to IEEE. But by that time we also had the DARPA grant to think about, the DARPA contract in 1988. We had all these things going on and I wasn't really making any headway, so I just dropped the ERA book project.

Aspray: And we had the NSF grant, too.

Norberg: Which one?

Aspray: The internal, DARPA and NSF, what each of the organizations had done for computing.

Norberg: Oh, you had that.

Aspray: We were both on there.

Norberg: Were we? Oh, I remember I was but I do remember you were the proposer and the principal investigator. You took that one with you to finish it at IEEE. So the book essentially had to go into limbo while we did the DARPA book, because that was a contract not a grant. There were time limits, and we had to produce something in a reasonable amount of time. Within four years we had a report that we submitted to the Defense Department, which they didn't really like. They accepted it, but that was the end of the interaction with them. We wanted to turn it into a volume. That part was essentially paid for through CBI funds. When I went back into the faculty and left the directorship in 1993. Then it was my research time that I was devoting to the DARPA study, and Judy O'Neil and I completed a book manuscript that the Johns Hopkins Press accepted in mid-1994. The manuscript was edited by a person at the Press who did a marvelous job. Then I came down with cancer and so I had to deal with that and was teaching still. I taught all during the treatment and managed to index the book manuscript and managed to go over the editors comments and it was out in 1996. And now I could take up the ERA book again. Well, by this time I knew a lot more. There were a lot more papers available. Many papers had been arranged and were available for research at The Hagley Museum and Library. CBI had several collections that we had put together, small though they were/are. So I was just getting back into that again, I was director of graduate studies at that point and Alan Shapiro wanted to leave on sabbatical, so I was head of the program for a year. I wasn't getting a hell of a lot done. But, I came to a very important realization; that the story of ERA was insufficient as a published volume. It might make a nice memorial to the men and women who were there at the time (1940s and 1950s) but it wasn't going to appeal to our historian colleagues. And that's when I broadened it to study Eckert and Mauchly and make a comparison between the two. Well, then I realized that I couldn't really make the conclusions that I thought were appropriate unless I looked at Remington Rand in the period when the two were independent subsidiaries of Remington Rand. And so I took that on as well and that meant that I had to learn all about the Univac I and that's when I did the software study that you, Jeff, published in *Iterations*. It was all this added work that I had put into it to complete the manuscript of the extended study—it was seventeen years from the beginning of the project until that point. But now it was better, now it was better. And then I did the revisions—some of the text had gone back to 1988—I did revisions and put it together. I sent it to Bill. I forgot who else I sent it to, was it Mike Mahoney?

Aspray: Might have been.

Norberg: I asked Jeff to read. Anyway I sent to three or four people…Oh Cortada. I sent it to Jim Cortada. I got their comments back and I incorporated them. They were very good comments in picking up the places where it really didn't hang together because of the wide span of time in which the research and writing were done. Jeff made a great suggestion about the title to incorporate all the ideas in the book. Then, I sent it to the MIT Press, and in time another review came back. Turned out it was Steve Usselman who had done the review; he told me later. And his comments were all about the introduction, that the introduction didn't fit together, it wasn't coherent. It seemed like a series of false starts. And he was right. So I redid the introduction and did whatever else needed to be done, it wasn't much, and sent that back to MIT Press. The book appeared in 2005, twenty years after the start.

Aspray: And what was the reception of the book?

Norberg: I don't know. There have been no reviews yet. Martin wrote a brief one for *Computing Reviews*, which was very positive. Mike's reaction to the book was good, yours was good, Cortada's reaction was good, Usselman was pleased and so at least my friends, eminent in the field, thought it was at least credible to put out there before the public. Well now we have to wait and see what the reaction is. People on the technical side who have read it—Erwin Tomash, Paul Baran—thought it was good. After the book, Paul Baran wrote me a lovely letter, a four page letter after reading it. He worked for Eckert and Mauchly and I didn't know that! He worked for them for a year as a young engineer. He was pleased with the book. He was also pleased with the DARPA book, I have to say. Lee Keet liked it, too. Nobody in my computer science department read it, I don't think. So it is still too early to tell. It's only been out six months or so. … Oh yes, there was one review from *Choice*, didn't I tell you [Yost] about that? You got one at the same time. It was either *Choice* or *Library Journal*, both of which are library journals and both were treated, your book on the computer industry and the ERA book, favorably. The journals recommended both for library collections. I took a look in WorldCat one day when Jeff and I were talking about the reception of the books and it turned out that the number of entries in WorldCat, the world catalog of books, was exactly the same for *The Computer Industry* and for mine. It was 121 libraries. So it's getting attention, but we'll see. My own belief is that it came out too late; that period is not of as much interest now to either historians or to the technical community. It's too far in the past now.

Aspray: As it was say ten years earlier.

Norberg: That's right, yes. And I think if the book had come out say in 1990 it would have received a more significant reception.

Aspray: Well, but we know for example that there is going to have to be people going back and looking at the hardware industry again and that's when the book I think is going to have some…

Norberg: Well, I certainly hope that's right. It was a pleasure for me to do the book. I really…you know when you're a graduate student, going back to those days again in the interview here, when you're a graduate student and you're expecting to…at least hoping to get a job in the academic world you know you must publish. I was determined that I would produce many publications. Then when I got to graduate school, David Lindberg and I were talking about this one day in his office I was asking him about reviewing and how does one go about it because I was complaining that I was turning in these papers and getting grades back but there weren't any comments on the papers. How did I know whether the paper was any good? He was saying did you get an A? "Yes." "Don't worry about it," that was his remark. I thought that was awful. So we began talking about reviewing and how many people did reviewing. He said to me, and I don't remember why this subject came up, but he said to me, looking at reviews in *Isis* and places like that, he said to me that he didn't think people could do a good book review until they published a book of their own. Because then a reviewer knew what the dynamics were and they knew what the problems were and they knew what you should say about an authors' intention and so on. Whereas if you've never published a book, never written one you couldn't. So I always had this goal, some day I was going to do a book. First I thought it was going to be my dissertation, but that didn't happen, I crafted four articles out of it and that was it. So when the DARPA book came out that was a really great day for me. I really thought I had achieved something. But it was joint authorship; that was not enough for me. And that's why the ERA book was finished. Not just to please the NSF, not just to walk away from CBI to say I took this money and I delivered on the money... If I never do another book it doesn't matter. I did one alone. And I have an average of maybe one publication a year. Those things stuck in my mind. It's remarkable what sticks in your mind, what you think about with respect to your own goals.

Aspray: Can you tell us about the context of the DARPA history project and the book itself? You have talked about your other book, let's talk about this one.

Norberg: Yes, if you remember Bill they came to us, that was not a proposal that we made. We were invited to go to their committee meeting. I don't remember which meeting it was whether it was the first one, the second, or third. They had been talking about this for some time and I...we were invited at some point to come, you and me. We went to the meeting and that's when we learned that Merritt Roe Smith had been offered the contract, but he said he didn't want to do it. For him, it had to do with his time and other commitments; he didn't really want to take it on. Because he kept telling them that to get a good book on DARPA...they wanted a book in two years maximum, as if you could just sit down and write it. "Just listen to me I'll tell you what you should write" was the attitude of the professional people on the committee. But Roe said to get a good book it was going to take five to ten years and he said that over and over again at the meetings that we went to. So we went to this meeting and it was clear they had nobody to do the job. Roe had decided to turn the job down. For whatever reason we were invited, I don't remember, I don't remember whether they wanted to know about CBI or they wanted to know whether we'd serve on a committee. But anyway Bill and I sat there and we contributed, I guess, to the meeting. We listened certainly, and we were glad to meet members of the committee we didn't know. Saul Amarel, the incoming director of the DARPA information program, was there that day, and Allen Newell was there, I'd never met Allen before. It was a good experience to meet these people. Then we came back to the institute, we came back on a Friday night, and Bill said he'd like to do the project and I said no I don't think I want to do the project. I don't think CBI should do the project. We parted company for the weekend. On Monday, I said I thought about it some more and I think I want to do the project with you. What was I going to do? And so we talked all that week on and off about this at lunch and in the office and so on. Finally, I convinced myself I should participate. Bill did not convince me to participate. I don't know whether he wanted me to or not—and you don't have to say anything Bill—I don't know whether he wanted me to or not. I decided I was going to participate in the project, as I remember it. That is when we put the proposal for a million dollars together and sent it off to them. We asked for a million bucks. We wrote a schema for the book, which was essentially what came out in the end. The initial item was going to be the report, and this is where the report went in to the finished manuscript. It was going to be roughly half the volume on the DARPA office, the Information Processing Techniques Office, and that would serve as the basis for studying the Defense Department's interest in this. But in order to justify their influence, we believed we had to do case studies and we set out to do six, we outlined six for them. We ended up doing four of the six with some comments about the others but only in passing. And we gave it to them. Well they were appalled at a million dollars for a history book. [laughs] A third of that would have gone to the University, so we're talking about 660,000 for direct expenses. And they said, 'No, no way. No million dollars.' Saul had just come in as the program officer in that office, under its new name, and he said there's just no way he could get a million dollars out of them. So we went to the next committee meeting and we talked and talked and talked about what we thought we could do and why we thought it was important to do it this way. Saul said the best I'm going to be able to do is half that, half a million. So Bill and I talked about it during a break in the meeting and we shaved the proposal. We cut down on the amount of front matter, and if you remember the book it only has one chapter on the office itself and the people involved, and we dropped two of the case studies. We took the four case studies in which we thought there was a lot of material and that we could do. And then they accepted it. They gave us the grant for a half a million. We then kept going to meetings and I remember one of them very clearly because I wasn't clear on what was being said so I had to think about it for a long time. When we presented our progress, or what we thought was progress anyway, I remember Licklider standing up and saying this is all very well but it's a concentration on...what's the complement of semantics?

Aspray: Syntactic.

Norberg: Syntactic. It's syntactic, it's not semantic. I thought what the hell does that mean? He was a psychologist of course and somebody who had worked on AI problems and problem solving and knew exactly what he meant but it took me a couple of months to realize what was going on. We were doing syntax: construction of the office and programs. We were getting our organizational information ready, we were trying to understand timelines in the office. We were trying to understand where the people came from and so on. We weren't dealing with meaning. We weren't dealing with programs. And indeed we had no records to deal with meaning at that time—it wasn't until later that we got them. So Bill and I went out and did some interviews. He concentrated on people like...I almost said Finerman...the man that you…

Yost: Ed Feigenbaum.

Norberg: Feigenbaum, yes, Ed Feignebaum. Concentrated on Ed Feigenbaum and I went off to do some of the DARPA...the IPTO directors and did some of the people in networking. Judy did Paul Baran and I did Larry Roberts. We sort of divided this up a little bit and it wasn't until at least a year that we had any semantic contribution to make and they were getting discouraged at that point about why we were so slow. Bill in the meantime got the offer to go to IEEE as director of their history center, and it was a good opportunity. We talked about whether the project should be shifted to IEEE and I didn't want to let it go, to be quite honest. I didn't want to let it go, so I said no. I don't think it was very acrimonious because he had the NSF grant and he took that one with him. So there didn't seem to be a lot of conflict about it. But nevertheless I decided I wanted to keep it at CBI.

Aspray: One of the things that was in our minds at the time was that we could learn something from doing the NSF and the DARPA studies at the same time, comparative analysis.

Norberg: Yes, and we had some trouble with that. England at NSF was really a poor manager of that project I thought. When I went to see him privately to try to get him to change his mind about his attitudes toward the products, which I had had no part in... In a sense the project was still being managed by CBI and therefore the University of Minnesota was the sponsoring agency so I felt I could go and talk to England. But he was just standoffish. He wouldn't agree. Then, Bill, you had the trouble with the NSF people in education.

Aspray: Yes.

Norberg: So it really wasn't a good result. Left both of us with a bitter taste in our mouth.

Aspray: And we really had problems because Bernie Williams was working on a project; he was right in the middle of a divorce, and we just couldn't get his attention.

Norberg: Anyway, once Judy O'Neill and I had finished the report, which was a typical government style report from consultants where roughly half of it was a description of the programs and the people and that sort of thing, and the second half was the case studies, the four of them. We submitted that and then my next thought was publication. I approached Johns Hopkins. I think it was Roe Smith who at a committee meeting of the DARPA project after reading the report—and he had been talking about Johns Hopkins as a press because he was the outside editor for the series in the history of technology—and so he said, “There’s a problem here, we can’t publish this. This is not going to be acceptable to the press.” He didn’t say it wouldn’t be acceptable to him, but he had already told me that more privately. He said, “It has to be an integrated study and not just integrate the case studies into the book but get some sort of a driving strategy from the management section of the book.” And so that’s how I structured the book. That’s why chapter one is the management chapter and gives some criteria for judging the rest of the activity: the case studies, the various conferences that they ran and so on, are in the second half. Judy wrote the drafts of the networking and the timesharing chapters. I wrote the AI chapter, which took me a year.

TAPE 2 (Side B)

Norberg: So I wrote the AI chapter and the chapter on Mosis.

Aspray: Integrated circuits.

Norberg: Integrated circuits, I wrote that chapter and supercomputing, parallel computing, and then the conclusion. And I wrote the introduction. Well, O’Neill and I discussed…I won’t say argued, but we discussed a lot about just how far we could go in drawing conclusions from the data. I wanted to push things as far as I could in drawing conclusions. Every time I would present her with conclusions she would want to draw back to the data, the data doesn't say that and so on. It was a sort of a typical nuts and bolts historian of technology approaching a field where you don't go beyond your artifact. So finally I just decided I'm going to do this, I'm not going to pay attention to her and I finished the manuscript and sent it. She never said anything about it, but I'm sure she didn't like it. And then I told you the rest of the story about the editing which we did, which I did in 1996. Kerry Freedman wrote a draft of the chapter on graphics. It was good for say [ACM] SIGGRAPH, the people there would have appreciated it, but it wasn't like the other four chapters in the book. So I had to rewrite it. And she didn't mind, at least not then. I don't know what she feels about it now. But I rewrote it in such a way that it fit into the rest of the book. So as far as the production is concerned, I would say I did seventy-five percent of the work and they did twenty-five percent. But without their help I could never have gotten it done even in that time. And it took seven years from the beginning of the project until the publication and by that time everybody was gone or going, Licklider was dead. Newell was facing death from cancer.

Aspray: Fano was retired.

Norberg: Fano was retired. Saul Amarel was gone from IPTO or whatever it was called. Steve Squires was still at DARPA, but he never thought we did a good job, which I believe, is why the second contract went to Alex Roland rather than to us.

Aspray: Squires didn't like the Roland volume either.

Norberg: At least that's my conclusion, too. Squires never told me that. John Toole had served as the program officer, not the program director, but the program officer. So I was basically dealing with John Toole at the end. And the press had been under the impression, and so had I, that DARPA was going to buy five hundred copies from the press. So they wrote the contract in such a way that the first five hundred copies were royalty free because that would then satisfy the government's right of…

Aspray: [laughs] Geez.

Norberg: Whatever. And I certainly agreed with that because that's five hundred that were going to be distributed free so why complain about that. So they wanted to do five hundred. Well Squires and Barry Boehm and Toole didn't think it was worth doing that, so they didn't buy any. I think the press was very disappointed. So the eight hundred copies roughly that have been sold, none of them went to DARPA as far as I know. So the book came out in July of 1996. The Katie Hafner and Matthew Lyon book [*When Wizards Stay Up Late*] on IPTO came out at the same time on networking and the internet. That book got all the attention. Johns Hopkins Press did nothing, nothing to advertise the book, did nothing to say this is the authoritative study on the Arpanet/Internet. They made no attempt to get it distributed more broadly. So it fell flat on its face as far as I can tell.

Aspray: What were the reviews like?

Norberg: The reviews were by and large good. Campbell-Kelly wrote a very nice review. Paul Edwards, he was the one who did the review in *Science*. It was a very long review. Paul's review was very favorable. He made one set of criticisms, which were on target, he felt that we should have done more to emphasize the secondary impacts of DARPA's influence, not just what happened at Xerox Park, but what happened at Apple, too. We didn't really talk about that. What happened at Intel and so on. That was a good criticism, had we thought of it we might have done it. If we had more time, we might have thought of it and done it. So I thought his review was very good. There was one other review, which I was not pleased with because the reviewer essentially talked about a different book that should have been written, and I don't think that is really the purview of a reviewer. So I just discounted that review. There were various others in other technical journals. *IEEE Spectrum* did one that was nice, it wasn't particularly insightful, but it was nice. So the book was favorably received I would say. A testament to it, even though it's an indirect testament, is the fact that the most popular set of interviews in the Babbage Institute collection downloaded, and in terms of research citations, have been the DARPA interviews. And they are good. They were done by three different people so they have a caché, I think, which is very complementary in terms of the styles of the interviews. And the people that were interviewed are the people that should have been interviewed. We might have interviewed more of course. The Blue interview is priceless, I think. The one with Lukasik is less so, but the one with Herzfeld, the DARPA director after Lukasik, is really very informative about the interaction between IPTO and the Defense Department and the program directors within IPTO. The one with Feigenbaum is good on DARPA, but nothing else is covered there, and that's why Jeff did another interview with him. This use is an indicator about the attitude of the community outside and it dovetails nicely with Newell's attitude as expressed in his interview. So there is significant material in the interviews and they have been used over and over again. They were used by Hafner and Lyon, and they were used by Janet Abbate. Some were used by David Dickson for *PC News* and *Nature* and a couple of others that don't come to mind at the moment.

Yost: You two both interviewed J.C.R Licklider, which was the only oral history with him, and was used extensively by M. Mitchell Waldrop in his Licklider biography.

Norberg: Right, you're right. I'd forgotten that one.

Aspray: And what was the reaction from the DARPA insiders?

Norberg: Well, I think I gave an indication of that in the Barry Boehm, Steve Squires, and John Toole…attitude…

Aspray: You told us that they weren't happy, but you didn't tell us why.

Norberg: Well, yes, I have to conclude now, because they never really gave a firm statement of this, but for example Bob Kahn who was very good to us, I don't mean this to be critical, Bob Kahn was one of the people we sent the manuscript to for review, the manuscript of the book. And he called me, he set an arrangement and he was going to call me and Judy at the Babbage on a Saturday, and for five hours on the telephone he went through page by page by page. He had comments on every page. We took most of them, we didn't take all of them and the reason we didn't take all of them, and I think this gets to the heart of your question, is that he wanted to insert adjectives or adverbs that changed the nature of our statement, that made it more positive, more glorious and I was unwilling to do that because that's as far as I was going to go with the statement. It was what I thought was a well thought out statement and that's all I was going to say. Bob accepted that and as I said he was very helpful. His was the longest criticism in terms of attention to the manuscript. Some of the others in looking at the manuscript, people like Uncapher…what was Uncapher's first name?

Aspray: Keith.

Norberg: Keith Uncapher, thank you. Keith Uncapher was disappointed that we didn't ever use the word unique. He thought that the book should start out, "IPTO was a unique office in the Defense Department and extremely valuable to the computer science community." The only part of that I remember is the unique, he used the word unique. And he wanted to use unique here and there and everywhere else in the text. And I said, "Keith that's a conclusion, that's not a premise. And I don't think we can conclude that because we put stuff in there that shows that some of the practices were already being followed by the Air Force, so DARPA just borrowed them." Some of the other practices were time honored government contracting, which didn't change until 1980. But he wouldn't believe that and I don't think 'til the day he died he believed it. So that was the kind of reaction that we got. But none of those people reviewed the book in print so they never said anything publicly.

Aspray: What do you think was the value of the book in terms of understanding the history of computing, the DARPA contributions, the relationship between the military and technology?

Norberg: Ok. We have to drop the word military, because we never used the word in the book. We talked about the defense community. DARPA as I see it, at least up to 1986, was a defense community organization. The contracts and the grants that were given by them were as part of the defense background, because the computer science area was still very primitive. It certainly was nowhere as mature as it is now. So they had to make a lot of stabs in the dark, in some instances, in supporting programs to try and get a basis built for doing some of the things that they really wanted to do, that they didn't really get to until the 1980s. Things which showed applied applications to some of these principles in graphics and AI and so on that were going to be useful in computer science. So the military doesn't play any role in this as far as I'm concerned. They show up in the introduction and then they disappear from it and we talk about why they disappear from it. So we did address the DARPA interests in a general way and IPTO interests in a very specific way. We did detail the relationship between IPTO officers and DARPA officers. We never considered intensely DARPA's interaction with other DOD agencies. Nor did we examine closely DARPAS's relationship with the Secretary of Defense's office. But we understood enough about these relationships, I think, to know what IPTO tried to do with directives from their superiors. The best part of the book on the type of support, the way it was obtained, the changing specifications in order to get the right result from IPTO's perspective. All of that is detailed in the case studies. The management structure changes…the management approach changes over time. From the beginning, it was blue sky research, in a certain sense with big bucks regardless of what was needed to get the job done. There was no economic testing of any kind. This was just we'll pay what it needs. There was some change in programmatic approaches when different program directors came in. When Larry Roberts came in he changed in some ways what Taylor was doing. When Saul Amarel came in he changed some other things. IPTO began to change. The one time that the DARPA offices—the top people—had an effect on IPTO and therefore the overall community was when…terrible with these names…I want to say Heilbron but that isn't right…the guy who came in from RCA anyway who had been a presidential fellow who became head of DARPA—Heilmeier. Heilmeier was insistent that more applications materialize from what they were doing with all this theoretical work—that more work ought to be done that would end up in the military's hands. And that's when test beds became more significant. So he claimed that he redirected parts of the program, but left the program basically intact. The people outside in the universities, and some of the companies like MITRE, thought that he had actually changed the direction of the program. I think he's right. My conclusion was that he's right, and they were wrong. They were beating a personal interest. So we got that right I think. I think that there were too many technical issues discussed to provide uniform understanding for all of them. That just needed more research. So in some respects the AI chapter really isn't good, in other respects there was nothing else out there at the time. Daniel Crevier's book *AI: The Tumultuous History of the Search for AI* [1993] was just coming out, and that's so pro-MIT that it's very hard to compare his book with our chapter on AI. But that needed more work, I think. And it took me a year to write that chapter so I wasn't going to do any more. I think the networking and timesharing are done. I think Judy did a good job in the research and in the initial writing of those two areas. I think the graphics needed a lot more work. The conclusion I think is right on. I believe the conclusion. It's a positive conclusion, which meant that some of our colleagues didn't like it. Bill Leslie didn't like it. He thought I was too generous to the DARPA people and the IPTO people, and since he had this negative attitude toward them anyway, I just took that with a grain of salt. But no other person besides him expressed such an interest in these things. Paul Edwards was very pleased with our conclusions, he thought they were very reasonable.

Aspray: And he's somewhat hostile too, in general.

Norberg: That's true coming out of that Santa Cruz program, that's right.

Aspray: These are sort of two very closely related questions. How important were the DARPA things that you studied to the history of computing? And the related question is how important is this book to the literature on the history of computing?

Norberg: Ok. I can answer that…First of all, this is the period when people were just becoming interested in the Internet and networking and this provided a good grounding for those people, for Lyon and Hafner, and Abbate, and there were a couple of more people in there who were doing some work on IPTO. I think those two books were really very good. I think the graphics didn't come through in our book. As I just said, I don't think that our chapter really ended up being very good in the overall sense, so that chapter has no influence on the development of the history of graphics. The semi-conductor stuff, I think, Ross Bassett used it in forming his own questions about semi-conductor developments and Christophe Lécuyer in some respects did too, because they had studied the book in a seminar with Tim Lenoir at Stanford. But it certainly wouldn't have been a highly challenging set of questions for them, so they built on it, or other bases. So I don't think that did much. It gave a reasonably good account of Mosis and the interaction with the computer companies, if anyone's interested and I don't know who would be. The management chapter is, I think, as good as you're going to get. Somebody else might write something different, but it's not going to be better. So from this perspective, the management chapter is an important contribution to our understanding of *one* part of the government's role in the development of computing. I think because of the influence of IPTO and DARPA and the defense department money on the growth of the field of computing, especially computer artifacts development, that the conclusion that we drew that the computer on your desk in 1996 was really the prodding, or resulted from the prodding of IPTO. I think that conclusion is going to stand the test of time. In that sense it is a contribution to the field. But nobody is studying IPTO now, they've done that. There's very little studying of military impacts being done, a lot of it was done and some of it is out there in easy reach and some of its not. So in that sense I think what I could conclude is that you can pick and choose in our book from a whole range of topics that could contribute to research projects of others, but no one is going to redo that book. I just can't imagine it. The studies of DARPA as a community are not very good. David Dickson's is not very good. Paul Edwards I don't think hit the mark. I think that Waldrop's biography of Licklider has spent too much time talking about kids riding tricycles around the circle inside the Pentagon to really get to the issues. I don't think he could handle the issues. It's a nice biography but as far as giving us any more information about the DARPA community I don't think it does. I basically said that in my review of it, which was never published. So when people begin to do some synthetic work, do a really good history of computing in a fifty year period from 1945 to 1995, they are going to have to take this book into serious consideration. Whether they agree with the conclusions we made or think they are irrelevant, they are going to have to take the book into serious consideration.

Aspray: Let's talk in another context, about the very issue we've just discussed, which is historians of technology, historians of science, business and industry historians and historians more generally, what do they gain from this study?

Norberg: Well, those that are interested in government and the way in which government goes about funding projects and managing projects, agencies which have a different approach (at least for the first thirty-five years or so of its history)…that have a different approach than say procurement from the military, procurement from the Department of Agriculture and so on, that this would help them to understand some of the variations within. Coupled with other reading they do and research they do that would provide them with enough information on variations that it would be a very good contribution for them, for people who study government and science and technology development in the post World War II period. For the business historians, I think there's very little there because we don't really talk about business. Intel is the only company I think that gets any attention and that's really…in emphasizing the contrast between what they would do and what they wouldn't do and what the Defense Department wanted them to do. And so that's very minor. So the business historian is not going to get anything out of it. Eventually, cultural historians might. Nobody has really taken up the Leslie premise, well two Leslie premises. One that the sciences were co-opted and worked for the military and for the defense agencies and defense companies and that that limited what they could do and it then led technical fields down a different path than what they might have gone down if they were left to themselves and given the same amount of money. The second premise in his work, besides the co-optation is that science and technology in the educational world and the university world and in the business world is different than it would have been without the Cold War and without the heavy influence of the Defense Department. There have been a few studies. Ann Markusen, who is now at Minnesota by the way left Berkeley and is now in the Humphrey Institute. Manuel De Landa published a study on AI and defense (*War in the Age of Intelligent Machines*). There are the studies of course on Intel. So there's something there in terms of the technical background that people can use, and once people get around to the cultural study, I think… Going beyond how well the women are doing in the field and get on to what society is like now as a result of all of these items based on semi-conductor developments and the computer based on the tremendous hardware and software developments and application in a whole variety of areas like Cortada's *Digital Hand* book demonstrates, I think, that this line of investigation is going to be important as an indicator of where the push came in the early years. Bill, you've done the work on the NSF, the push was not strong there because they didn't have a lot of money. The DARPA people had a lot of money and therefore they controlled the research agenda for twenty-five years as we pointed out in our study. All this points to a significant cultural phenomenon: It's not just the government being involved in science and technology but it's a cultural phenomenon in the sense of what that stuff is used for and how it really influenced cultural developments. Why we have desktops; why we have laptops that are used all the time; why we have PDAs that now will receive internet messages on them, and so on. All of that has changed the culture. Sometime somebody will get around to studying these aspects of computer development and they will conclude that these aspects reveal that these contributions are far beyond what the developments meant to the technical defense community. The non-defense technical community by and large does not seem to be interested in these later developments. Whether they ever will be I don't know. I sometimes wonder if that's not because they already think they know the story and they don't need to read this. I also wonder sometimes—I talked to Paul Edwards about this a couple of times—I also wonder at times whether or not there are the DARPA people and the anti-DARPA people and that the anti-DARPA people don't want to know about what DARPA did anyway. And so they're not interested and they make up the bulk of the profession. It's a puzzle… …if we had set out, you and me, Bill, had set out to design a project, I don't think that's the project we would have designed. We might very well have gone through with the NSF study because of its value in educational circles, what they did for education, what they did for the community at large in getting it tools for research. I think Bob Seidel's work on the computer as a scientific instrument is important in this whole examination of defense work, asking what would have been done without DARPA. There was enough coming out on DARPA, we didn't need to do that. So when a synthetic study is indeed done, when a more complete study of the history of computing is written the authors are going to have to address Stuart Leslie's second premise with respect to computing. They can't avoid it.

Aspray: Yes, I know. I would like to come back to the first ten years of the Babbage Institute. We have talked a lot about the organizational history and the Babbage Foundation, and so on. We have talked about the nature of the community. We have talked about some of the things that you did programmatically or individually in talking to people to build up the profession, build up the knowledge of the field. What was the role of you and what was the role of Babbage Institute in building that?

Norberg: O.K. There are a number of responses I can offer to that question. The first is that the Babbage program really begins in 1983, when you arrive. Up to that time the Institute activity was largely concerned with organization. Of course, we had the graduate students doing some work, some of which was useful, but in the end most of it was not. In 1982, I was still feeling my way around the university and the funding community and trying to understand where I was going to go with all of this. I had no help basically because it is not like the meeting we had with Tom Misa yesterday where we have a fund of knowledge about the past twenty five years and that is going to be very valuable to him when he walks in the door and takes over as director. But I didn't have that. There was no one to give me that kind of advice and the advice I was getting from Erwin, which was very good, was not directly programmatic. The advice I was getting from Multhauf I didn't like, so I by and large didn't take it. I had some interchanges with Charlie Bashe, who was chairman of the program committee, because I didn't like what he was trying to push us to do and so I wasn't going to do them. There were lots of things going around in 1982 that really didn't come to fruition. Starting in 1983, though, we began to get stronger. In the fall of '83 when you came, the two of us began to design and incorporate a research program for the Institute. And then in January of 1984, we asked Bruce Bruemmer to join us. Then we were really on the go. Now, what did we do. Well, the first thing that you and I did was to attend meetings. I started in the very first year going to various meetings and you were there, I remember various committees and so on. There was a good deal of talk among the converted about what should be done: what sort of publication should be done and how we should and would contribute to the field. Then there were the technical people that we had to assuage so that there was some change in their expectations to the level where we could meet the expectations that they had. Their expectations were too high. Doug Ross kept saying, "You have to save everything! After all memory is going to be cheap, you can put it on a memory disk." No thought about indexing. No thought about appraisal, none of that. What the hell are we going to do? So I thought that was a bit of a problem. But I went to all of these meetings. The first year I went to twenty-two meetings between the fall of 1981 and the fall of 1982. I went to twenty-two meetings in various groups: archival, historical, technical, in some cases...I was going to say social but that's not right…but new organizations that were trying to look at new ideas, like the National Commission on Preservation and Access from the library side. I went to those meetings for a while. Basically, what I was doing was not trying to get them to change their agendas, I was trying to understand their agendas, and how they related to ours. I was also trying to let them know that we were there, that we wanted to help, and we had a mission of our own and that we would like them to take that into consideration. We received a reasonably good response from them, except for the group running Pioneer Days at NCC. We went to these Pioneer Days, and I think, those people assumed I could not contribute anything. Maurice Wilkes was very rude. Every time I saw him he was very rude. They had their house historian—Hank Tropp—and did not need any criticism from us. I didn't have much respect for Tropp's historical work. Nice man, but he wasn't an historian. I didn't think much of Jim Tamako's historical work either. Maybe instinctively they knew I wasn't going to take advice from them. As a result, Pioneer Days were a real trial for me. I would go and listen, but I had no contributions to make. Bill you showed a flare for interacting with the community, much more than I do, much more of a flare…I don't have that flare. So you began taking on the responsibility of keeping track of everybody, talking to them on the phone about their projects or to get material for the newsletter so that we could both keep up with them and get their information out to others who might be interested in it. So I just backed away from that and now I hardly do it at all. Jeff does it all now. I don't talk to reporters anymore. When students call, Jeff takes the calls usually and tries to help them out. So I learned that I didn't have to do that, since I wasn't good at it anyway and I didn't want to be doing it. I realized that somebody else could take that on. But that benefited Babbage a great deal. It got the name around. Ed Layton used to tell me when he would come back from trips to meetings and especially trips to Europe, how much people told him about the Babbage and how good it was to know the organization. That was different. Here was somebody who was totally independent of the Babbage and totally independent of me, and people who I didn't know were telling him they knew about the Babbage. And they knew about it because of what we were doing in publication. They knew about CBI because of the contacts that you both had made with these people outside. They knew it because of our public documents, the newsletter and various other things we submitted for their perusal. So in that sense in the early years, I would say from 1983—I'm not willing to go back as far as 1981—from 1983 to 1990 or so CBI's impact on the field was that the people who wanted to look at problems in some phase of computing or information processing, they learned that there was an organization that could help them. As much as anything else, that established our reputation. Then when we started an all court press on collection development people began coming to use the collections. In those days, email was still pretty primitive so people corresponded with us by USPS and not by email. Most people came to the Institute at one time or another to use the materials and that sparked a whole new attitude toward CBI. The collections and visitors gradually grew. We kept to our program. We varied the topics; we did not vary the program. I would not relent on the basic aspects of the program. And once the directors of the Babbage Foundation realized that, they never brought the subject up again. I wasn't going to change my view. But we did enough for them that they seemed to feel happy until they realized they couldn't raise any money on that basis. So we influenced a group of scholars by providing knowledge, by providing resources, by providing information of resources elsewhere, and by a little handholding once in a while—when people would either come or call. I think I was less effective in the technical community. I was so turned off by the NCC meetings and by dealing with the people there and generally the AIFIPS leaders, I really didn't do much about the technical community. That was a mistake. I should have either found somebody to interact with them. The other failing along this line is that I did not take the library seriously enough and now we are paying the price. I should have started some cooperative projects with the library very early, back in the eighties. And I didn't do it. So we had no influence there. So our influence is largely outside, outside the Minnesota community. As many attempts as we made to try to work with the people inside Minnesota, we achieved little. The 1986 meeting on ERA and the past, present, and future of computing conference was really the best we did. And while that was a good conference, I think, it had no follow through.

Aspray: What is the relationship then and the importance of the Babbage to the History of the Science and Technology program at Minnesota?

Norberg: CBI has supported graduate students from time to time. People who praised the Babbage thought well of the History of Science and Technology program and people who looked at the History of Science and Technology program saw the Babbage as a good adjunct to it. This sort of mutual admiration society, such as it was, was beneficial to both sides I think. It certainly made an important contribution when the program was going after the research and training grant from the NSF. And the Babbage and the Bakken Library were featured strongly and they are both very good research resources so that helped. I think there was a tension between me and some of the History of Science and Technology faculty, because I wanted hands off CBI. This is my baby not yours, you're not going to tell me how to run it and I'm not going to do all the things you want because I have other responsibilities. And that rankled from time to time, because there were some things I just simply wouldn't do. I think that they expected, some of them anyway, expected that whenever there was money I would support graduate students. Well, I didn't do that. I supported graduate students when I needed them. At times when we were flush with money, some faculty thought we should give some money to HST. The hell with that. I wasn't going to do either. And by and large I didn't attend very many functions. I went to Colloquium in the beginning on a regular basis, but over time I fell off and went on an irregular schedule, until I stopped going altogether. I think there were mutual benefits, but they are slender and tenuous to be sure. I think the University has seen it—the University…the IT Dean's Office—has seen that CBI is an important example of how broad the Institute of Technology program is on the campus and that we don't just do nerdy technical work but we do these other things as well. The Deans trot CBI out whenever it seems convenient to them, but they've never trotted me out to do anything, except once, to the legislature. When I went to California to participate in a Dean's "show and tell" meeting and to participate in a couple of other activities with the Charles Babbage Foundation, the Dean, Ted Davis at the time, was talking about developments in electrical engineering and mechanical engineering. I was sitting right in front of him and he never once mentioned the Babbage Institute and yet there were two people in the room that Erwin and I were sweet talking at the time. I was really offended with that. He introduced Mostafa Kaveh as head of EE. Oh boy, I thought that was really bad form. So you know they trotted us out when they thought it was useful to them, but if they didn’t care, they didn’t. We never got any regular help while I was director from the University on fundraising. And that’s a failing of the university as far as I can tell.

Aspray: Did they prevent you from doing things?

Norberg: They tried; they never succeeded. There is a pecking order in fundraising on campus. There are outside people and organizations that are reserved for the right of the president. There is a second group that is reserved to the right of Deans in individual colleges. Then there is a group that is reserved for special projects and special building capital expense projects. And then there is the rest. Then there is a division between who the Minnesota Foundation can go after and who the colleges can go after and so there has to be coordination between the development officers in the college and the development officers in the Foundation. And then it gets down to me and you look at this list and virtually everybody I wanted to approach was proscribed. Well, I saw that once and I told Erwin, I said, “I’m not going to follow this list.” Bill Drake was one of the one’s who was on the president’s list. So the next time I had lunch with him I said, “Bill I’m not going to pay attention to that list.” He said, “You shouldn’t.” So I didn’t and I just simply kept fundraising with the idea that if they decide that I’m doing wrong they’ll slap my hands. And then we’ll decide whether I want to stay in the Babbage or not. But I never paid a lot of attention to what their goals were because they never paid any attention to mine.

Aspray: And you never really got your hand slapped?

Norberg: No. They didn't care. What the Dean was interested in was having an organization that ran without problems, was funded adequately and didn't run into deficit. That's what the Dean wanted. And the only one who interfered was [Dean Francis] Kolacki [1993-95], of course, when I wasn't there, but he was the only one who interfered with the process. He opened his big mouth at the wrong time.

Aspray: I know we don't have very much time.

Norberg: No, because we'll have to check out by one o'clock.

Yost: I suggest I do a short session in Minneapolis on the more recent period.

Aspray: OK

An Interview with

ARTHUR L. NORBERG

OH 379(part 2)

Conducted by William Aspray and Jeffrey Yost

on

9 February 2006

Minneapolis, MN

[Continuation of Arthur L. Norberg Oral History interview began on January 20, 2006 in Chicago, Illinois, by William Aspray and Jeffrey Yost. The following Minneapolis session was conducted by Jeffrey Yost at the Charles Babbage Institute in consultation with William Aspray]

Charles Babbage Institute
Center for the History of Information Processing
University of Minnesota, Minneapolis

TAPE 3 (side A)

Yost: My name is Jeffrey Yost and I'm here today with CBI Director Arthur Norberg. It's February 9th, 2006, and we're at the CBI offices on the West Bank of the University of Minnesota. Arthur, in our session in Chicago with Bill Aspray, most of the time was spent discussing research projects of CBI and broad policies of the Institute, with less on the development of the collections. Could you spend a bit of time discussing your collection development policy, and how the collection evolved in the early years at CBI? And as part of this could you relate the degree and nature of how your past experience at the Bancroft Library influenced your collection development philosophy, strategy, and management at CBI?

Norberg: Well, I think the interesting thing that an historian who takes over a position like this has going for him or her is that we've spent at least sometime if not a great deal of time working in archives ourselves. So we get a sense of what is present to examine a particular problem we're interested in, in my case, the aspects of Simon Newcomb's career. And we get a sense of what's not there. And if you never take on an archival position, it just goes into your assessment of your particular problem. But once you get into a situation where you have to begin thinking about someone else's future research needs, then at least the thoughtful person, and I'd like to think I'm one of them, the thoughtful person begins to think in much larger...in a much larger context about how to go about collecting. And there, I could argue, I think that the steps from my own archival research in the early to middle '70's...my experience at the Bancroft Library with a particular project that some faculty had designed, not me, were stepping stones to what happened here at the Charles Babbage Institute. They were stepping stones in the following sense. The Newcomb Papers contained 55,000 items, and are in the Library of Congress. And I went through virtually all of them. I also looked at the Naval Observatory and the Nautical Almanac Office records in the National Archives. There was really little arrangement of the Federal Agency records for the 19^{th} century. They had been maintained in metal boxes for year "x", year "y", year "z". So, in order to find anything, you had to go one page at a time. Well you begin to understand that that's taking a lot more time than you'd like to give to a task of that kind. But I did find enough to make it worthwhile. Then when I got to the Bancroft, my predecessors there were Roger Hahn and John Heilbron, both on the history faculty, and James D. Hart who was the director of the library and a professor of English. Hart had written a very important book on the history of American books, and had been a Vice Chancellor of the Berkeley campus. So he was very powerful and very thoughtful in his own right. He was a great fundraiser I might add. This is where I learned some of the things that I know about fundraising. These gentlemen designed this project and they had found the money for it. I had nothing to do with that. I was hired to carry it out. Well, as I think I said last time, the project had two aspects to it. It had the history of physics on one hand, history of nuclear physics, and on the other hand history of the electronics industry on the San Francisco peninsula. This is before the days of Silicon Valley. And they were two very disparate topics to investigate. One was highly academic, i.e., what was going on within the Berkeley Radiation Laboratory on the campus and the other was to discuss with business men what they had in mind when they founded some of those early electronics companies, only a few of which became very famous afterwards like Hewlett and

Packard. Taking another cut, the project was also two fold another way, it was to do collecting, and not offend others collecting records too. Stanford, especially, was very chary about having someone from Berkeley come in on their territory. It proved to be all right in the end. We made a tentative agreement with Stanford that we would go for the Nuclear Physics papers at Berkeley, and we would place them in the Bancroft Library along with the collections of records retained by people from the Berkeley system. An example of this was Glenn Seaborg's papers. Seaborg was a Nobel Laureate and chairman of the AEC during the 1960s and Chancellor of Berkeley when Hart was Vice Chancellor. We would look into the possibility of getting corporate records, maybe even personal records of people who were associated with the electronics industry. They would go to Stanford, by and large. If the person had an association with Stanford, was a graduate, whatever, they would go to Stanford automatically. We didn't even argue about that. If they did not, then it was up to the donor to decide what the donor wanted. So we never ran into any difficulty, but in the process you can see that someone else had defined the collecting strategy. I broadened it somewhat in that I decided that the nuclear physics aspect was too narrow and that there were many other things at the Berkeley Radiation Laboratory and on the campus in the nuclear sciences such as nuclear chemistry and physiology in biomedical and so on, that also used nuclear physics tracer techniques, and various sorts of bombardment procedures for generating the kind of radiation you use in the treatment of some diseases like cancer. This broadened the project substantially. I looked for the money for that and found at least a sufficient amount so that we were able to do that. And when I left 6 years later, I left this aspect of the work in the capable hands of Sally Hughes. And so, I think you can see here that I was thinking of collection development in a much larger sense. If people came to the Bancroft only interested in nuclear physics, they were not going to learn about all this other part of collecting that had been going on regularly. I don't remember how many collections we brought in, but I'll say 50. Virtually all the Berkeley Nobel Prize winners' papers are there. We received the papers of a number of other people, too. Roger Hahn managed to get the Berkeley Laboratory records/Lawrence Papers into the Bancroft before I came. Since the University archives were part of Bancroft Library, department records went to the archives, but for all intents and purposes, the records went into the Bancroft. I worked somewhat closely with the people in the Manuscript Division. That's where the personal papers and the corporate records went. University records went to the archives. One of the women in the division, who did not work for me, was friendly with a couple of people in the biological sciences. She brought in a range of papers in entomology, in various other fields of that kind, so we built up that aspect of the sciences. I think, among the group of us, we built up a very, very strong, 20th century science and technology archive. I brought in some engineers as well especially from electrical and computing and some mathematicians. So there you have it. My job did not include research. Although I was called a research historian I was not required to do research. They wanted me to do research, and I did some research, but that was not required in that job. I was sort of a fish out of water. Was I the only one? Maybe there was one other. I was one of the very few people who had no library duties on a regular basis like serving on the reference desk, handling patron request from all sorts of fields rather than just science and technology. I didn't have that. I didn't have to go public as it were. So I could spend some time on research. We had an oral history program to supplement the collections development and I think before I left in those 5 years I did some 80 interviews. They range anywhere from an hour to fifteen hours on tape. It's very difficult to get Nobel Prize winners to sit down for more than an hour and a half. They just don't want to; they have other things to do. So I did about 80 interviews...other people, Sally did, I think, 25, and then she took over, and another fellow that we hired for the nuclear physics side, he must've done a dozen, maybe 15. And those are all available for research now. Some of them are good interviews and some of them are not so good. Some I eliminated because they made the interviewee, who in some cases was too old by that time, look sort of silly, so we just eliminated those.

Yost: Did the Bancroft have an oral history program, prior to this oral history activity you conducted?

Norberg: Yes, they did. They had a very substantial oral history program that went back maybe 25 years before I got there. It was a very long time and it was mostly oral histories of the memorial type. There was a great deal in them for research purposes, but they were really memorials to the individual who was being interviewed, and I took some cues from them. I certainly didn't think that I knew it all. I had never done any interviews before. So I read some of theirs and I talked to the people in that group to learn how they went about them. One of the things that they did that I picked up and carried on is intensive research before the interview. As many records as you could find, published papers if there were any, and any sort of public documents that might be available in newspapers etc. So I learned from them how to do a good interview I think.

One other thing though about the interviews, and this is the history of science and technology interviews, they were completely separate from the oral history program interviews. We had our own money to conduct oral histories. The oral history office had to raise money for every one of their interviews. Sometimes they were project funds that they managed to get from the State of California. Sometimes they were friends of Ms. X, who wanted an interview of Ms. X done, and they would pay $5,000 to have that done. So it was a difference in approach. Therefore, I could concentrate on who I thought were the important individuals in my fields, and create a good set of oral histories and papers that could then be used for research. If you look at the literature in nuclear physics, in history of electrical engineering, history of electronics, even history of computing, you find the HST interviews cited, sometimes both collections and the oral histories. Indeed, you find them cited rather regularly. So I think we managed to do a good job while I was there. However, at some point during that 5 year, 6 year period, I began to get a little itchy. I wanted to be on the other side of the table to be interviewed like we're doing today. I don't think I was arrogant enough to think that I was as good as Glenn Seaborg as an interview subject. After what he had accomplished, that would be going too far. But I felt I had done a lot and I thought I could really do something myself and so I wanted to get out of academia and get into politics. I worked on the Carter campaign and I tried to get Donald Kennedy to recommend me, he was Carter's agent running the campaign in California. Kennedy is now the editor of *Science*, he was head of the FDA during the Carter administration, and he was president of Stanford. He was very well known, a very well respected person. But he didn't know me very well, if at all, at that time. During the campaign, he had too much to do. But Mel Kranzberg, whose name you know well, was an acquaintance at least, could've even been a friend of Watson…what was Watson's first name, I can't remember now. He was a political scientist who was close to Carter when he was governor in Georgia and he was essentially the deputy campaign director. Hamilton Jordan was the head of the campaign and he was Jordan's deputy. And Mel Kranzberg was on one of the policy committees with Watson. Mel recommended me to Watson and Watson told him to have me send my resume, blah blah blah. Well they were trying to computerize all this. They were getting thousands of resumes as you can imagine. And I wrote that I would make a good Nuclear Regulatory commissioner, which was false, but I was overweening in my ambition, and I felt sure that they might bite for that. When I saw the people who were named I thought, oh boy, was I way out of line. So then I looked for a lower level job and nothing came my way. But I decided I wanted to go to Washington, I wanted to be there. And I was getting tired of just doing oral histories and collecting records, so we went to Washington. I had a year's leave from Berkeley to spend at the National Science Foundation. Then I had a job at the National Science Foundation, and that cured me pretty quickly of Washington D.C. politics. And when the Reagan administration came in, I wasn't interested anymore. And that occasioned the application here, as I talked about last time we met. So I came to the University of Minnesota. Now there's one thing about the oral histories at Bancroft that's important to the succeeding stage here at the Babbage Institute. In some of the fields, I was a novice. I had never studied any physiology; I had never studied any biochemistry; I had never studied organic chemistry; I had not done anything in biology up to that point, except for my specialty field as an historian in evolution, but that was a very confined topic, the way it was covered at Wisconsin. So, I basically knew nothing about these fields and here I was walking in to interview Nobel Prize winners. I started with Melvin Calvin, who did the important work on dark photosynthesis and electron cascading in plants. At the start of a session, we would talk about some family matters he was concerned about. Then I would get us down to the business of the meeting and ask my first question. He'd say "no no no no no no no...that's not the way the question should be asked". And so he would then give me an hour's lecture on whatever the topic was in biochemistry. And then after that I would ask the questions almost in the way he posed them. He went on and on about the topic on tape. Then I'd have to scurry back to the office and I'd have to do some more research because clearly I didn't know enough. The next time this happened also, through about 6 sessions. Well, by that time I had learned a hell of a lot of biochemistry. I had a wonderful tutor in the process. But also in the process I learned that he didn't do it alone. I came across the names of several other people who had published with him or were mentioned in one way or another with his research topics. So I went around and interviewed all of them. They represented their contributions as secondary to Calvin's overall strategy for the research. So I had, I don't know, half a dozen interviews in biochemistry. I did several in organic chemistry, with some of the older people who are still on campus who were members of the National Academy of Sciences. So I began to learn all these others fields. I began to think "my god, in order to do good interviews, you really do have to be solidly prepared. You can't do an interview without knowing the field, no matter what it is!" and that goes for sociology and religion as well as for science I concluded. So when I came here, and was offered this position, I had a conversation with the then dean, Roger Staehle, and with Roger Stuewer, who was head of the History of Science and Technology program at the

*Revised 2014-01-10 to correct spelling of Roger Staehle, p.103.

time. I told them that I wasn't going to come to Minnesota just to replicate the Berkeley Project. If that's what they wanted I'd rather stay in Washington, because I didn't have to leave. And they were, "oh fine, do whatever you want"—as long as it's in the field of history of science and technology—"Do whatever you want. We'd like you to do research; we'd like you to publish" and so on. Well, all promises that are made when you are being interviewed often don't follow through. In CBI, we virtually had unlimited planning time, so we could fit in research and publishing. Basically there was no one looking over my shoulder. I sent the Dean a report every year you know: what we were doing, what we had done, what we'd accomplished, and what we were planning. And I might have sent the annual report to other interested parties, as a courtesy, I don't remember, but I didn't feel any obligation to. I felt we could plan in the Institute as we chose. As a result, we could allot our time for research, for collection development, for public relations, for publishing and so on. In that first 12 year period, when I was director from 1981-1993, we, as a group, tried to allocate our time in that way. When Bruce Bruemmer came to CBI, he didn't believe he needed to do any publishing. Later, this attitude changed and he became a substantial publisher, and a national figure in the archives community. But he didn't think he needed to do that at the beginning. We had to maneuver him into publishing over the course of a few years. He took to it very well, and he enjoyed I think, working with us. I hired Bill, first, and he came aboard in Fall of 1983. I had been here two years by that time. And I hired Bruce, more correctly, the library and IT [Institute of Technology] hired Bruce in January of '84. So then we had our team. We had a very good administrative assistant in LaVonne Molde. Molde essentially ran the place and ran it well. She had been at the University awhile so she knew the ins and outs of how to run an organization in that setting. She did very well by us. So this group set out to design a program. We wanted it to be a combination of research, collection development, and service to the community. The community in this case is hard to define because in a sense it was everybody. We published historical pieces in "Computer User;" we published in the Canadian equivalent of *Datamation*. We helped various teachers' journals by publishing bibliographies on history of computing. We taught courses here at the University, so we did a lot of that public relations. But it was distributed and it was not all a very well defined program, but it did get the name of the Institute around. We attended many meetings as well—meetings for both computer professionals, as well as history professionals. But always in the back of my mind was this experience I had at Berkeley. I broadened my own interests to the point where I included the research and writing in computing. That I had not done at Berkeley. I had published 4 papers while I was there for five years at Berkeley. Two of them were in astronomy, one in chemistry, and one in electronics. So they were interesting pieces, but you know, it didn't take a lot of work to do because I'd already done the research. So I decided I wanted to do something larger when I came here. But it was very clear that trying to do something larger here was going to be very difficult because of all of the tasks we had to do. So while I kept the hope alive, for me, I made sure that Bill had ample time as Associate Director to do research and to publish. You know that story, he published very well and he published many good things which are now seen as classics in the field. So even if I wasn't doing the strong publishing, at least Institute people were. Bruce began publishing in archival journals and began participating in the professional sphere in ways that he hadn't done before. So it looked like a very lively group from the outside. We were doing very well. We translated this research into collection development. Now there, it's a little bit different in terms of how we got to the collection development program that we were interested in. And there I have to say something about Bruce. When Bruce came to us, he had not just archival experience in one organization where he was doing the usual sort of arrangement and making materials available on a confined topic, but he had been heading a project for the Minnesota Historical Society that was statewide in its concept. He was traveling around the state a good deal before he came to us and so learned about various archives and their differences around the state, because that's what they were trying to do. The society was trying to help archives, local historical societies, and local libraries. Bruce learned a good deal from that experience on how to build a larger collection. So when Bruce came to us he was ready. Bill, like me, had done a lot of archival research and so he understood what the problems were, and we talked a good deal about what we should collect. So that was one thing, we had this internal momentum going that we thought was going to carry us through. But at the same time we had this thorn in our side. We had people from the outside who were giving us money whose intention was in the right place, but their knowledge was sadly lacking about archives and archival development. The usual line was, "collect everything. We don't know what's important, so collect everything." Well, that's a cop out. And we knew we'd never have the kind of space that would allow us to do that sort of thing. Furthermore, how do you make these collections available? If you collect everything, what's really significant? So we decided to use the research as our indicator of importance. And that's when the research and the collection development really did come together as an integrated unit. But we had to fight off these people. I remember going to meetings of the history of computing committee of the American Federation of Information Processing Societies and of various other smaller groups and sitting in the room with these people, many of whom were computer scientists and listening to them tell me, “why aren’t you collecting that?” I just gave a reason why. “But you’re not doing this and you’re not doing that...” Well that’s true, we have three people and we can’t do everything. So I was fending people off. In the back of my mind, I had a plan, I would go so far as to say I had a vision of how the collection should develop. In the early years Bruce and Bill agreed with that vision. It had been developed as a group exercise. There were times we went away, went to a fishing cabin up north and we would sit around for 2 or 3 days and talk about where we were going, where we’d been, how we should do this, who we should approach, what was left out of what we were doing, why weren’t we getting certain things and so on, and away from the office and the telephone we discussed all those things. We did that I think three times and it was useful because at the beginning, at least the first one, there was some fighting going on. There were people who had things on their chests they wanted to get off. So we spent a half a day or so just arguing with each other, you know “you’re asking me to do this, and I want to do that” and so on. Then we’d get all the blood letting out and after that we’d begin to converge onto a consensus of what it is we’re doing and why. So we had this vision and I promoted that vision everywhere I went. I stuck to that vision regardless of what anybody said—regardless of the criticism. I remember at one board meeting of the Charles Babbage Foundation, when they were giving us annual gifts, people were telling me—this would be the early ‘80s...that it wasn’t important to do the history now, it was more important to do the archives. I just simply, flatly said “no, that is not right. If we don't do the historical research, we don't have a clear view of what is significant to collect." And then we'd get back into the argument about "you don't know what's significant, so everything has to be significant." No, were not going to do that. So we had what I think is a very clear vision and after a decade roughly, that board members who were still around—after their tenures, board members, our associates in both the historical and the archival community—understood where we were going, they saw what the collection was like, they saw how it was being used and while it was a narrow collection, it was largely mainframe to be sure, it was a good one. And at that point the criticism stopped. So that's how we developed our collection development policy. We had done it as a unit, as 3 people involved, everyone had ownership. I ran CBI by participatory management, because it's too small to start giving directives. I think that's false management in a case of this kind. Therefore, everybody had a say in what was going on. Now I always had the final say, if we didn't agree, but I very seldom used that and I think considering how friendly we've been over the years that this approach had done very well. However, circumstances in the community were not so great. Fundraising was getting more and more difficult. We, the Babbage Foundation primarily, and the institute, started various plans in the middle 1980s when the first recession of the 1980s hit the computer industry. We were making plans to do endowment fund drives and to do a calculated fundraising process with companies and individuals. We hired one after the other, two people actually to do the development work. One of them was OK at it; he was better at other things than that, and when he understood that, he left after a couple of years. The next fellow was with us about 18 months and died suddenly. So we really never got off the ground with a good fundraising program. Nevertheless, we raised almost 3 million dollars, some of that went into the endowment for the chair, as you know, and some of that went to enlarge the endowment fund for the CBI Tomash Fellowship, and some of it went into operating expenses. I think the directors and I did very well at raising the money. It was a group effort. By 1986, the Babbage Foundation had to admit that they were having more and more difficulty raising money. They weren't sure they could continue to keep their commitment to the University. At the time, one of our trustees was also a Regent of the University, Bill Drake. We had a meeting with him. A very nice man, very thoughtful, very optimistic. He tried to be helpful all the time, and was in many ways. The conversation centered around whether or not we could approach the University to get them to pick up more of the cost of CBI. In the fall of 1986, CBI ran its first major conference in Minneapolis. We had a smaller conference two years before for the 5th Anniversary of the Babbage Foundation, but that was mostly insiders. This, however, was the first big conference that we ran. It had two parts to it. It was to celebrate the 40th anniversary of the founding of Engineering Research Associates, and it was to advertise CBI. We titled the conference "The Past, Present, and Future of Computing." We invited, oh I don't know, twelve people I guess, twelve important people in the field to speak. One in architecture, one in semiconductors, one in software engineering, a couple of corporate people, and we ran a conference on the past, on the present, and on the future. It lasted a day and a half. We had a big dinner. We had about 400 people, 350-400 people who attended the conference and 125 that attended the dinner. And at the dinner, John Diebold gave the keynote address. John Parker, the president of ERA back in the forties made comments. Bill Norris, still chairman of Control Data, also made comments and so on. Erwin Tomash, the past chairman and CEO of Dataproducts, also spoke, and a good time was had by all. The retirees had a great time talking with each other and revisiting old events at the reception before that dinner. It was a big reception, 300 people I think. The president of the University, Ken Keller, came to the reception held before the dinner. He wasn't at the dinner, but he came to the reception. I managed to maneuver Bill Drake, Erwin Tomash, and Ken Keller together. And Erwin was going to pop the question about the need for more funds to him. I simply sidled away. I didn't participate in this discussion; I felt it inappropriate for me to stand there while the president was being grilled on whether or not he was going to be able to give more money to us. So I went away. The next day Ken met with them again, especially made a place on his calendar for them. The University agreed to do something. Well, it took 3 years. It took 3 years because it wasn't the president's office that was going to provide the money, Keller bounced it down to the Provost, and the Provost had bounced it down to the Dean of IT. The Dean of IT was resisting. He didn't want to have to pay it.

Yost: Who was the Dean of IT at that point?

Norberg: Ettore Infante, affectionately known as Jim. In the end, he provided $100,000 a year more to CBI. The quid pro quo was that they would take over the Institute. That's when we became, in 1989, when we became an organized research unit of the University. It was also the year in which the ERA Land-Grant Chair was established, which pays the salary of the director as director, it doesn't pay his teaching salary. So therefore there were many things going on simultaneously that solidified this package. There was the promise that the Babbage Foundation would continue to raise more money...try to raise more money I should say. One of the big mistakes that I made in the 1980s was that I decided to go it alone in fundraising. That is, I would use the Babbage Foundation as my source. I would work with them and they, I assumed, would get me into companies and get me to see individuals and we'd be able to raise the money that way. And the University's contribution would just go along as it had been. So I made no contacts with the University of Minnesota Foundation. I made no attempt to work with the development director in the Institute of Technology. That was an office that was just beginning in 1984 I guess, 1985? So by the time we really could've used their help, they didn't know who we were. Indeed, the money that came in was all being deposited to CBF's account and transferred to us. So as far as the University Foundation was concerned, we had one backer besides the University. CBI didn't have a group, so what did the University have to do for us. I assume this is the way they thought.

Then when I came back as director in 1999, I had a meeting with one of the top executives in the University Foundation, who told me I had made a strategic error in my first term as director of CBI. I should have made contact with them, worked in cooperation with them, let them help CBI to raise money. They might've been successful. They didn't say they could have done it, obviously, but they might have been successful. So that's the big mistake I made in fundraising. When the University took us over, there was a reporting line for income from funds received from outside the University. All the fundraising was to be done through the University Foundation. Well, it still took me a few years to learn that. I think when Bob Seidel was director it took him a few years to learn that too, because he believed that the Babbage Foundation was going to do it for

CBI. So that was his mistake number one. Mistake number two of mine I think in that period was not really trying to raise more money for more staff. Because we ended up, the three of us ended up working not just long hours, but intensively working all the time. You know, you get worn out after awhile. In the period when this money crisis was on, I was on the road quite a lot, trying to sweet talk people into giving us money. And as you know, we managed to get enough money, not just the endowment funds, but the operating funds as well. When I left after 12 years as Director, the budget was roughly $350,000, and it had been running that way for 6 or 7 years. We had spent about $4 million since the start, and the debt was about $50,000. So I think we did the job well, but there was a price to be paid for that. At one point, in the 1988-1989 years, we really were facing a much larger deficit. I didn't know how I was going to cover it. Babbage Foundation people were doing their best. The University didn't have any more money to give. There were three retrenchments in the 1980s, so they weren't going to give us any money. They did in 1989, but by that time we were coming out of it. So when the DARPA people approached us to do a history of the IPTO program for them, they'd already been turned down by Roe Smith. And whether Roe recommended us I don't know. When the DARPA contract came along, I saw my salvation, because at first we were proposing a project for a million dollar contract. They thought that was absurd for a history book. At least the computer scientists on the committee thought it was absurd. Anyway, they didn't want to spend that much money on it. So, as I said before, we shaved back the project, and we put in a project for half a million. They gave us the half a million, which meant that we had two-thirds of that for actual expenditures. It covered my salary budget for 3 years, so there was less fundraising, but there was a lot of travel for the DARPA project. We did many interviews and we did all kinds of research in many different places. And as we came to what seemed like the end of that project, we received another $100,000 from them to bring it up to $600,000 almost, so we really were all set as far as the budget was concerned. But the workload was fierce. They wanted a report in 2 years. We couldn't do a report in 2 years. Bill was still here but within 6 months of our assuming that contract, Bill was offered the directorship of the IEEE Center for the History of Electrical Engineering. It was a good opportunity for him, he was ready. He could move up. And I couldn't say "don't go Bill..." so he left. He was interested, I think, and he'd have to say this for himself, but I think he was interested in continuing on the DARPA project, but I simply said no. That there was too much work to do, and working across state borders was not really going to help the project, so I was going to do it here. I would hire Judy O'Neill to help with the project. Judy O'Neill was a graduate student at the time and was working on networking/time sharing. So she was a good fit for the project. So Bill agreed. Bill had just received a grant from the National Science Foundation to do a history of the National Science Foundation's involvement in computing. So he already has his hands full. So he took that with him. It was funded through CBI, but he took that with him and left the DARPA Project behind. That meant a greater portion of the DARPA Project fell on my shoulders. O'Neill was good, but she was a young historian and wanted to draw conclusions as close to the data as possible. I wanted to go further, stretch the data to understand more fully the entire context of IPTO. She also had to finish her dissertation, and she was serving as associate director, so her hands were full too. Towards the end she became pregnant as well. It took me approximately a year to work on just the one section on artificial intelligence. That took a long time. At the same time we were traveling a great deal for the project and other CBI business. Occasionally, I was teaching full time, too, by the way. So by the time we turned in the report in October 1992, four years after we got the contract, I was feeling the weight of CBI. I was tired. Mentally, I was just worn out. We had published a number of things; we had done all the interviews; we had done the report for DARPA. These were the items we had to deliver under the contract. So I approached Erwin and I said "I think it's time for me to step aside. I think the reasons are fairly obvious that the place needs a younger director. It needs someone who is out to make a reputation for him or herself, similar to me I was when I became director. Someone who has new ideas and can move CBI out of the areas we're in now and into new areas. And that will make a continued success of the Institute." Well Erwin was dissatisfied with that decision, but he understood it. And then I went to the Dean and I told him the same thing. It took two years to get somebody to replace me in CBI and on the faculty. So I stayed on for a year until we had somebody and then I was going off, I was going to go on partial leave. I didn't even get a full year's leave like other administrators did. Partial leave so...when it turned out that we weren't going to get the top candidate to come, we had to go to the second candidate who already had commitments for the following year, 1993-1994. The Dean asked me if I would stay on and I simply said "no." I wasn't going to do it. That turned out to be a fateful decision in many respects because O'Neill served as interim for awhile until Bob Seidel showed up and then Bob took it on in the summer of 1994. In the summer of 1995 I found I had cancer. And if I had been director then I don't think I could've done all the jobs that were required so probably it was a good thing that I stepped aside. There was a hint of a health problem in '94, but it wasn't clear so we didn't do anything about it. I assumed my duties as a faculty member, I was treated for cancer for 7 months, during which time I met my classes. I didn't do a lot of research, although I did index the book that year. We rewrote the report entirely to make the DARPA book out of it and make it an integrated story. The DARPA book came out in 1996. It came out two months after I'd finished my treatments. So I did get things done in the 10 months of treatment. If I had had to run the Babbage during the treatment, I couldn't have done that and teach. I wouldn't have had the energy for such a work load. (end of tape)

TAPE 3 (side B)

Norberg: The treatments were once a month and the first week of the month of the treatment was difficult, because of all the drugs...they were putting four drugs in at a time. While the toxins were being absorbed or expelled in the time remaining in the month before the next treatment, the three remaining weeks, I would begin to climb back up again. By the end of two weeks after, I was feeling well enough to have wine with dinner and to go out and do other things besides going to the office. In the 4th week, I was fine even though my psychological outlook started down as I started thinking about the next Friday I would get the next treatment. So it was that sort of roller coaster. Each time, you may know that when you do this type of chemotherapy for an extended period, each time the white blood cell count gets less and less after each treatment, and so you're more and more subject to infection. So toward the end of treatment, I was advised to stay out of theaters and stay out of public places and that sort of thing. But anyway...I went back to work on the ERA book after we had the DARPA book out and it just stumbled along. It was getting better and better, as I said last time, the 20 years made a big difference in the kind of arguments that I made and the kind of resources that were available for use, so it was a good thing from the point of view of scholarship. It was not a good thing from the point of view of thinking about when will I ever get this millstone from around my neck? But anyway, I did part of that and I became director of graduate studies. So I was pretty busy.

Yost: One thing I'd like to go back to is your management philosophy, or your approach to managerial practice. You've mentioned that Erwin Tomash was helpful in the development of your ability and knowledge in terms of management, and perhaps other executives at the Babbage Foundation were as well. Could you speak about that issue a bit?

Norberg: Even though CBI became an organized research center of the University and CBF was a separate organization, CBF and CBI were still very close partners. On the CBF side the people were either Directors or Trustees and sometimes both. When you look over the list of the trustees and directors at the time, there was very rarely a distinction between the trustee and director, the only important directors were those who had fiduciary responsibilities. There were people who had a great deal of important knowledge. For example, James Birkenstock, who had been an assistant to Thomas Watson Jr. during the important years of IBM's shift into computing, had been the person who had helped to negotiate with the Japanese to open up Japan for IBM, was a very knowledgeable person about the trade associations. He had served on some of them. He had been a sort of liaison between the corporations. So Jim was a good source about the computer industry. Jim was a nice man, I don't mean to say he wasn't, but he had a demeanor about him that was reserved. That's the word I want, reserved. So approaching him...I had a little bit of trepidation in approaching Jim, and it took me a long time to understand how I could really benefit from his help. By that point, this would be say 4 years in, by that point we had dinner together at various places around the country when we'd be in the same location and talked about matters associated with CBI. But in his case, he was interested in my liaison with the companies. So that's where he helped me. How to approach industrial people; what sort of information did they want to know about CBI; what was the best way to package our materials. He never talked about how to raise money; how best to package our materials. In keeping with his training at IBM, he was very cautious about anti-trust. So he was convinced we should not be in the policy business, we should not do any research in policy, and that we should try to stay away from anything that smelled of lobbying. He was very careful on the Babbage Board not ever to talk about things that could be seen as collusion among industrial leaders. As an attorney, he was very, very cautious. He kept away from those topics when we were meeting privately. It was always how best to do my work.

My association with Erwin and Adelle Tomash goes back to the middle 1970s. So when I took this position, I already felt comfortable with Erwin. We could talk about almost anything. Even if we didn't agree, it didn't matter. That was the most valuable thing of all because I could say what I thought to him and if he didn't like it, he would say "that isn't right." Then I would listen and there'd be something in his remarks of value for me in running the Institute. Now no one ever said to me, "well, you should use participatory management, it should not be a dictatorial relationship between you and your staff." Nobody ever said that. I came to dislike the way the Bancroft Library was run, because it was dictatorial in many respects, but there's a reason for that in libraries, and maybe the leadership there was correct. But I didn't want to do that. I also had learned something from my conversations with Melvin Calvin of a different kind. I learned a lot about chemistry, as I said a few hours ago...but I also learned his technique of running his laboratory. Once he got the Nobel Prize, he angled for a laboratory of his own, it was really a very nice building, circular building where the center portion was a congregating place and labs and so on all on the side. I think he was the only one who had a closed office. He was dictatorial. I thought that was unfair. So I decided I wasn't going to do that. So there were these influences I'd already had before I started talking to Erwin about how to do things. I remember one time Erwin came to visit us, it was in the first two years because there was only LaVonne and myself participating in the meeting with Erwin, and I started writing down my plan for what the Institute could do, would do, might do. It was clear from Erwin's remarks about what I was putting on the board that he noted an omission. He said that he saw one of the Institute's important contributions as giving the computer community a legacy. I said "I don't do legacy. It's not what we're here for. We're here to do historical research. We're here to provide materials for other people to do historical research. If they want to give a legacy, that's up to them, it's how they use the materials. I don't do that." Well that was I think, our second major disagreement. But Erwin's very polite. He didn't go into a snit about it, and he didn't fight with me about it. The subject of legacy never came up again. In the process, I think there was enough other material on the blackboard that would require a substantial amount of effort that Erwin could leave that aside for the moment. I'm confident that he was just biding his time. He'd wait and see what happened. He was always trying to tell me "when you don't have something, keep something you already have," because I wanted to get rid of the *CBI Newsletter*. Right from the very beginning I wanted to get rid of the Newsletter. Erwin said, "no! You don't have anything else. You'll have no publicity if you don't keep sending out the newsletter." Well that was wise, that was a very wise remark. So we did, we kept it up, and we still do it. But it was things like that, that Erwin understood, "ok, there's a vision; but there's also a day-to-day responsibility that you have to keep if you really want to achieve your goal." That's what I learned from Erwin. And anytime I had a problem, I talked to Erwin. I had a staff problem, I talked to Erwin, I did not talk to the staff. That was just not something that I thought I could do. The staff, when they had their own arguments didn't bring them to me. I knew they had arguments but I didn't say anything. I was not going to get in the middle of it. I was not going to try to legislate or to defend one or the other. I learned that from Erwin too. You give them responsibility, and you let them do it. If they don't do it, then you exercise your authority. But if you do, then you talk about what was right and what was wrong so the next time, they can do it better. So there were many, many of those conversations. I saw Erwin at least twice a year, if not three or four times a year, either in Minnesota or out in California. We corresponded a good deal. This is before e-mail, so it was all going by the U.S. Postal Service. We talked on the telephone a good deal in those early years, and he was a great help. If I was mad at somebody, I could tell Erwin, and it would...it would be diffused through the conversation.

Now other people on the board were different. Bill Drake was a different kind of person, as I said awhile ago. His characteristics were such that I couldn't turn to him for advice about matters that were troubling me, because I didn't feel that I would get the kind of analysis I needed. I might get a good answer, but I don't think I'd get the helpful kind of analysis, because he was so optimistic, he would say "oh no... you are reading it wrong" or whatever. I didn't want to hear that, I wanted to hear some analysis. So I didn't bring those sorts of problems to him, but instead I tried to use him as my wedge into the University. He and Ken Keller were close and so that really worked to our advantage. I tried to use him with the local community to get me in to see people—that worked. He went with me a number of times to see corporate executives here. He gave a lot of money himself. And we'd have good lunches at the Minneapolis Club, have a nice lunch and a couple of drinks. And talk for two hours. At the end of two hours I'd ask him when the next check was coming…I didn't have to say anything else. And the next check arrived at some point later. So Bill was good from that point of view. Walter Bauer is a different kind of individual. Walter Bauer tried to be a good chairman of the Babbage Foundation. He took it over for 4 years in the late 1980s. He tried to lift the vision of the organization to executives higher up on the chain. He made an appointment for us to go and see Thomas Watson Jr. for example. What he wanted to do was to have a computer history hall of fame. They were going to induct Watson as the first candidate. Well, Watson dodged that one, I thought, very smoothly. But anyway, that didn't work. He took me to see Walter Hafner, who was an important Swiss industrialist, and made oodles of money—after WWII and beyond. He was giving us money. He was part of

UCEL in the United States. He was one of the principal venture capitalists at UCEL. So we went to see him. So that's the kind of person Walter was interested in. Erwin was keeping at the level with which he knew people. People at Honeywell, people in Hewlett Packard, people at Unisys, people in whatever that computing software company was down here that Dick Daley ran...Compserve—not Compuserve, but another company—Compserve I think it was called. So he was sticking at a level down from the David Packards and Watsons and so on. I had already done a couple of sessions of interviews with David Packard, so I went back and did a couple more thinking we'd get him into the scheme. That never worked. In fact that interview is simply not available. So Walter Bauer was helpful, tried to be helpful anyway at that end, but because it got so frustrating for him, he decided to step aside. He had a difficult time taking "no" for an answer. And in fundraising there are more "no's" than there ever are "yeses", so you have to just go with the flow as it were. So Bauer was an interesting person in his approach. And there were a few others; a couple of them are dead now. The man who was in Mathematica, what was his name? (Tibor Fabian) We have an interview with him here. And a few other people like that, who had associates in the industrial community. Bernie Galler was good, by the way, in getting some of his software people involved so...he got Bernie Goldstein from Broadview Associates involved at one point. I've forgotten who got John Diebold involved. Maybe it was John Whitmore who was our development director. Maybe he got Diebold involved. I went there to lunch once to talk with Diebold about becoming part of the Babbage Foundation. And he said yes; he became a trustee. So that's the kind of interaction we had. I put a lot of faith in that and I think the faith was justified. I think we didn't get as many gains because of the ups and downs of the economy as we might have otherwise, so in that sense I would put Erwin at the top, I would put Jim Birkenstock next, Bill Drake third, and then the rest as a group. When I came back, there were some equally important people, but only on the fundraising side. Paul Baran, Lee Keet, Bill Coleman from BEA Systems, and one or two more people of that caliber. They were good, but they developed a different agenda for CBF. So working with the new people was not like the early years when I was working with Erwin and his friends. To bring this to a close, I believed that paying attention to these men and learning from them made me a better manager. And I've not been shy about saying that in public either, that I've learned a great deal from those people and I think it made me not only a better manager but a better person because I could deal with people differently than I had before that. I had some rough times at NSF dealing with secretaries, believe me. So there were lessons to be learned there. I did learn and so I didn't have those problems here. It was their advice, their confidence in me, beyond just working together, it was their confidence in me to get the job done. Now they may say that differently, but I'm going to say it this way. That they had confidence that I would get the job done, and therefore they were willing to put up with a little bit of immaturity, a little bit of nonsense, a little bit of arrogance, if the job would get done. I think the testament is that it was done. They were very happy when I left the first time.

Yost: You mentioned that there was a lot of pressure from people in the computer associations and industry to collect everything in the early years and that simply wasn't possible and knowledge had to be gained. And you spoke of how the research program contributed to that. CBI also had a role in helping other organizations and a broader mission in help and advising other organizations how to collect and insure that computing materials were saved, processed, and made available to researchers, realizing that everything couldn't come to CBI. Can you speak about that?

Norberg: Well, with a few rare exceptions where either Bill or I were involved in stimulating a deposit of a collection to be contained by one or another archive, it was really Bruce's doing. Bruce had a collegial attitude about archives development. I guess it was because of my experience with Stanford that I understood that point. He would articulate it, and I had no problem with it. If somebody's papers like Alan Newell's, for example, who spent all his career at Carnegie Mellon, became available, they should go to Carnegie Mellon; they shouldn't come to CBI. We were not in the business of undercutting other organizations. So Bruce handled all of that and Beth Kaplan handled it after she came here. So when you have limited space, and we always knew we would have limited space, unless they gave us all of Walter Library after it was remodeled, we never would have had enough space to take all the collections that had been brought into various archives. Unless we had unlimited space, we really couldn't become a total archive, that is we could not become the only archive for this sort of material. On the other hand, I did not want to do what the American Institute of Physics and the IEEE Center for History of Electrical Engineering did. I didn't want to do what they did, in that in some respects they became the repository of last resort. Because then you just get a bunch of stuff. You don't really have a collection...that's my view. I don't think Spencer Weart would see it that way but...that's my view anyway. You don't really have a strong collection. So we tried to develop a policy to balance these two things. We were going to build a collection, and if you look at our collection here, you see that there are manuals, there is product literature, there are serials. There are publications not widely circulated. These are things that other archives wouldn't be taking, so we made it a point to get all of that material in to CBI as best we could. We rarely took the papers of individuals who were in academia. We took Marvin Stein's papers because he was here at the University of Minnesota and had an illustrious career in the field, but I would have just as soon they had gone to the University Archives, but they didn't. Later on the University Archives saw they should be taking some of those papers, so we had an influence I think there. But we didn't take them from outside unless the person was from industry. Then there was no real home they should go to. So we began taking those. We also understood from our local circumstance, Minnesota, that there were corporate collections that were going begging. When we thought about the Hagley Museum and Library and their substantial collection on industry of the Delaware Valley, we thought that's the model to follow for industrial records...corporate records, and so that's when we began collecting ERA, and we began negotiating for the CDC collection, because it was very clear in the middle...early middle eighties that CDC wasn't going to be keeping their archives forever, they were running into all sorts of money problems. They had a substantial historical archive that Molly Hounds had put together for CDC. Molly was angling to get it to come to CBI too. So we would meet with the CDC executives regularly and finally we managed to get the collection transferred to CBI. So a long struggle, but I think a good one. We learned a lot from it too. We used that as our way into corporate papers. That's when we did the high technology industry study as a follow on to what had been done by the Society of American Archivists on scientific papers.

Bruce and Sheldon Hochheiser did that project. Bill and I did very little of that. So we developed this policy by way of looking ahead toward having a substantial collection that people would use here, but a collection that wouldn't normally be built elsewhere. We weren't going to distribute it elsewhere. That we thought was false. So that's how we put that together. And we never became overwhelmed. I don't think you can ever be overwhelmed with collections. That's where I differ with a number of archivists and librarians. Nevertheless, we had a stream of material coming in that wasn't a steady flow but an oscillatory flow. You'd get many collections one year and almost no collections the following year. So you always had to look at the building of a collection as some sort of average over a number of years in order to see how it was going. And by the early 1990s—it was clear to people outside, it was not so clear to me, I was too wrapped up in the DARPA project and thinking of getting out and so on—it was clear to people outside that we had not done enough with software. That really was a lacuna in the collection. Now I think that was a false impression. The reason I say that is because no one really knew the product manuals collection the way Bruce did and the way I came to know it later on, but there is an awful lot of software in there, manuals for doing programming on Unisys I, II, and so on. So there is a lot of it. Once we made that clear to our constituency their criticism that our collection was too mainframe oriented dampened. Nevertheless, the arguments against our collecting policy were always shifting to meet somebody else's idea of what we should collect. That's when the big software project was designed and funded by NSF. I had nothing to do with that.

Yost: There were also a number of oral histories that have been done in the software area.

Norberg: There were, but they were done largely I think because they were corporate executives not because they were software developers.

Yost: But some number of people in graphics and AI with DARPA funding.

Norberg: Oh, with the DARPA study, we did some interviews with software people there.

Yost: Can you characterize the relationship of CBI to both the Charles Babbage Foundation and the University when you left in 1993?

Norberg: I think that the relationship with the University was good. There had been a change of Deans again—Jim Infante had moved up to be Provost. The University conducted a search for a new CBI director. Gordon Beavers, who had been associate Dean, became interim Dean. It was Jim I went to with my resignation, but it was Gordon Beavers who oversaw the search for the new CBI director. Gordon and I talked a number of times about how to do this, about the state of CBI, about what was needed for the future. He put together a small committee to evaluate CBI. It was me and Bruce and Walter Johnson from physics, who had been an associate Dean and Chairman of the department, Tom Hoffman from the Carlson school, Arnold Cohen from the Babbage Foundation. They essentially did the investigation; evaluated where we'd been, what the original goal was, what had we achieved of it, and so on. We wrote this four or five page single space report for the Dean that was used as the grounds for searching for a new director. Basically this report was endorsed by the search committee and Alan was the head of the search committee as I remember it...I wasn't even on the search committee for that one. I refused.

Yost: Alan Shapiro head of the History of Science and Technology Program?

Norberg: Alan Shapiro head of the History of Science and Technology Program was the chair of the search committee in 1993. The committee was an array of people from different departments. I think Bruce was on it, but I'm not sure that's true. He might've been the library representative on the committee. I was not on it and Bill had left so there was no one else in CBI in the capacity to serve there. Anyway they read this report, they studied CBI and they endorsed it. So that was the University's endorsement of CBI, the measure of success, I won't say total success but the measure of success and the fact that it was doing well by the University. The Babbage people on the other hand: Erwin was still chairman, back in as chairman; Walter Bauer had served four years and Erwin went back in. At my request, I believe. He may have been thinking about going back in, but I asked him to. When I stepped down there were a number of private letters to me from Erwin, from Walter Bauer...from...the man from Mathematica I can't remember his name...starts with a "t"; from Jim Birkenstock. People I'd worked with closely. There were resolutions and testimonials passed by the board. They had a nice dinner for me. Everybody was very polite and very praising, very grateful they said. Indeed, when it looked like we were going to have trouble in getting a replacement, I offered to stay 3 more years. I went to the board meeting and I said, "OK, I'll stay 3 more years, we'll get this organized and we'll run the search again." And Arthur Humphries, I was told later on, I was not in the room when the discussion happened about my proposal, but Arthur Humphries said, according to Erwin, who told me, Humphrey's said "look, he's done a wonderful job for us for 12 years. The Institute is thriving, we're happy with the result, he wants to go, let him go. It's our problem to find the replacement not his." They agreed. So that was the end of that. I would have come back for three or four more years, although I would've ended up with the cancer and so on so Lord knows how effective that term would've been. I think at that time, my personal relationship with the Foundation people was good. By that time we had gotten through all the fighting, there was no more...there were always calls for "you have to save everything after all storage is going to be trivial in cost..." and I would keep saying "but the indexing is not trivial no matter what format you're using." It was all friendly banter at that point. It was never as acrimonious as it often had been sometimes before. So all of that was over. AFIPS was going out of existence; they didn't have any more money coming to us, so they began to serve the organization in a different way. People like Galler and Isaac Auerbach began actively to serve CBF without their former AFIPS tie...so all that was behind us. The vision was clear, the publications were coming out, and it looked great. Everybody could pat themselves on the back and say we did a good job. So from that point of view, the relationship with the Babbage Foundation was very good. The relationship between the University and the Babbage Foundation was always sort of arm's length. The University was always grateful for whatever CBF did for CBI. When the Babbage Foundation came to the University for a meeting, they would also meet with the Dean or the Dean would come to the CBF dinner. The relationship was always very cordial. Deans one after the other, right up to Steven Crouch, made praising statements about CBI. So I think I can be very proud of what was achieved and the staff can be very proud of that too. We've done a job, it may not be the job everybody wanted, it may not be the greatest collection 20 years from now, but it's a damn good one, and it's been valuable to people over the past two decades.

Yost: From the early to middle '90s, to the late '90s you taught full time in the History of Science of Technology Program and served in administrative capacities for the program. Can you talk about that, and especially address whether you enjoyed the opportunity to teach more, and whether you taught courses you hadn't taught before?

Norberg: It seems Jeff that I was always teaching a new course, whether it was a seminar or a lecture course, and regardless of what people tell you, seminars are just as difficult to do the first time if you're going to do them well as any undergraduate course might be, even at the lowest level. During my twelve years as director in the 1980s, I had taught history of computing, Technology and American culture, and the Survey of Technological Development in the United States. I had occasionally offered a seminar, one in the history of military policy and one or two seminars on development of technology, for graduate students, the high end. At that time we were teaching 3 courses a year so people were essentially doing one every term and since I had supposedly a half time teaching load I was doing two a year. I didn't have to do two a year, but I was always doing two a year. These were usually the same courses, although my style is not to teach the same course the same way twice. So while the basis of the course this year becomes the basis for next year, examples change, discussion topics change—as I learn more about a particular topic, or the distribution of students in the class suggest a different range of topics because of their majors. If there were a number of people from agriculture, then I thought I ought to do something for them and so on. So courses were never the same regardless. When I left the Babbage and went back into the faculty full time, Alan Shapiro told me that it was going to be a different world. In my career before that, since getting my Ph.D., I'd always been in groups at one time or another working at Bancroft Library, NSF, whatever. He told me now you're going to be alone. The way our program here runs, you're even more alone because the faculty is not sited in the same area. We have offices in different departments between Minneapolis and St. Paul. So you didn't run into faculty very often, your colleagues in History of Science and Technology, unless it was a designed meeting. So he said "you're going to be in your office by yourself a lot, are you going to be able to handle that?" Well my solution to that of course was to get involved in many things. Immediately when I left CBI I was asked to be director of graduate studies, which for the History of Science and Technology Program, is a major activity—because basically you see to the care and feeding of all the graduate student's programs. Not to the graduate students themselves, but to their programs. The advisors take care of the graduate students. So while you're passing a lot of paper, you're also involved in graduate school policy, study, analysis, and action. You're involved in working with other departments and trying to take care of graduate student program needs and so on. And so it's a busy job, for our program it was small. It's not like computer science, which not only has an admissions committee, but has a full time secretary for the admissions committee. We didn't need that. Basically you did the work yourself. So I did that for four years, and when I came back to the Babbage I gave that up and that's when Sally Kohlstedt took it over. One of those years, Alan wanted to go on leave, so I was asked to be head of the program, which again...during that year we shuffled paper. Alan does a lot more than I did, and he does a lot more policy guidance than I did, because I was only a caretaker for a year. So I did that at the same time that I was teaching full time. I taught several new courses...courses which we already had on the books, but I took them over. The one I liked best, besides American Technology, which I really did like teaching every year, the one I liked best, was a third quarter (now semester) in the History of Science and Technology focusing on the modern period. So I covered 4 topics, I think, in 10 weeks. Biology, physics, which meant relativity and quantum mechanics, evolution and molecular biology, geology, because we had a number of students from geology, so build up to the Darwin story and then plate techtonics later on. And then the fourth area was sort of a catch all. It might be genetics, if there were enough people from the biological sciences, it might be in the...something like chemistry, quantitative chemistry sometimes, and so on. So I managed to build a nice little course around that. I had 2 teaching assistants who helped me design the course and so not only did I get assistance in finding things in the library, developing transparencies, that's before websites, and doing all the things that need to be done to get the course ready, but I was also involved in their education, because they were learning how to teach, and indeed, two of them have made that claim publicly. So there's a multiplicity of teaching involved when you're doing something like that. And I did enjoy that a great deal. Now my enjoyment in teaching began to decline in the 1990s. In part, that was just me getting older. I'll take all the blame. But I shouldn't have to. It's just me getting older, and not really wanting to reach as far as I might've reached ten years earlier. But the external circumstance had changed. Because of all the technology that's available, video technology, programs on television on Roman soldiers and battles and Eastern Europe and whatever, it's an uphill battle to get the students to pay attention to what you're saying. That if it isn't moving, if it isn't coming from a sound system, some how or another, most of them are not paying attention in the undergraduate courses. Now they may be doing so in their majors, but they're not doing it in elective courses. I began to be very distressed with that. And the examples that I was using, I think I've told you this before, the examples that I was using all came from my experience, obviously, and not from theirs. I began to notice when I would use examples...first of all I began to notice that the women tuned out on my examples, because as more and more women were in the class, using examples from the military, using examples from boy films, and boy toys and so on, the women were turned off. So I began changing my examples as a result of that. Then I began to notice that even those examples were outdated. If I talked about a Clint Eastwood movie, they'd know who Clint Eastwood was, but they would have never seen the movie, "The Good the Bad and the Ugly" for example. Ed Layton used to use Charlie and the Chocolate Factory. The opening of that is a good example of mass production techniques. And the students looked at it and they just sort of shrugged their shoulders. So I began to be distressed that I was not able to keep up. I talked with a woman in the English department about that at a dinner party one night—Birdsal. She was head of the writing program, and a professor of English. She told us how some of her experiences were the same. She's probably ten years younger than me. What she related was that she was talking about stereotyping in literature, especially about female stereotyping in literature. People were saying to her, “Oh no, no, that’s gone, that’s not relevant anymore, that doesn’t appear in things we read, it’s not on television and so on...” and so she said “OK. Do you watch...” she started listing shows. Well no, none of them, they didn’t watch any of those. So she said “well, what do you watch?” So they told her. It was things like “Friends” and “Seinfeld”.. Well she never watched them. And neither did I. So she said “well, all right we’ll use those shows. I’ll watch them over the next two weeks. You watch them and we’ll talk about the stereotyping in them; we’ll talk about anything the students told her. So when they came back, indeed the students didn’t have anything to talk about. They didn’t see it in those shows and she started pointing it out to them and so on. And then after awhile they began to get it. But they didn’t think it was important. It upset her that they didn’t think it was important. Of course, we’ve seen a lot of that in the literature since then, how young women today don’t see the problem of gender as women say twenty years ago did. So I began to understand that it was us, her and me, that were the problem here, not the students, but I hung on. I continued to teach the same courses with a few more graduate courses thrown in, just to keep my hand in, and to train my own students. Those I think were the best courses. Indeed one of them on the Industrial Revolution was the best course I gave in my entire career of 44 years. That’s how good I think it was and the students were exceptional. They were Ron Frazzini and Nicholas Bergeron. Bergeron was in there, it was his first year. Frazzini was in his third I think, and then a woman from Rhetoric who was very interested in the topic too. But we did a tremendous amount of work in there. They were work-a-holics for that seminar. It was really very good. So I had enough positive feedback that I could keep going. Then I decided to retire. My health was declining; it was getting harder and harder to keep a schedule. It was very difficult to travel, and so I decided at that time that it was indeed time to go. Once I decided to retire, then my whole attitude changed. When I went into my last class, it was the worst class of my career. It wasn't their fault I think. I had a website up, but it didn't matter. But it just...for some reason, when the course was over, I was glad that I was done. Because if I had had to do that again and had the same experience, I think I would've gone stark raving mad. It was very upsetting, very upsetting to them. Such a bad experience. Now I may exaggerate this. There were certainly people in there who felt that they got a lot out of it, so they said, but still I felt I had failed. And so I was glad that it was over and that I wasn't going to be teaching anymore. So when I returned to direct the Babbage in 1999, I shared a number of those responsibilities, as I said. I stopped teaching the undergraduate courses, I stopped being Director of Graduate Studies. Alan hasn't been on leave since, so there's been no question about who will take over. And I have slowly withdrawn from teaching, although I taught more than half time every year on average. But they were graduate seminars, so a lot different, a lot better. But I am glad it's over though, because I think I lost touch.

Yost: Before we get into your second stint as CBI Director...

Norberg: Can we take a break?

Yost: Sure, let's take a break, and I'll change the tape.

TAPE 4 (Side A)

Yost: Arthur, can you discuss the status of the Babbage Institute when you decided to return, when you were asked to return. And discuss what was going through your mind in making the decision to return to CBI?

Norberg: Well, let's take the second part of the question first. It was rolling through my mind. It wasn't clear to me whether it was a good idea for me to go back into the Babbage, when I was considering this in 1998. It was in the fall of 1998 that we began to consider what was going to happen and the Dean, Ted Davis at the time, the Dean had made an agreement with the History of Science and Technology program faculty about future replacements because we had two people coming up to retirement. Roger Stuewer and Ed Layton. Then it was reasonably thought I'd be next because of my age. I said I would be going. I didn't say when but I said I wasn't going to stay on forever. So what the dean wanted at the time was for us to agree that we replace Stuewer and Layton, but that a Babbage director wouldn't be replaced, that it couldn't be done until either I or Bob Seidel left the faculty. Even then it wasn't clear what they were going to do. So when the dean wanted to make a change in CBI, he asked the faculty to bring somebody back, it was seemingly the view—I can't speak for everybody on the History of Science and Technology faculty about this—but I think the view was that I should go back in—that I was the obvious candidate. It wasn't going to be Sally and it wasn't going to be John Beatty. They just weren't interested in this field. So if somebody is going in different than Seidel, it was me. I understood that. Now I could have said, "Tough luck fellows, I'm not going to do it." But I didn't do that. I evaluated the situation very carefully I believe. I talked to people whose opinions I trust. By and large everyone said that I should go back in. That Babbage's situation was precarious at the time in terms of community relations, in terms of collection development, in terms of productivity, and therefore it needed some new revivification. I said, "Well, the reason I left was I didn't think I could do that anymore. Now you're telling me you want me to go back in and do it anyway." And so I talked with, as I said a number of people, and I was finally convinced that it was in the interest of the University, it was in the interest of CBI and maybe of the community at large for someone to go in and since I was here it would be me. So I agreed. I came back in July 1999 and tried to bring the program back to where it was before and that didn't work. It didn't work for several reasons…outside of personalities…I mean I'm not trying to say it was anybody's fault that it didn't work. But several things had transpired in the previous five or six years. One of them was that there was a whole new staff. You, Jeff were here; Beth Kaplan was just coming in as the Archivist. There was no assistant archivist, Kevin had left. We had been through five secretaries in that period and so on. So how do you mold a new staff under these circumstances, especially when they have ideas of their own because it was not…it was a situation where the people who were there in 1999 were already experienced. It wasn't like bringing in Bruce—bringing Beth Kaplan in was not like bringing in Bruce Bruemmer, who had no real experience in this sort of thing. She had substantial experience with running an archive herself and so on. So how do you deal with that? You Jeff, had already been here a year. You were doing things that I had done before I came back. And I decided that I didn't want to upset your schedule because you were doing it well. Nobody was looking for me to step in. Secondly, I really decided I didn't want to do it anyway and that's all the administrative work you've been doing over the past five years, six years. So here I was coming in to CBI with an equal. I didn't have Bill Aspray coming in to try to learn from me or to participate in a new experience. It was a different staff that had to be addressed. And while I had no problem with that, it meant the program had to be changed somewhat.

There was also the new grant from NSF on the history of software. As I've said publicly, I wish we had never received that grant. That took your time away. It took a great deal of effort to carry out that task; you and Phil Frana did the work, not me. I didn't have anything to do with it. So there was another thing. You were removed from any participation in new things I might want to do because of this obligation. Another thing I was concerned about was I already had pretty substantial obligations elsewhere, too, and I wasn't going to give up on those. I had to finish the ERA book and I was still advisor to this and that, and still teaching. The fundraising looked serious to me. All CBI's contacts had to be reestablished. In the process of reestablishing these, we found that the Babbage Foundation wanted to change its views. They wanted to change their mode of operation, wanted to change their status in the internal revenue code, wanted to have a different kind of an organization both for fundraising and for getting the job done. Some of those things we were doing. As far as I was concerned, they were getting in the way. We tried to accommodate to that. It was nowhere near as acrimonious as the earlier situation with AFIPS, but it was not a comfortable situation either. So that had to be dealt with. Then there was a backlog of material in the Institute that had to be taken care of, made available for research, and so on, and that caused the library staff to spend more time on internal matters than on external matters.

Yost: On processing rather than collection development?

Norberg: That's right. I've always been a firm believer in aggressive collection development and I believed the direction CBI was headed in was not a good direction for CBI. At first I didn't try to do anything about it. Let them catch up, and maybe they would change. But things never did change, that was their style and that's what they did. So I didn't succeed there. What we did succeed in during the last six years was to publish three books. You put out two, I put out one. We developed a conference on software history convened at Xerox PARC. We've regained the initiative in historical research and publication, in public relations, and use of the collections continued right on so there was no problem there. But we regained the initiative and when that was done, when we regained the initiative, it really was time for me to leave and this time for good. So after four years I spoke to the dean, the new dean, Ted Davis, and I said I wanted out. I was going to retire completely this time and that I wasn't coming back. And he was not totally happy with that decision. The reason being, I think, that he believes that where there's no problem, you don't try to fix anything. He was happy with the way we were running things. We weren't asking him for money; we weren't causing any ruckuses around here, so he was quite pleased with what was going on. But I said, "No, I just can't do it anymore." As you know, I'd had two major surgeries in that period, 2002-2003, and I had one problem after another that interfered with what I could do or couldn't do. So it really was time to get a person with energy in here. The reasons are different now than they were in 1993, when I first stepped aside. A project done alone is always one to test you a good deal more than a project done with a group. So I have no qualms about being able to continue in research and to continue in promoting the Babbage Institute, but I think that's unwise now so I'm stepping aside.

Yost: You mentioned the software history project and earlier you mentioned that the Babbage Foundation had pushed the area of software history. CBI as you said had done a number of things in the software history area during your first tenure as director, but that project, your research, the PARC conference, the Paderborn conference were all in this area. CBI seemed to be targeting software more than ever in the past decade. Can you talk about that and was it balancing the field? Also how does it relate to broader research outside of CBI?

Norberg: Well, I don't think I gave much thought to those questions, Jeff, because when I came back in 1999 we were saddled with a three year project. There was nothing you could do; you had to get the project done. Thankfully, you and Phil did it. I didn't agree, as I think I said earlier, I didn't agree that we were so negligent on software. I did agree that we had focused almost exclusively on hardware mainframes so we had to broaden that somewhat. But my justification for not trying to overhaul the program as it existed at the time in 1999, 2000, 2001 was that we've done a lot in other areas. Your bibliographic book on scientific computing, for example, opened up a whole new area for research for other people. The DARPA book discussed all sorts of different topics in the field and made arguments that went way beyond the mainframe area. We hadn't been stagnant by any means. The collections that were being brought in were disparate to be sure, but they weren't trivial. They are significant collections in areas that are outside the mainframe, topically at least, like security, even though that may be largely associated with mainframe machines in the earlier years. Nevertheless, it is a different form of collecting for the field and if that can be continued I think we'd have a great amount of new aspects added to the collection. The interviews that we've been doing go beyond the mainframe area. If you look at the number of people, even if we focus only on the software people who were done for the software project, CBI staff worked on concepts and developments that go way beyond the mainframe. It's just not the same world anymore. Markowitz…

Yost: Harry Markowitz

Norberg: Harry Markowitz. His work, that's not mainframe. So I don't think we should accept a criticism that we have not moved out of that area. So I didn't see any reason to change things. Now we've been working on IBM in areas that are way beyond the mainframe—if you want to agree to that, you may not think the AS/400 is outside the mainframe area but I think it is—but we've begun to focus on different aspects of even industrial development. We are also looking at more social issues than we did before. I think the program just naturally changed on the basis of our response to what we thought was important even in the software area, as judged by the interviews that were done. So I don't think I've changed my view about CBI. I did recognize that the mainframe was a major focus in those early years, but we've gone way beyond it. And to have people criticize us for not doing more on software, I think, is unjustified and rather ignorant on their part.

Yost: You mentioned that we've also gotten more involved in broader contextual issues of the field, you did a paper at the London School of Economics that was part of a conference that's now a volume, and we've both done other research that is looking at policy and social questions. Do you see that as mirroring what's happened in the history of technology over this period, and how do you see the development of the history of technology as a field influencing the directions that CBI headed in its research?

Norberg: One of the advantages of having been in the field for thirty-five years or so…thirty-two years, is that you see cycles come and go. So it's not new that people are working on policy all of a sudden. It is new, I think, that the number of people in the field (history of technology) who have less or no technical background when they enter the field, such as coming out of engineering or coming out of one of the sciences. I think that is a trend quite different. That leads them to be interested in questions of institutional development, of social impacts of competing solutions to problems such as appropriate technology versus high range military technology. These things have played a role in some of the problems that have been selected and even the people who have shifted to look at technical issues, such as Janet Abbate, for example, in a very tentative fashion. That is a trend in the field. Now is that going to affect CBI? We haven't done much on gender, so we can't say that it's going to affect CBI particularly. We have discussed doing interviews with more women, and we certainly have retained collections assembled by women professionals. But I guess maybe I'm just an old white guy who can't see the real issue there, but I'd like to think that the field itself as it grows older is going to change and therefore some of the research that's done will take on the gender question without having to be forced on the Institute as they tried to force the software on us. For people who are interested in some larger international boundary issues, such as Tom Misa is interested in, I think that there is a great deal of opportunity for CBI. Now we tried to do an analogous thing back in the 1980s by trying to either stimulate or become involved with projects in Europe and Japan that were interested in the growth of the field in those geographical localities and that proved to be useful until they ran out of money. We couldn't fund them. And we ran, I don't know how many, six, seven conferences in Europe at various places and in association with other international meetings in order to promote the field there. I think we were reasonably successful, up to a point. So we've tried that but we were doing it on the technical end and so now maybe there is going to be a different approach by Tom in doing it more toward, not so much the social end although he may do that, but more toward institutional development and impact than we did. The field of the History of Technology is bumping up against another tide in cultural studies and it is the fact that so many people are interested in technology now for other reasons, and while they are not interested in the artifacts, they are interested in the role of the iPod, of digital projectors, and the computer on our desks and in our pockets and all sorts of similar goods. They are interested in these problems and not from a history of technology point of view. Their presuppositions are different; their hypotheses about what is to be investigated are different. Indeed, their methods are even different because they are using more of a cultural studies approach; develop a theory and then apply the theory to your research. Historians of technology have traditionally not done that. They might be starting, but they haven't done it yet. I don't think things like social construction fit that category. So that change in cultural studies is going to have an effect on the history of technology I think. I don't see how it can't. Now does it have an effect on CBI? I think yes, because there are a number of collections that we didn't even look for. We don't look for anything in social impacts. The most you can say is that things like Cortada's *Digital Hand* volumes depend on many of CBI's collections from the consulting companies. We received those collections by default; we didn't go out looking for the collections because we assumed that much of the data in the collections was still proprietary. So there's a good example of how you get a collection and then it's used for an entirely different purpose. But I think that as more and more topics are investigated that are outside of the artifacts area we need other collections to meet that need. And where do you get the new ones? I haven't the slightest idea, unless you start taking in large collections, which go well beyond what we do now. For example, if you took in Marshall Fields' and Dayton's papers and tried to see in there where the new computer technologies had an impact on what they are doing, in relation to the overall size of the collections the data part is probably very small. Well, that's a very big archive and I don't see the Babbage doing that. I don't see the library allowing it. They'd expect the Marshall Fields papers to go to the Historical Society and even they probably wouldn't take them.

Yost: Are there organizational records that have looked more at the social impact side of computing and information technology that perhaps would be able to contribute to the study of those fields and topics? I'm thinking of a collection like our collection of the records of Computer Professionals for Social Responsibility and organizations like that.

Norberg: Well, there certainly are some of those and we have a number of them. We have the SHARE papers and we have the USE papers and a few others. We haven't gone after EDUCOM as I mentioned to Tom the other day, EDUCOM and Cause, which I think would be useful for studying such questions in education but I think there are other groups that are probably better disposed to help this area. I think of the center that Tony Oettinger established at Harvard on telecommunications policy and if you look back into their work over twenty-five, thirty years, however long its been going, they, while focusing on policy, have assembled a wide range of historical data that they didn't assemble for historical purposes, but they assembled it for their then present purpose, for policy analysis. Their objectives were different but I think those records would be valuable. I think the various projects that Ithiel de Sola Pool did at MIT—if those papers still exist, and I don't know if they do, I think his papers are there but whether the centers papers are I don't know—they would be useful. And they could be used for this purpose, but they already exist in, if they exist, they exist in archives that are established so they are available. There's the Brookings Institution, which has done quite a large array of studies of this kind over the years, from the impact of technological development all the way through labor relations and effects on the economy and so on, that I would hope would be available too. Again they should be available at the Brookings. There are a few freestanding places although they don't actually come to mind at the moment. There's the center in Virginia on Law and Public Policy. They have done a number of studies on privacy, so there should be historical data in their collections as well. I don't know whether they keep any, but that should go somewhere. There's the I…what's it called… the Institute for Information Sciences, IIS, out at Marina Del Rey that Keith Uncapher started. Those papers should go to USC, but I don't know how much classified material is in there. There could be a lot and if there is, well, who knows what's going to happen there. But that is a collection that would be useful including those investigations done by RAND in this area. You've been to their archives, you know what's being made available at Rand. So there are a lot of these private groups and I suppose Babbage should ensure that the papers are going somewhere so that they could be available and that Babbage could sort of claim a role here that we tried to make sure that there was historical data on these developments on social impacts. But I don't think we have to do it ourselves. I think that it could be brought or kept elsewhere as long as the papers become available.

Yost: Can you discuss the individual research projects you've done in your second tenure as CBI Director? Specifically I'm thinking of the tables project, the policy piece, and the project on the history and historiography of software.

Norberg: Sure. All of them were sort of thrown in my lap. I didn't go looking for any one of them. The initial origins of the policy paper on information policy in the United States since 1943 that is in that international studies book, was a project that came my way while I was away from the Babbage. That came as a consulting job. The U.S. Congress had asked for—somebody in the U.S. Congress—had asked for a study of the government's role in technological development in the recent past between 1960 and

1990. The contract had gone to Langdon Crane, who was an old acquaintance of mine when I was in Washington. I've forgotten who Langdon was with at the time but it was another policy outfit inside the beltway that he worked for. He put together a group of people like Jonathan Coopersmith and me and I've forgotten some of the others now, to prepare essays on government's role in different areas. Jonathan Coopersmith did the fax machine and I did the computer. That was then presented to the Congress as a report; we got paid for it. Jonathan kept telling me that it was going to be published that they were going to publish the whole set of them. Well, time went by and by and by and nothing was happening. Before I came back in 1999, I received an invitation to participate in this conference at the London School of Economics on that topic but now related just to information. Since I knew that I was coming back into the Babbage, I thought this would be a good time to reestablish links with the people in Europe and so I agreed to go. I broadened the paper; I widened it to policy and away from just involvement in the machine artifact or software development and tried to fit it into the theme of the conference. Other people can judge what the value is. But I did that and the other people finished their work. Richard Coopey put out the volume through Oxford University Press. So half the work had been done in the original consulting contract and the other half had been done for and after the conference in London. The second thing you mentioned the table making was for a volume that was being edited by Campbell-Kelly and several of his colleagues, which would be published by Oxford University Press. Basically, what they were trying to do was to examine table development from Sumerian times with clay tablets all the way through to modern times with computers. Some of the essay abstracts looked quite good to me and it certainly deserved to be published, but there was one abstract in the proposal that I especially liked but I thought was incomplete. That was one done by George Wilkins, who was an old acquaintance of mine from astronomy days. George was going to do an essay for them on the shift to computers in the Nautical Almanac Office in England in the 20th century. This would be a major movement away from punch cards into electronic computing. That topic focuses on a procedural issue to me and it doesn't get deeply into the issue of table making in astronomy. I sent my criticism to the press. I said that I thought that there was one lacuna in the set of abstracts and that was the work previous to the astronomy developments of the 20th century. Before then table making was done in a period of great uncertainty and that someone ought to look at the development of the French mathematicians work in the late 18th century and into the 19th century to set the stage for Wilkins piece. Then I sent a message to George telling him what I had done so that George would know that I wasn't criticizing his piece but I thought there was a need for the earlier context, and he agreed. Anyway, that was done, I don't know, some time in late 1999 and by that time CBI and other historians were already involved in the planning for the Paderborn conference. That conference was being planned before I returned to CBI. I was not part of the group. It was Bill Aspray, Ulf Hashagen, Mike Mahoney and Martin Campbell-Kelly. When they heard I was to return to CBI, they invited me to participate in this planning group. Basically, they were looking for a way to define a historical research agenda for software concerns. We studied this pretty intensely. We met at Princeton once, I've forgotten, we met somewhere else once, and I think Bill and Ulf met in Paderborn once. But anyway, we managed to put together what I think was a really good idea, good concept. We then went about finding the people to present papers or comment on sessions. A number of good people agreed to participate. They came from computer science, history of technology, history of business, and sociology of science. I asked David Edge to come, and he agreed. Babbage paid for the U.S. people to go over there; the MuseumsForum paid for the Europeans. Several Japanese attended, but they paid their own way. We organized the format of the conference; we organized the speakers. I wrote the introduction to the conference, which I did too late to send around before people wrote their papers and there was some griping about that, and I think it was justified. I had a vision in my mind about what the conference was going to be and that was not the vision that everybody else had and so it was a bit difficult. But we got by that and we managed to put together a good group of papers and to publish in that volume. But again that fell into my lap. I didn't initiate it; I'd like to think I had a good influence on it, but that's all. So now we're back to the tables paper, because it was at the Paderborn conference that that subject came up. And it was sort of amusing in that nothing had come up during the meeting but on Sunday morning Ulf was driving the three of us to the airport, we were headed home. Martin Campbell-Kelly and Mary Croarken were going back to England and I was coming back to the States. I was sitting in the front with Ulf and Martin and Mary were in the backseat. So Martin said to me, "Hey, by the way I need some advice from you." He talked about this proposal that had gone to the Oxford University Press. The Press asked the editorial group to try to fill the lacuna that had been noted by the reviewer. I was sitting in the front seat trying my best not to laugh. So he goes on and on about this and so well then I couldn't, I just couldn't keep a straight face so I started laughing. And he said, "You proposed it! Well, now you'll have to write the essay." And I said, "Martin, I have it half done. It comes right out of my dissertation." Well, it didn't quite. (chuckles) But anyway that's how I got involved with that one and it returned me to an old love: planetary prediction theory before digital machines. I pulled this stuff out of the dissertation, looked at it and I thought, "No, this is not good enough for this conference and it's not good enough for a publication of the kind that's going to come out from it." So I remodeled it and we gave the papers, it was right after 9/11, and so it was a difficult time. I didn't know whether I'd be able to fly back because 9/11 happened and I flew over there on the 18th. So it was touch and go. Anyway, when I heard all the other papers at the conference I was sort of upset with myself because mine was much too technical, much too dense. I'd almost want to say much too sophisticated but that would be unfair to the others I think. But it wasn't in the same class with all the other papers. And so before we left I said to Martin that I really didn't think my paper fit in with his volume. I believed I could send it somewhere else, so I wasn't concerned about publication. I told him he should think about it and if he thought the same thing then I would just withdraw it. Well, then I started getting comments from people, people like Doron Swade from the Science Museum in London, praising it and talking about how it raised the level of the conference and it was a well thought out approach to the problem and so on. So I began rethinking it. So I sent a note to Martin saying that I had rethought it and that I thought that maybe if I made the following changes that it would work. So he wrote back said he was glad with that, fine go ahead. So I did. And then he turned it over to one of the editors, of which there were four, for assessment and final action. He turned mine over to an Oxford Fellow, a woman, Eleanor Robeson, who is a specialist in Sumerian culture. I thought, "What is this? How are we going to get any meaningful commentary from her?" Well, when the comments came back, boy did I have to eat my words. She was first rate. In addition to just pointing out what she didn't understand, which then allowed me to redo things in a better way, she also made a number of suggestions on organization that were just marvelous, and I just followed them. And that's what you see in the volume. So the volume…My contribution to the volume is much better than it would have been if she hadn't taken it on. They published it. So again that fell in my lap. The IBM project we're doing now, that fell into our lap. We didn't go out looking for that one either. So my second career here, if you want to call it that, is a series of serendipitous events that led to three maybe four publications.

Yost: And, of course, these opportunities come in because of our, CBI's, reputation. There are projects that we do, but there is a substantial number of collaborative projects that are proposed that don't seem to make much sense for the Babbage Institute.

Norberg: That's right, and we've avoided those.

Yost: Can you talk a bit about the project that you're leading and that we're are both currently working on about IBM Rochester's history? What you see as interesting about this particular project, can you assess the value of it?

Norberg: It's the kind of project we've done in other circumstances. But it's also a different project. Here we're back to the legacy question. Are we going to provide a legacy for anybody? While I said back in the 1980s that we're not providing a legacy, in fact we have done that in various ways. But we have not done it with determination. We have not set out to do that. In the case of the IBM project, the IBM Rochester project, my view of it is that we are indeed doing that. Our objective for them, as they have expressed it, is to provide them with a polished accurate statement of their history that is a legacy for them. They recognize the shortcomings of the 25 year anniversary history of IBM Rochester that was done. They understood what the problem was. They didn't have a solution to their dilemma and that's when they called us in. So we went down there, as you know, and we listened to their thoughts and we asked questions about what materials were available to do this and so on and it was a very unsettled circumstance. They really didn't seem to have a clear idea of what the publication they wanted to pass out would be like. They had other ideas about timelines and about aspects of exhibiting and they had arranged with a company to do what I suspect will be a good job at displaying their history. But how do you do this in a text that a public relations person would look at and will think it's fine but couldn't have done it themselves because they don't have the historical background and training? So I think that's what they wanted from us. At first I thought "oh, do I really want to do this?" Partly, I wasn't sure that we could do the job. Not because of our capabilities, but because their records were nonexistent. We found that to be the case to a certain extent. We have had to scramble a great deal to find appropriate materials to use. So it has been a problem. And there was also the problem of many, many cooks working on the broth. I was worried that we would end up in a situation that they'd be tearing each other apart trying to get certain views in there and that we'd be left holding the bag. So that concerned me at first. That proved not to be the case. They've been very open and hands off about the whole thing. We've made certain claims to them about what we would do and what we expected them to do. In addition, they would have carte blanche to approve or disapprove the document in the end that I think has settled their views that they're not going to be stuck with something that they don't want. However in the end, by the time we get it into them, it's only a few months before the celebration so they don't have a hell of a lot of time to find somebody new. Now, what came to mind as I started to look at the IBM technical histories, look at the books, which people down at Rochester have published, especially on the AS/400 series, look at the published materials that is in the employee newspaper, *Rochester News*, the sort of information that was in there and so on, I began to appreciate that there was an important story to be told. They were right; it was like the DARPA story. They were right. This is an important story to tell. And it was a story that wasn't going to be told by the people who were involved, as Frank Soltis has done with his appendices in the two books he did on the AS/400. It really did require a hand from the outside. What the hand from outside could bring to the task was all the accumulated knowledge of the field of the history of computing to be able to set developments at IBM Rochester in a proper context, which is both visible and important, and which is also satisfying. That caught my attention, and that's when I started really working on the project and what I would like to say, in a substantial way, but we don't know whether that's true or not. In any case, we then went to the IBM *Systems Journal* to look at the materials in there. We looked online at the IBM archives site and we managed, I believe, to piece together a record, which if they really understand their history, they're going to like a great deal. So that's a positive reinforcement that keeps people working on the project. It's also a short term project; we're going to spend 4 months on it and that's probably it. Now it has some auxiliary aspects to it. We did get some cash gifts from

IBM, and that helped the Babbage and its budget and helps us with the University and potentially provides more gifts in the future. In addition to that, I would say that we at least, in some ways, solidified our association with IBM again. We had an association with New York IBM in 1980s, but we never had a relationship with IBM Rochester. So now we do. I think that assuming they are satisfied with the result and it does get published in 10,000 copies as they said they might do, then we get a lot of publicity, we get kudos from IBM, that they'll like what we did, and they'd like us in the future. So from that point of view, it's very good. I will tell you that I received an e-mail message yesterday, I'm not sure that it's still up here, an e-mail message yesterday...

TAPE 4 (Side B)

Norberg: Well, I guess I didn't keep it. I'll just paraphrase the remark. The woman that we've been dealing with who's our project director I suppose you'd call her, at IBM, Valerie Pace, has...we've been sending e-mails back and forth with her, keeping her up to date on what we're doing, indicating when something has happened. I wrote thanking them for the help last Tuesday. And so she wrote back a note saying, "It has been a very wonderful experience working with you." So even if they don't like the result, they're certainly happy with the process. So I think it's an example where, we are trying our best to do a good historical statement about IBM Rochester, but doing it in a positive PR sense that they can pass it out to the public and brag about their innovative practices.

Yost: And this has really been the first analysis of mid-range systems as well.

You've touched upon this question, but can you tell me whether you see the IEEE History Center, the Chemical Heritage Foundation, and the AIP History Center as peer institutions and can you compare and contrast CBI to/from those organizations?

Norberg: I certainly would consider them peer institutions, because we do many of the same things although for different reasons and in different ways. We are all interested in the history of the area that we're...working in. We are all in some way or another helping the public to understand what has happened. We align ourselves with the various professional umbrella groups that are above us, such as the Society for the History of Technology, and occasionally we participate together in doing projects or doing various programs. Where we differ is I think at least in the case of AIP and IEEE is that they are inside a professional organization. In the case of these two centers, they exist for the benefit of the members and the history they do provides a line to the major people and events in the academic field they have chosen for their career. Spencer Weart is such a tremendous historian and such a capable administrator that I think he has balanced the two sides of the AIP program and he's carried it out so well. The organization is very happy with the work of its center. I don't know as much about Geselowitz at IEEE and so I can't say the same thing about IEEE. But if you look at the IEEE's site, you can see that they're doing things similar to AIP on their site, I refer to the Albert Einstein exhibit opened in 2005 and the Marie Curie exhibit shown some years ago. They've been putting these virtual exhibits up so that they can educate the public about the history of electrical engineering. But that has taken them farther afield from real solid historical research. While they've done some solid historical research and publishing, to be sure, they have funded other people to go there and pursue a research topic. The connections of its program and member services is not as tightly connected as AIP's activities.

Now the Chemical Heritage Foundation is different. It's different for a variety of reasons not the least of which is money. They have a lot more money than any of the rest of us, any of the other three. Arnold Thackray, as both the organizer and the director, is a very, very capable entrepreneur, thinker, and manipulator. And he is connected, very well connected, too, to other major cultural organizations in the country. Arnold saw in the beginning, what we saw here I think, because he and I discussed a great number of things before he actually founded the [Chemical] Heritage Center. We discussed CBF and CBI and their relationship with the University and so on. And at first, the Center was affiliated with the University of Pennsylvania. So there were many constraints that come from University affiliation. But as time wore on they got more and more money. He separated the two, and he kept tight control over the Heritage Foundation, but kept it separate from the University so that all the money that they have is controlled by Arnold's advisory board and the Foundation, not the University. He also remodeled an historic building, so the center could be in Independence Square a very public place with much foot traffic, rather than having to be on campus. He left a portion of the academic activity within the University even though the center is in Independence Square site. I believe he has the best of both worlds. Arnold began right from the very beginning to provide a legacy for chemistry. He was clear about that. And he has done so. If you look at what they've published, what projects they've done and so on, they're providing a legacy. Now it has the same sort of firm, historical foundation as our Rochester piece, so from that point of view, he's not compromising his professional affiliation, but the topics

*Revised 2014-01-10 to correct spelling of Arnold Thackray, p.155.

are chosen and the activity is done in order to please the community. So they don't do any collecting as far as I know outside of the book area. I would guess that most of the records are going to either Hagley or some place like that; Arnold doesn't want to be bothered with that, or at least he didn't years ago. The other two places don't collect archives, AIP never has. IEEE took them as a last resort and then found other homes for them. So in that sense CBI is different. But we are peers. Indeed, all four organizations have influenced the collecting of scientific and technological records, along with the Hagley.

Yost: What about the Computer History Museum? How is CBI different and are the undertakings of the organizations complementary?

Norberg: Well, that's hard to say. The computer museum's had a rough ride. Rough in the sense that they were established in Massachusetts and that went on for awhile, but they ran into the same problems that CBI did with regard to money. In order to keep it funded and keep programs going, they allied themselves with the school district in Boston and surrounding communities and ran a lot of school programs. That just took away from everything else, and then they began to lose their funding. We had the University to back us up. They didn't have any subsidy. So they just took the collection and lock stock and barrel moved to California. New people became involved, Len Shustek and others from the IT world, and those people, a group of very high level IT executives, became involved in the fundraising and up until 2001 they were going gang busters and doing very well. They had a substantial number of pledges and they had a significant amount of cash. They started by developing public relations programs to get themselves better known, while they were trying to develop the program of exhibits. Then the dot com bubble burst. It was not clear whether all the pledges would be paid. They had to go back out on the fundraising trail again—and just a lot of turmoil over and over and over again. So they had a long stretch in which they were planning, planning, planning. They now, because of the dot com collapse, ironically, they now have a building, which they would've had to build themselves before, they got it at a cheap price I understand.

Yost: The old Silicon Graphics building.

Norberg: Right, in Mountain View. And they have a new grant to develop exhibits. So they're on their way to becoming a real full-fledged museum. They have done some collecting of records. It's been an individual enterprise I think for the most part. They approached this person, that person, another person trying to obtain records. But there's no determined program for archives that I know about. Soon as they get their exhibits up that aspect of their activities could change. They don't have any historians on staff, so how they're going to do their exhibits I don't know. They could certainly hire them now. They have an advisory board of historians. Maybe that will have an influence and so on. But I would say it's five to six years before they're really established as an open, full scale, fully occupied museum. Therefore they can't really address the other problems until that's done. So whether they become an archival unit like the Smithsonian did, whether they do a wide range of oral histories like they claim they'd like to do, that I think is up in the air. They may find that with their programs too distributed, they don't want to have an archives program. Then they'll just become a museum. And then we'll be complementary. A good relationship with CBI could very well be built up. If they try to become not only the museum, but another CBI then they might be caught short. But we get along with the Smithsonian all right and they collect. There's plenty to do, we don't have to concern ourselves with competition.

Yost: Can you characterize the Babbage Institute and what overall it has meant to the history of computing and historians of computing as a profession, after more than 25 years of its history as an organization?

Norberg: Well we certainly hinted at the answer to that question by talking about the use of the CBI collection and the large number of publications that have emerged from CBI by us and by others from outside who used the collections. In the beginning, and I mean that right from 1978 on, even before I came to the Babbage Institute, it was a rallying point for people interested in this topic. As you know, a number of people's work was supported both in the [Adelle and Erwin Tomash] Fellowship Program as well as by private gifts that they received from us or from others. I would guess, and it's hard to say what the feelings of these people were, but I have the impression that at least some of them believed that CBI's existence gave them an authority in the historical profession that they might not have had otherwise. If you look at some areas in the history of technology where there isn't a large collection, people have to align themselves with other groups, other special interest groups and that sort of thing, in order to be able to do what they want to do. Jennifer Light at Northwestern is a good example of that. So I think that CBI became a place where people could identify, yes, there is this organization, it's sitting out here in Minnesota and we're doing our research there, and there's always an interchange between the two. That got stronger as the years went on. It got stronger because we had more and more fellows who obtained degrees and published their dissertations and published other work related to their dissertations. It also changed because people who were already doing research in the field, people like Richard Rosenbloom at Harvard Business School were already working on executive programs within the IT community and these scholars began using our collections or consulting with us. They asked us what we thought about why this company or that company didn't get to do what it wanted, or when it did it was greatly successful. So we began to form a...not an independent community because we never tried to do that, we always tried to establish it under Society for the History of Technology, but we had a community that could say if the Society for the History of Technology went away we wouldn't, we're still here. The more publications that emerged from CBI gave rise to a strong impression that Babbage is playing a major role in the field. If you just think back to last November when the Society for the History of Technology met here in Minneapolis, many people, who attended the meeting, visited our reading room that week and the following week. The week before the meeting, the meeting, and the following week after the meeting, how many people were here looking at materials? It's a remarkable tribute to the kind of collection that we have. So all of those things I think place us at the forefront of the field. We aided it; we abetted it; we...criticized it; we carried it along when there were weaknesses. *Annals* did the same thing, so we're not alone. There are other players in this community. *Annals* had an influence similar to CBI's, and a number of conferences also contributed to the development of the field.

Yost: What is the relationship of the Charles Babbage Institute today to the Charles Babbage Foundation and to the University of Minnesota?

Norberg: Well, to the University of Minnesota it's a simple answer, we're an organized research unit within the Institute of Technology, which means that faculty serve here and that we have responsibilities to the University, not just to raise money but also to participate in the education of young people. Our relationship with the University is essentially that we work for them as an organized research unit. We have standing in the college; we have standing among our peers in the University. The Babbage Foundation is a different case. Starting from being a half owner of the Institute along with the University after 1980, to trying to become a foundation in the sense of the Sloan Foundation, their view of their association with us has changed remarkably. In part, that was a fundraising issue. When the Babbage Foundation was originally established in 1980, it was to be an operating foundation, it passed money through. It collected the money from whomever, and passed the money from the donor to CBI; it was only an operating foundation. But an operating foundation has limitations for the donors in terms of how much the donor can deduct from his/her income taxes. And it's 20%, 25%, 30%, it's not 50% and it's not 100%. And so to ask a donor for big dollars like the University does, if someone gives 10 million dollars to the University depending on their tax status, of course, they can write up to the whole amount off. But if they gave it to the Babbage

Foundation 10 years ago they couldn't do that. It was limited. So the Babbage Foundation people wanted to change that situation because they thought they could attract larger amounts of money and they were probably right. So they decided to revamp their organizational structure, their infrastructure. Once that decision was made, then a whole series of other consequent decisions had to be made. If you are going to become a charitable foundation, you can't just be supporting one activity. That's not allowed by the rules. So they have to open it up to others. It has to be an open situation. Now you can define what you're going to do. You can be in biomedical devices; you can be in information technology; you can provide aid to dependent children, it doesn't matter. But you can't say that you're going to front this person, this group. You cannot fund a particular group unless there are criteria stating why you're going to do it. So the Babbage Foundation had to change its structure. Now their first goal as I understand it has been to raise money. So they hired people to run the organization that would help them to raise money and never settled the question about how are they are going to distribute the money. What are their criteria for projects and support? Are they going to have a rotating focus point, or are they going to focus on just one thing? They just simply did not address those questions for whatever reason. And so CBI must compete with others for the funds that CBF did not raise. They tried to raise money by changing their status, but they haven't been able to raise very much money. They're raising enough to support the CBF infrastructure, but they're not raising enough to give money to anybody else. Consequently, CBF and CBI are now in a hands-off relationship. They have no ownership of CBI; they have no control over us; they can fund us if they choose. CBI doesn't have any say in that matter and neither does the IRS. So in that sense, CBI is completely divorced from CBF. CBI can try to keep up a personal relationship and it can try to keep up a professional relationship, but I don't know what that would take because they haven't defined their program. They know they have to define their program because they've been told that by consultants. As far as I know, they haven't done it yet.

Yost: What do you see as the most important future opportunities and challenges for the Babbage Institute moving forward?

Norberg: Well, I guess I haven't focused on that, because if I had focused on that I would still be running the place. I want to make a statement of my own. What I paid attention to are the problems that CBI is facing, not the least of which is fundraising. CBI has no real firm base of supporters. We have supporters, but we have no firm base of supporters. And as years go by we lose supporters for one reason or another. We lost two this year (2006) through death. CBI's funding is still not stable and large enough. It appears as though that's not going to change anytime soon. It's going to continue like that unless some big donor comes along like they did for the Chemical Heritage Center. So that's problem number one for me. It's always been problem number one for me, right from the beginning. And I don't see it changing. This problem has to be addressed. I've told this to Tom, too. I've told it to the library people. I've told it to the Dean's office. The second problem I think or the second focus ought to be broadening the program beyond its present focus. It should broaden in two ways: one the line of research should change somewhat so as to improve the collection in new ways. And that

I haven't given a lot of thought to. I mean, I'm old fashioned in thinking that we should do more work in networking and we should do more work in areas that are on the forefront of computing like DNA computing. CBI needs to keep up with the field. We can't do everything. So I'm at a loss to say let's do this or let's do that since I'm leaving. So I haven't done that but I see that there are opportunities out there that should be focused on. From my point of view, they tend to be technical however, or business like, and firms, rather than social, and rather than policy. I've always believed that being in Minnesota you're too far away from the policy community unless it's the state house and so it's not a comfortable set of tasks. I found when I was still looking at policy developments in the early 1980s that Washington was too far away, I couldn't keep track. I couldn't be there at important meetings and so on. It was not a bus ride for us like it would be from Philadelphia. So I gave up on that, and I think it's not a direction CBI should go. On the social side, whether it's gender cases, impacts in education, a changing society around us as a result of the use of computer technology, all that is for someone else to define. That's not my area, but it certainly is a valid one to look into. And I think Tom is sensitive to this. The business side I think we've covered pretty well. And that could continue if people are interested. The third problem is the staff problem. The Institute is still an organization that is divided. It's been divided since about 1987 when a number of changes were made in reporting lines to the library principally for staff like Bruemmer, and in the route that money flows on campus to CBI and who controlled it. Unlike the AIP or the IEEE Center or the Chemical Heritage Foundation, we are in a position where things are being done by agreement rather than by consensus and control, command and control if you like. And it's always a negotiation process between the two sides of the house, archives and research. I think that's troublesome. I've said it publicly on a number of occasions. I think it's troublesome. I think I can't solve the problem. I wasn't able to certainly, and I wasn't able to convince the people that a new scheme should be introduced. How that's going to play out I don't know. It could be solved with new personalities. Maybe we'll get back to the old days, when Bruce Bruemmer was here and we had very good associations among us, such that everybody worked together in ways that weren't impinged upon by other organizations. The Institute of Technology wasn't asking anything of me, and the library wasn't asking anything of Bruce. Now that's all changed. The librarians are helping to serve a much larger library structure, which is a good idea. I don't deny that that's important and valuable. But for a small institute like CBI this situation is problematic and nobody's willing to address that, at least not up to now. Those are the three problems. The opportunities are abundant. All sorts of research can be done, all sorts of collecting I think can be done so we make headway. Some of the collecting is hard to do, as you well know.

Yost: Does the pace of technological change and especially in information technology necessitate that historians move closer to the present in topics they address and also that we do our best to try and obtain collections that can facilitate such research?

Norberg: I think the second half of that question is certainly true. We have to get collections in those newer technological areas in order to be able to provide a well-rounded collection in the future. I think some of this broadening is being done. The game materials collected by Stanford covers that area, or at least it starts to cover it. The manuals collection that we have is apparently complementary to what the Computer History Museum has. This is an area where more cooperation could be gained between the CHM and CBI. Better access to the collections would lead to more important use of them, especially if we keep moving along and collecting in the new areas such as PC software, which we have not done with any determination. So that I think will have to be done. The first part of the question is a little different I think. Can you repeat the first part of the question for me, sorry?

Yost: The pace of technological change and conducting historical research.

Norberg: Oh, yes. The pace of technological change forces us to come up closer to the present. There's an example that Galler gave years ago that has stuck in my mind. At a board meeting of *Annals* in 1983 we talked about what criteria makes a topic a historical area and how old does it have to be to be a historical question. He had set the limits for *Annals* of 15 years for publishing in the journal. Problems or events had to be older than 15 years to get into the *Annals*. There were people arguing that we should come up closer to the present. I didn't have an opinion one way or the other. In talking to other historians about it…I remember Sheldon Hochheiser…I'll come back to Galler in a minute…I remember Sheldon Hochheiser saying that time limits were not really significant. What was significant is whether or not an issue is closed. When the geopolitical changes came quickly in 1989, historians can view the Cold War in new and more complete ways. I'm not sure I believe that either, because many things are never really closed; they hang on for a very long time. Look at the various political problems we're having now. Some problems have been around sometimes for centuries. So I'm not sure I would agree with him. But Galler said something very interesting that made me take notice of his views. He said "in 1983 if we had decided that the tennis craze of 1979, and '80, and '81 was an important trend in our society," we would have just missed it, because it didn't last. By '83, it was gone. It was a real bubble and it was gone. So to focus on that as an important historical issue, we would've been fooled. When I look at current technology—cell phone development, iPod development, game development, some of the sophisticated medical developments using computers and semiconductors and so on, I try to see it as part of a chain, part of an unfinished entry if you like. Part of a chain that has roots in the past; game technology doesn't start with the game technology developed in the last few decades. There is a connection to earlier developments in semiconductor use in graphics technology that is a way of designing new equipment and so on. So it's not an isolated issue. To study games is fine from the archival point of view. We study it in order to get the right collections, in order to see we have a representative sample of materials for future research.

Yost: Thank you very much for doing this oral history. It has been a pleasure.

Norberg: It has been a pleasure for me, too.