An interview with
Phyllis A. Fox

Conducted by Thomas Haigh
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ABSTRACT

Phyllis Fox grew up in Colorado during the Depression of the 1930s, before earning a degree in mathematics from Wellesley College, Massachusetts during 1944. After graduation she worked for two years at the General Electric Company in Schenectady, NY, first as a human computer and then as operator of a differential analyzer. After earning a bachelor's degree in Electrical Engineering from the University of Colorado in 1948, she went on to obtain a master's in Electrical Engineering (1949) and a Sc.D. in Mathematics (1954), both from MIT. During 1949 she worked with the Whirlwind team, producing a program for the still-unfinished computer as part of her master's thesis. From 1954 to 1958 Fox was on the staff of the AEC Computing Center at the Courant Institute of New York University, working on Univac programs for atomic research. In 1958 she accompanied her husband back to Massachusetts, where she worked as a research associate for MIT, at first with Jay Forrester's industrial dynamics group to develop the DYNAMO simulation language, and then with John McCarthy's artificial intelligence group, for which she wrote the first programming manual for LISP. From 1963 to 1973 Fox was on the faculty of the Newark College of Engineering. In 1973 she moved to Bell Labs, where she had previously undertaken work on mathematical software during the summer of 1967, with Joe Traub and Morven Gentlemen. At Bell Labs she oversaw creation of the PORT mathematical software library and its enhancement through two expanded releases. As its name suggests, PORT was designed to achieve portability across a wide range of machines, something it achieved through the use of machine constants and through use of a common subset of Fortran instructions (enforced via a tool known as the PFORT verifier). Contributors to this project included Norm Schryer, Linda Kaufman, Jim Blue, Wayne Fullerton, David Gay, Andy Hall, Wes Petersen, Stu Feldman and Barbara Ryder. Fox left Bell in 1984, when funding for PORT was eliminated in preparation for AT&T's legally mandated breakup. From 1984 to 1993 Fox was a consultant with Bell Communications Research.

HAIGH: Thank you very much for agreeing to take part in the interview.

FOX: Well, I think it's a very interesting project that you're involved in, and I'm delighted to have you come visit us here.

HAIGH: I wonder if you could begin by talking in general terms about your family background and early life.

FOX: Glad to do so. Stop me if I use too much detail. I was born in Denver, Colorado, in 1923, and went to public schools in Denver. When it came to be high school time, my family was afraid of the local high school which was full, they would say, of many Mexicans, a very hazardous place. So I went to a girl's school, Kent School, in Denver. We were the largest class up to that time, twelve people. It was the Depression. I was brought up in the middle of the Depression, which was very difficult. The school had problems because they couldn't afford teachers. The Latin teacher was good, but there was no English teacher, history teacher, nor math teacher. The assistant head of the school taught mathematics but she didn't know anything about mathematics. One student finally threw a book at her because it was so impossible. So there wasn't much education there at all: no science, no math. But we had a good time, played field hockey, all that kind of thing.

The Depression was hard. My family owned their house but we ate canned spaghetti, and there was essentially no money, at all, coming in. So wherever I went to school, I always went on full scholarship, including the Kent School, where my mother made my uniforms.

HAIGH: What were the occupations of your parents?

FOX: My father was MIT 1912, a mechanical engineer. My mother's father had started a business which they called the Vulcan Ironworks and my father bought into it. So he worked there, designing hoists and things for mines, which was fine until the Depression, and then everything stopped. A terrible time. My mother, who at nine had become quite deaf, raised two children nonetheless. It was a strain on her. She was very highly educated, by herself. She read all the time, a very smart woman I would say. A phenomenal vocabulary. Not scientific at all.

HAIGH: Did you have siblings?

FOX: Yes. I had a brother seven years younger. My mother got married late, as we all do in this family, for some reason. When the Depression hit, my mother was pregnant. My father had a nervous breakdown and went to bed. Then my brother. Denny, was born. He had a very good mathematical gene. But, in time, he became head of the English department at Victoria College, Toronto. He was a Medievalist. He learned to read when he was three. He sat at my mother's knee and would ask, "What word, what word?" And so he learned to read. Once we went out to dinner, I guess we must have had some money by then, and Denny was reading something. It may have been the Bible because he thought it sounded good. (We are totally unreligious, all of us). And the people thought we'd made him memorize these pages. No, he was reading it. So he was quite a reader, and he became a Medievalist of some renown. He died because he kept a pipe in his mouth from seven in the morning until two at night. And, well, you don't want to know more about him. At one point, my mother thought we should know our IQs. So we took IQ tests. That was the big year for determining everyone's IQ. And to Denny's distress, mine was three points higher than his. (It is the mathematics component that makes one do well in such tests.) God, he was mad.

HAIGH: So it was a competitive kind of relationship, then?

FOX: No, we had some separate interests, but we were congenial enough. Yet Mother would say, "Do you want to push the baby around the block in his carriage," and I would say, "No." I wasn't so maternal.

HAIGH: So as you were growing up, did exposure to your father's engineering work give you any kind of interest in science or technology?

FOX: I don't think so. I loved tennis and skiing, and [chuckles] clothes. But I always liked math. They tell me when I was very, very young, I would put myself to sleep with numerical problems and things of that sort. So there's a gene that runs through the family. My niece went to MIT. She's a sort of geometric type. I'm more algebraic, numerical. But no, I never would have thought of doing such a thing, at all. It came much later.

HAIGH: You had implied that the education wasn't terribly good. So were you essentially teaching mathematics to yourself?

FOX: I didn't even bother with it much. I just looked out the window, so I could think. You know, it's like when you're learning to read and you read the school book the first day, what do you do after that? There just wasn't anything interesting to stimulate that, I would say. It was zilch. So I didn't really know any mathematics. But no one at Wellesley did much either, then [chuckles].

HAIGH: So at the point that you were applying for college, were you already aware that you wanted to focus on mathematics, or did that come after you arrived there?

FOX: I was aware of that for the wrong reasons, because if you major in mathematics, you don't have to remember anything, write any papers, read any books. Why not—the lazy approach.

HAIGH: And why did you choose to go to college at Wellesley?

FOX: It was fashionable that year. People went to Wellesley. I really would have like to have gone to Swarthmore, but Wellesley offered me a full scholarship, room and board, and everything else, because I had done well in the College Boards, which at that time were IQ tests. I also did well in the French and Latin ones. The French and Latin teacher got me through those, and the vocabulary area, and I did very well. So I got a full scholarship to Wellesley.

HAIGH: Actually, where is Wellesley?

FOX: In the town of Wellesley itself, in Massachusetts.

HAIGH: And how was the college at that time?

FOX: Very good in various fields. It was especially good in the history of Art, and other things of that sort. There were very few of us majoring in math. Wellesley had one or two good math teachers. But to give you an idea of the general level of people at that time, I took an Economics course. You needed special permission to take 101 Economics. I got in, and the teacher seemed pretty good. But then he was dismayed. It turned out that none of the girls (I'll call them "girls"; now it's "women") in the class knew how to read a graph. So he explained about the x direction and the y direction, and how you could read a graph. That took a good deal of time. It wasn't very advanced. In the Math department the first course would have been algebra and geometry. (These days one takes calculus, I guess, in the junior or sophomore year of high school.) There was a good differential equations teacher. Complex variables, perhaps, a little bit, that was good. So they had women teachers in each of these, and they were quite good. What a difference from the previous school. So that was fine.

I didn't like Wellesley. The whole tenor of it was that the women had to have their engagement diamond by the time they graduated, so the education wasn't too serious. Now it turns out some phenomenal women, I don't want to malign it, because it does one of the best jobs there is around in turning out exceptional women, Hillary [Clinton] and the others.

HAIGH: Were there any courses that you found that were interesting or memorable in areas other than mathematics?

FOX: Philosophy. We had a wonderful teacher of early Greek philosophy, and later Spinoza, and other philosophers. I liked that very much, and thought of majoring in Philosophy. Then I thought it was too difficult, and so I turned back to mathematics as an easy path. Actually, in graduation, I got the mathematics prize. At one point they asked me if I would like to do research in mathematics, but my interest still wasn't enough. And I thought, "Why burn it, because I had, all the way through school, felt I had to get all A's or the scholarship would disappear. There was a lot of pressure that way. But my family didn't apply the pressure. They kept saying, "Don't study so much," but I felt it was necessary to get all As, and that's not conducive to peaceful, speculative thought, exactly. So I studied very hard, but not productively.

HAIGH: So there weren't any areas from your undergraduate experience, then, that you really, really, dug into, or had a feeling you would want to take further?

FOX: No, I had one inkling, which was negative. I took a physics course, because the war was on. You see, I went from the Depression to the war. And the physics I found very, very difficult somehow. I have a friend, she is younger than I, who went to Radcliff and Harvard to major in Physics but had the same reaction. She's very smart, but she realized physics wasn't for her, and ended up majoring in mathematics. The whole question of women's ability in science and mathematics keeps coming up. Do you know Lawrence Summer's and his comments on the matter? See his quote included a bit below.

HAIGH: Oh, yes, yes. Actually, his parents were at Wharton School at the University of Pennsylvania, and I had a slight acquaintance with his mother while I was working there.

FOX: The Wharton School? Summers? Summers must be, he's 40 or 50, I guess, or more. Interesting. Yes.

HAIGH: Yes. I did work for the Real Estate Department there, so I was, actually I got their family Christmas card one year.

FOX: Lawrence Summers became president of Harvard University in 2001. In his effort to improve and expand the university he alienated various faculty members. For example at a conference in 2005 he suggested that innate differences might explain why fewer women than men succeed in math and science. I'm not sure where I stand, but certain people feel there's a slight difference in women and men. I think it's not a conceptual smartness, whatever, but the bias towards math say over physics may be there. Yet there are many very good women physicists. So it still stands to be investigated. (Summers' comments were naturally not well-received, and in general various other unfortunate encounters caused him to resign his presidency in February 2006.)

HAIGH: So you're suggesting there were reasons that not many of the students at Wellesley were interested in science?

FOX: I think that's true, yes. There were so few math majors, maybe six or seven in the entire college. Wellesley was probably (this is a guess) 1,600 students at that point.

HAIGH: Now there were very few women, at that point, in any areas of science and technology. But my impression is that mathematics, and particularly applied mathematics, was an area that women found relatively hospitable compared to hard science.

FOX: Yes. There's an Association for Women in Mathematics, AWM. It's a very good organization and has done a great deal for women mathematicians. There is a book on the history of women from the beginning, in Mathematics called *Complexities*. The book talks about many women mathematicians who were very, very good, despite all the odds. They did beautiful work not in science or engineering, but in pure mathematics, Fermat's theorem and the rest of it. So there are good women. But not at Wellesley, at that point. It didn't come into our sights.

HAIGH: As you approached graduation, did you have any thoughts about career options and how you wanted your life to develop at that point?

FOX: No, I didn't have the diamond…. Most people were settled by then, but I wanted to have a job for the money, but also I wanted to do something for the war. So I interviewed at various places. Amongst them were GE, also the origins of CDC[1] and a few other places. They all sent letters to the math majors, and they offered us Engineering Assistant jobs, at $32 a week, which was very little, even for those times. But they all agreed on that rate, it was a combine.

HAIGH: Now were those interviews for jobs that would have ordinarily been open to women, or had they opened up because of the war?

FOX: A little of each. Women have always been used as desk computers and so on. But they changed the term to "Engineering Assistant." And there was a little scope to do something because there were fewer men around.

HAIGH: So would most people doing those jobs have been working with desk calculators?

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[1] Fox probably means ERA, an early Minnesota computer company eventually merged into the Univac division of Sperry Rand. William C. Norris, who founded CDC in 1957, had been a manager at ERA.

FOX: That's right. I was very good at desk calculators, the Marchant and the Freiden, or whatever they were. Yes, we did desk computations. It was all right. It was a job.

HAIGH: All right. So all the jobs that you were applying for at that point would have been working as a human computer?

FOX: Well, computational, yes. They probably wanted people to do the "grunge" work.

HAIGH: Okay. Did you receive offers from places other than GE?

FOX: I think so, and I think they all ran into a pattern. I'm sure they were a combine. I had a friend who was a math major who lived in Schenectady, which is where GE is. And maybe that influenced me. But GE sounded like an interesting place, at that time, so I took a job there in Schenectady, New York.

HAIGH: Can you describe your experiences of arriving there, the kinds of work that you were doing, and how the job progressed?

FOX: Yes. Well, it wasn't the differential analyzer initially. I was one of a sequence of people. There was a guy there who had been the "young engineer of the world," of the country or something, but he was getting on. And each year, he wanted a new, female graduate from either Smith, Vassar, or Wellesley, who was a Phi Beta. So I was the next one in line. The previous one had quit for a good reason. He didn't allow us a Marchant, a desk computer. He had long columns of numbers for us to add up; he was trying integrate differential equations. But he'd write down all the numbers and we'd have to add up these long lines of things. That was the job all day. Interesting. Well, I found somewhere, by walking around downstairs, there was a desk computer, a Marchant. On my lunch hour, I went and did the day's work and came back. But you had to be careful. They had a soft shoe, that English term, a soft shoe man in the hall, walking up and down, that would watch in each office to see if everybody was working. A wonderful atmosphere. Yes! And so I learned. I got everything done with the Marchant. But down where the Marchant was, the people seemed to have these other interesting things, including the differential analyzer, I got to see it. I don't know if they asked me or if I asked them, but I wanted to work on that very much.

HAIGH: Before we move on to the analyzer, let me just ask a few more questions. So your job there as an "engineering assistant" which in practice came down to essentially doing the calculations for one individual?

FOX: Yes. There was one other small job I got to work on at GE I want to tell you about, just because I thought it was such an interesting thing. The war was on, okay, and there was a very good engineer at GE, who had a project working with the plans for an aircraft carrier. If you've never seen plans for an aircraft carrier, you can't think of the numbers of sheets. And what GE had been asked to do was to look at the wiring and lighting and power in this thing, and how it could survive if it took hits. So we went through these diagrams of the aircraft carrier to see what would happen if there were hits here or there, and if there was enough over wiring and everything else that could allow the carrier to go on, even with multiple hits. I was so naïve. I said to them, "What is this thing, WC?" [Chuckles] I didn't know what that was; we were very innocent. But that was the most interesting, a classified job, essentially. And I think it probably helped a bit in the war. And then I went to the differential analyzer.

HAIGH: Do you have an idea of whether there would have been many engineering assistants working different offices at GE?

FOX: I have no idea. I was in isolation. The guy walking up and down the hall kept you in your office.

HAIGH: So you really didn't have an opportunity to meet other people there?

FOX: No. They did good research there, which is somehow what the differential analyzer was involved with. But no. I didn't have a chance initially to meet people in research..

HAIGH: Well, let's move on to the differential analyzer now. Do you know anything about the history of that particular installation?

FOX: No. On the Vannevar Bush thing, the Net will be better than I will. I don't know how long GE had had it, but there were a couple of engineers who ran it, so I went and helped them. But pretty soon, I got very involved and interested. I used to set up things. The differential analyzer was an analog computer built with mechanical integrators and gears and shafts. It could be used to solve sets of ordinary differential equations. Variables were represented by rotating shafts; addition and multiplication were accomplished by feeding the values into sets of gears - - sometimes differential gears, of course, for direction change. The integrator was in essence a variable-speed gear, and took the form of a rotating horizontal disk on which a small knife-edge wheel rested. The wheel was driven by friction and the gear ratio was altered by varying the distance of the wheel from the axis of rotation of the disk. The integral was the value of the rotation of the small wheel. To multiply a variable by a constant, say 0.756, required determining a chain combination of meshing gears that would multiply the rotation of the input variable to the gears by 0.756 for the ouput shaft. The gears were affixed to the shafts by set screws – a job requiring me to get my hands covered in oil and grease. I learned now to set up and run the machine, and there was a mechanic to help keeping it running and oiled.

HAIGH: Now, what kinds of applications was the machine being used for?

FOX: Well, a lot of classified things. I'll tell you about one problem: The U.S. had gotten from the Germans some V2 bombs and was shooting them off in Texas. We were computing trajectories. It turned out that some bombs were landing in the midst of a huge herd of valuable cattle. So the experiment had to be changed and we had to set up a whole new solution.

HAIGH: Now for you to work the machine, was it necessary for them to tell you anything about the actual application of the problem? Or, did they just give you a set of equations and say, "Solve these"?

FOX: Oh, they could tell me as much, perhaps, as I could use.

HAIGH: When you moved down from the previous GE job to the differential analyzer, did that require you to officially change jobs?

FOX: Yes. This has happened to me alot. I had to make an ultimatum: "I can't work for this guy anymore. I'll quit, or I could work over there." They allowed me to move.

HAIGH: How large was the staff of people associated with the machine?

FOX: Large?

HAIGH: Yes. I mean, you've mentioned that there was always some kind of mechanical person there to keep it working. Were there a number of operators and a supervisor…?

FOX: Oh, no, no. You did it all yourself. It filled a large room but I could set it up, gears and all and run it by myself

HAIGH: So people from different departments in GE would come to you with problems and you would help them?

FOX: They'd probably go to various engineers and scientists to get the equations right. Given a set of equations, then I could set it up.

HAIGH: Now, I mentioned that this is a topic that would be of interest to people. So could you take a few minutes to talk about the experience of learning how to work the machine, whether there were any kind of little tricks that perhaps people who come along and just look at the written records later on would have no way knowing about. Tell me how straightforward it was, what you had to figure out yourself, and what was written down for you—those kinds of things.

FOX: Actually, I don't recall any documentation whatsoever. It was "seat of the pants" kind of activity. Yes, you had to realize a few concepts such as the following: a quantity doesn't haven't to be integrated with respect to time, but the differential could be dx/dv to integrate x with respect to the velocity. So there are things of this nature.

And setting up appropriate gear ratios was complicated. We would, in fact, have pages of constants… if you needed a constant like .756, well, there would be tables that you could use to try to determine the right gear chain ratios, or you could try to work them out from scratch. But though it was straightforward it just was tricky to find the right thing. And yes, I did a few things. One concept I had, and I can't remember its details, one engineer said it wouldn't work, the other guy said, "That's very good. That's going to do it." That was a lovely day. So I got adept at it.

HAIGH: Had there been a previous operator there, who was able to show you how it worked?

FOX: No. The guys set it themselves. I worked, I guess, with them for a while, and they could see that I could do the same, and so they just showed me how to do it, sort of. It isn't very complicated. Well, maybe it is.

HAIGH: So in a sense, then, being the full-time operator was a new position that you had created?

FOX: I guess so, yes. That's right.

HAIGH: And did you have any awareness of the existence of other differential analyzers in other places?

FOX: No. I was in a cocoon. Nothing at all.

HAIGH: You mentioned that you were able to take training courses.

FOX: Oh, training? No, the "ABC" courses that GE offered to their engineers were very difficult. The material went up essentially through a doctorate. They were given at night and offered no credit, but anyone who got through the "C" level was quite remarkable. I tried the first course and I would stay at nights working on these problems that they gave, but I didn't have an engineering degree, and it was very difficult. I persevered with the courses, but I didn't have enough background to really do well. These were all like masters and doctorate level of engineering. And also, the social life was pretty good. The only men left in America were engineers at GE, so socializing was good.

HAIGH: So the mathematical preparation that you had received as an undergraduate wasn't sufficient?

FOX: No. It was the engineering part, the physics and so on. For the analyzer itself you didn't need anything but differential equations, some feeling for magnitude and what was a reasonable answer.

HAIGH: So you were at GE from 1944 to 1946?

FOX: Yes, I'm now eager to tell you how my job ended there. I find this pouring out of me.

HAIGH: Go ahead.

FOX: All right. I really enjoyed GE then, and I was debating, you know, "Shall I stay here, or this…" I probably didn't want to stay in Schenectady and marry an engineer. It just didn't seem the right future. I loved that job. But one day the door opened, and some higher powered person came in with three men. They were probably all in suits, and they looked at the analyzer. I was used to visitors, so I showed them everything. Towards the end of this visit, the big boss who was with them introduced us, and said, "Now, these three gentlemen are going to work here and run this machine, and you will work for them." This is over and over the pattern for many of us women even now. But at that time I wasn't so sensitive about the problems for women working. I thought, "Well, it's true. I'm not an engineer." And they were in the war, and they were engineers. But I didn't want to do that. It didn't seem to me the way to spend my life. So after a while, I decided, "I've got to get an Engineering degree." So I did.

HAIGH: So were those three people basically doing the things that you had been doing on your own before?

FOX: I guess so. I probably left them to it after a while. But, yes, I mean, it wasn't difficult. And I accepted that GE had to re-absorb the engineers. I mean, that was the right thing to do, you know, take them back to their jobs. So I was the "fall" guy—the "fall" woman.

HAIGH: So at that point, you didn't really see a career path within GE?

FOX: No, because you had to be an engineer or a scientist, and I wasn’t. And not everyone wants to spend their life in Schenectady.

HAIGH: That’s true. And it wouldn’t have been possible for you to continue with the classes and get a degree while working there? I would imagine that many of the people within GE would have been able to get an engineering degree while working at the company.

FOX: Oh, no. It was too much of a gap. And they didn’t give credit or anything else.

HAIGH: So they were equivalent to degree level courses, but they didn’t really lead to a degree?

FOX: Yes, because they didn’t want to get into having an engineering school that might compete with Union College in Schenectady. So they just gave more education, and their scientists and engineers were eager to know all that stuff, so they took them.

HAIGH: So in 1948, you left GE and you did a Bachelor of Science degree in Electrical Engineering.

FOX: Yes. It was very hard to get into an engineering school then because of all of the returning vets, especially in electrical engineering. But if you were born in the state as I was, you could get accepted into the University of Colorado, which supposedly had a very good engineering school, with very little tuition, and you could get in. They accepted me.

HAIGH: And when you returned to Colorado, did you go back and live with your family?

FOX: No, I had a room up in Boulder, Colorado. But I would go home on weekends, or skiing, depending.

HAIGH: Can you talk about your experiences there at the University of Colorado?

FOX: Yes. Well, this sounds disloyal, but it was a very old-fashioned engineering school. In many of the courses there were all these formulas you had to memorize. You had to know them by heart, and then when you took the exam, you would know the right one to apply to the problem. Can you envision an education like this? You know, “Look it up in the book, and you can do it.” It was of this kind. For example, there was no requirement to take a course in differential equations, for electrical engineers! There was a great deal of emphasis on drafting, India ink, and doing diagrams. And lettering, lots of lettering. In each lab you had to turn in a very finished lab report, done in India ink with lettering, on the results of the test, whatever it was. As to the branch of engineering to choose, I realized that many of the veterans were going into electronics, and since I figured they would needed power, I would choose the less crowded field and become a power engineer.

HAIGH: Was that related in any way to your experiences at GE?

FOX: No. It was very different. Among the other courses there was one on alternators and generators and things. There was a lab associated with it. While we were running the machines I was set the task of taking notes. I wasn’t forceful enough to bump them out of the way and say, “I want to run this thing,” but I would take the notes. I thought you could learn as much doing that.

HAIGH: Had there'd been women in the engineering school previously?

FOX: No. At MIT my father's classes always had women. But in the engineering school at the University of Colorado, I think there was one other girl there. I had a class with her and we found that none of the engineers would sit near us. Women in an engineering school at that time often had this experience. You accepted that after a while, and you got to know them otherwise. It was an strange situation. At that time the important electrical engineering society was Beta Kappa Nu. They took in star students as members, but never had women. They wouldn't accept women. Nonetheless, I always got along well with all these guys, and so at the final Beta Kappa Nu dinner, they invited me, and I went to the dinner. They even offered me a cigar [chuckles]. I had a lovely time? So that queen bee syndrome, you know, it's seductive a little. You know what I mean? I mean after all, you're the only one.

HAIGH: So as the studies progressed, you were able to forge good relationships with your classmates?

FOX: Oh, yes. Sure. You see, I was perhaps conditioned similarly, really.

HAIGH: And how were things with the instructors?

FOX: Well, they were not very good. They worked very hard, but they just used the same book every year. Of course, you have to give the school credit. It wasn't easy to hire professors at that point. Everyone had been in the war, and there was a great demand. But certainly the preofessors were not very inspiring. And I didn't learn very much at all that year.

HAIGH: Now, did you have a feeling, at that point, that as a woman, a full range of career options within engineering would be open to you, or did you expect to work in very specialized area?

FOX: I don't know that I thought about that. I never looked at it so far ahead as you imply. I just thought, "Well, I'll plug ahead and see if I can get more education," which I thought was always the key. I didn't know where I wanted to go, but it seemed to me that it never hurts to have a degree.

HAIGH: So at that point, you didn't have any specific career goals?

FOX: No, but I always wanted to work. I don't think this was the Depression, it's my nature, probably a German background through my father. You know, work!

HAIGH: Sure. All right. So as you neared graduation from Colorado, what kinds of steps were you taking to find another job?

FOX: Well, you see, I hadn't decided by then. I realized that I hadn't really gotten any education there essentially. So I thought I would try to get more knowledge in something. I looked around the country and thought, "Which school can I possibly get accepted into for a Master's in Engineering?" And I thought the best school at that point was Caltech. It was very good. So I wrote Caltech that I would like to come there and try to get a Master's degree. And nothing happened. I didn't hear for a long, long time. Finally, I got a letter from someone's secretary.

The letter said, "We don't take girls at Caltech." So you see, it kept going along like that. However, in this case I think it was for the best. Cal Tech would have been a mistake for me. So I applied to MIT, and they accpted me and gave me a full research scholarship, and a research associate position, which is much better than a teaching associate because you have time to do some work. This enabled me to work toward the Master's degree.

[Tape 1, Side B]

HAIGH: Now, you had also mentioned that your father had studied at MIT.

FOX: Yes he had a bachelor's degree in Mechanical engineering. He was in the class of 1912.

HAIGH: So did he give you any kind of advice in applying there? Was that a factor in your decision?

FOX: No, I don't think so, really, at all. My parents always let me decide my life. But it was nice to go where he went, and he was always very fond of MIT.

HAIGH: How did you find MIT? I imagine it must have been quite a contrast with Colorado.

FOX: [Chuckles] Yes. But they've always taken girls…women. When I think of it now, the year I had there was really quite challenging, because in one year I got a master's. Now at that point, what they required was a full set of courses of various math and other things, physics, electronics. And a full master's thesis, which I may tell you about, and also a seminar paper. The seminar paper alone was a many page thing. We had to type all of these things ourselves, or I did. So that was all in one year. That was pretty good, I think.

HAIGH: And how long did people usually take?

FOX: I don't know now. I think it takes at least a couple of years. I worked very hard, but I enjoyed it because I found the Whirlwind computer, and life became more beautiful than it did even with the differential analyzer.

HAIGH: Just before we move on to that, I wonder, were there courses associated with the master's that you found particularly interesting, or which led to any of your later work?

FOX: Well, there wasn't much on computers, because at that point, the Electrical Engineering Department thought of computers as sort of a slide rule. They wouldn't have even imagined there being a computer science division. It was a very reactionary department (Though obviously in a very few years the department changed tremendously.) So there was, essentially, no interest and no courses in computing. So it was Whirlwind that really turned me on. I had an idea. Because I had majored in power systems I wanted to try to compute the behavior of a simple power network on a digital computer. At that time another group at MIT had a contract with the government to build a large digital computer. It had to be made with vacuum tubes; there were no transistors or integrated systems around yet. They were designing and building the computer. They called it "Whirlwind.".

HAIGH: Now I know that at least earlier, I believe, MIT had developed a network analyzer.

FOX: MIT had various analogue computers and network simulators, but no automatic digital computer .

HAIGH: Was there any kind of exposure to numerical analysis as part of the engineering degree?

FOX: There was a course in numerical analysis in the math department. It had rather low prestige there because the real research topics at that time dealt with topological spaces, group theory and such. I think I took the numerical course when I was doing my doctorate later. I didn't take it during the masters.

HAIGH: All right. Now you're master's thesis is tied to Whirlwind, isn't it?

FOX: Yes. And I'm much prouder of that than I am of my doctoral dissertation. I set up a fairly simple power system. I don't know power systems any more but then I wanted something you could program and run and see about stability and such things, for network performance. So I approached the electronic engineering group with the idea that I would like to do this for my Master's thesis. But they had no one in the department who would do anything about computers and who would be able to supervise my work. They were older professors, many of them. Eventually they said okay, but they couldn't find me an advisor. Finally they found me a professor who said he would look at it, but the only way he would accept it was if I wrote the whole program out and then did the computation entirely by hand. You see the computer was still being built. So I spent the bulk of the year busy with all this other stuff, doing a computer program in machine language, or assembly language, for the Whirlwind. And every now and then, I'd see that I needed a certain instruction that they hadn't yet included in Whirlwind's commands. And I would tell the Whirlwind engineers about it. I had the timing diagrams showing the paths and timing of the electric pulses, so I knew how the machine worked and what was feasible. For example I found that it would be useful to have an instruction that would add one to the current address, "AO" I called it. It was essentially, I guess, a loop mechanism that I was inventing then. So they changed some of the instruction set on Whirlwind and I think they found it useful to have someone actually writing a program for it.

HAIGH: Now, if you're thesis was in 1949, this would all have been before Whirlwind was fully operational, isn't it?

FOX: That's right, but I could simulate it by hand..

HAIGH: Let's move back, then, in talking in more detail about Whirlwind. Just before we do that, you were suggesting, earlier, I think that women were relatively more prevalent in the engineering courses at MIT. Were there a significant number of female students at that point?

FOX: Not so many. I can tell you one figure because I was interested in this too. At the time I was there, there were 6,000 students, graduates and undergraduates at MIT, and there were 60 women.

HAIGH: Hmmm. And do you think you received equal treatment from the professors there?

FOX: Yes, I think so.

HAIGH: So back to Whirlwind. Was your research assistantship explicitly tied to Whirlwind, or was it something that you had to seek out for yourself?

FOX: Well, they didn't bother too much about what I was doing, but I worked at Whirlwind a lot. I read all the stuff they had on it, all the timing diagrams and how it was constructed, and everything else. I don't know how much I helped them, but maybe here and there.

HAIGH: So as a research assistant you weren't ordered to report to a particular professor and do whatever you were told to do?

FOX: No. I had the master's thesis advisor, but that didn't really count.

HAIGH: So how did you become aware of Whirlwind?

FOX: I'd always been interested in computers, from reading about the early computers and from doing computations. But it was being built, you know, and everyone in the Electrical Engineering Department knew about it, and I went over… Now whether they gave me an official job there, as research assistant, I can't tell you. But it was effectively like that.

HAIGH: You just mentioned that prior to your exposure to Whirlwind, you already had an interest in computing technology. I was wondering if that was something that had come as a result of your exposure to the different analyzer at GE?

FOX: Oh, probably, yes. But even before that. I remember somewhere, giving a paper on it. I tried to summarize early computational devices that people had thought of, because there was some information around. So I was generally interested in computers. I'm not as much a research mathematician. I'm sort of a machine kind of person.

HAIGH: Now, were you already aware of the idea of a digital programmable computer, before seeing Whirlwind?

FOX: Oh, yes. I'd read all about that. Even Babbage's attempts, before the technology was up to it of the the stored programming devices. When I was in Cambridge of course, Harvard had their machine going along. Did you go to college over here?

HAIGH: No. Well, my first two degrees were from the University of Manchester, and then I did my Ph.D. at the University of Pennsylvania.

FOX: Oh, great. So you knew Manchester and all those people, Wilkes and Gill and all his people?

HAIGH: Oh, well actually, I've met Wilkes once, and I've worked with him editing a little thing that he wrote. But those guys were at Cambridge. They built the EDSAC. Manchester built the first stored program computer, but it was very small. I don't think it was much use, though.

FOX: Who was at Manchester, that I knew?

HAIGH: Kilburn, Turing, and Williams of Williams Tube fame.

FOX: Yes, sure. He was Manchester. Oh, yes. That was a great place to go to school, yes.

HAIGH: And then, of course, at Penn, they built the ENIAC.

FOX: Oh, yes. I went down to look at it once. Oh, a wonderful thing, yes. I love them, all these things.

HAIGH: All right. So can you just describe what stage Whirlwind had reached when you first saw it? And what it was really like to be there—how large the group was, and what kind of culture the people had there, and those kinds of things.

FOX: At Whirlwind mostly there was desperation, because people would come to visit from ONR, the Office of Naval Research, which was supporting the project, to see how it was coming along. The admirals would come pounding in, unhappy at the delays and shout "Come on." (As a sidenote, Whirlwind was initially supposed to be a flight simulator, but it changed its direction toward general computation.)

HAIGH: Yes. And 1949, the year you arrived, I think was the darkest point in the project. So people were very anxious about it.

FOX: Right. The tubes failed all the time and all sorts of problems arose. You'd get it going for a minute, and a tube would fail. So they invented marginal checking—other people have too—so they would give it decreased power and see what failed. So they found a way of predicting failure a little bit. There were many very clever people there.

Initially we had to put a program in with toggle switches. There wasn't any way of input or output. Incidentally the paper I wrote for the required master's seminar was on input and output devices for large-scale digital computers. I wrote a nice paper, I have to say, rather comprehensive.

HAIGH: So how large would the group of people associating with the machine have been?

FOX: Oh, I can't say, because some people came and went. And there were the engineers and the scientists. I was going to guess 50, but near a hundred, probably, all around.

HAIGH: And would the machine always have been surrounded by people doing things to it?

FOX: Well, or figuring out what to do next. The input and output were always a problem. They tried a lot of different things. They tried reading in punched paper tape, and also using it as an output device Then they tried photographic output, which meant you had to develop it before you could use it again. Then they had cathode ray tubes so you could read output from the screen. I did a program that would compute the bit pattern of bits for characters so that you could put in an input value, and the characters would be printed out on the screen. (That actually may have been a little later, because the machine would have had to be working before that worked.) The problem of devising an internal memory scheme for Whirlwind was crucial. They tried photography, magnetics, and of course, a great invention, as you probably know, were the toroids, the magnetic donuts, that were invented by Jay Forrester, a very smart guy

HAIGH: Did you try and survey other computing efforts and see what they were doing with input-output devices.

FOX: There wasn't much being published then. People tried magnetic things, or cards or tape or photography or cathode ray tubes, whatever, to get stuff in and get it out. I think, now, how archaic it was, because people were involved in getting out of a lot of numerical results, which you don't want at all. But also, they would do graphs on the CRT. No numerical analysis, no programs for differential equations. There was none of that. It was just getting something that really would work. That approach probably came from ideas of simulation. You know, it was a simulator, initially.

HAIGH: That's true. If it had been intended for data processing, then probably there would have been a lot more attention to input and output at the beginning.

FOX: Yes. I think that was the reason for the bias.

HAIGH: Now, do you remember anything about any of the people that were working on the project or managing it?

FOX: Well, Forrester was the "big gun."

HAIGH: Did you have that much exposure to him personally, at that point?

FOX: At that point, not much, no. He was sweating out his troubles, I guess. Much later, perhaps too much, when I went into the DYNAMO project and worked for him. There were really a lot of very smart people at Whirlwind. I can't think of their names, it was so far back. Some of them went to the SAGE System. Charlie Adams was in charge of the kind of group I was in, which would have been computation and programming. He later formed a company, Adams Associates. He was a smart guy. Jack Gilmore [?] was another member of Charlie's group.

HAIGH: No. For the history of the project, there's a couple of books about it, so the things are fairly well known. But I just thought you might have more of a personal angle on it, just if you remember any stories about the people.

FOX: No. Because I was probably at too low of a level. One thing, some of the women really came through. First, we were putting code in with toggle switches. Then we did machine language programming. Then they got the idea of assembly language. Programmers could now write code in assembly instead of machine language. This was a huge help. We had such marvelous keypunch operators that they were able in their heads to translate that into the machine language as they punched. They would look at "Add One" and translate it into the bit pattern that would be the machine equivalent to carry that out. Later of course programs were written to do the translation automatically.

HAIGH: All right. Well, let's talk a little bit about those human issues on the machine. So as it became operational, and you could actually run programs on it, were entered by a specialized key punch staff?

FOX: Punched cards, and punched tape were the main inputs.

HAIGH: And were all the people who were doing the punching women?

FOX: Oh, yes. Well, I'm not sure. If they wanted to, the guys could punch, too. But there were women who did it mainly, one or two

HAIGH: Aside from the keypunch people, were there any other women involved with the project?

FOX: I don't remember any, really, because most people came out of EE, which was still quite male-oriented.

HAIGH: Do you know anything about what kind of background the keypunch people would have had?

FOX: Not a clue, no. They probably ran the gamut from a college degree to a high school degree.

<u>Session Two begins on the afternoon of June the 7th</u>

HAIGH: So before lunch, we had gotten as far as discussing your involvement with the Whirlwind Project. You just mentioned to me that a name had come back to you.

FOX: Oh, that was Bob Everett, a very clever guy.

HAIGH: Do you have any particular recollection of him?

FOX: Not really, but I think he worked out a lot of the timing of the instructions, and what the instruction set should be. Forrester was more the engineering person. Everett hired me. When I went for an interview, I said something about a tautology or some word that was a long word my mother always used, and he said, "Oh, quite a word!" So he hired me.

HAIGH: So how did things progress, then, when you completed your thesis and graduated with your master's degree? Did you remain at MIT after that, and immediately continue work for your Ph.D.?

FOX: Yes. So what happened next was, of course, I decided that a Master's degree wasn't enough education. So I approached the Electrical Engineering Department, about going on for a doctorate. And they debated that matter. Their verdict was, "We won't take you. (This was the old guard.) "Because you have not built radios as a boy," was one of the comments. There it was. So then I toddled over to the Math Department, because I wanted to get a doctorate. I thought I'd get further with a doctorate, which is right—you know it's good to have it. So I went over there, and they accepted me, sort of contingently. To get into the Doctorate program, you had to take a qualifying exam. So I studied hard for the qualifying exam, even though I had decided to drop out, because the match seemed hopeless. The math department was very theoretical, topology, group theory, all that sort of thing. But I thought, "At least, here I am. I've taken courses. I'll take the qualifyings, even though I probably won't get in." I had a friend there, and he didn't study for the qualifyings and didn't make it. I studied very hard and I did just fine. I got accepted. But even then I quit, because it really was the wrong place for me.

So that's when I worked doing hand computing at MIT for an astronomer called Zdenek Kopal. That's where those unexpected star publications come from on my resumé. A guy named P. A. Carrus and I worked for Zdenek Kopal. We did computing mainly, hand computations and algebraic things for him. And he was very nice and gave us our names on the papers.That was the job I had for a while, when I had given up on the doctorate.

HAIGH: So to get the chronology straight, the engineers wouldn't accept you for a Ph.D., then you were accepted for Mathematics. But then you dropped out, initially.

FOX:Yes. I was only accepted after I had taken the year off and studied for and passed the qualifying. That's right. And then they accepted me. But I dropped out because it seemed the wrong place. They were very theoretical and I was more interested in applying computers to problems.

HAIGH: Were you doing this computation work during the year out, or was that after you dropped out?

FOX: I think they overlapped a little bit.

HAIGH: Okay. But in 1954 you received your degree. How did you go back?

FOX: After I had dropped out, I got a fellowship, a Phillip Morse fellowship in machine computing that would support me for my graduate work. I don't know if Mina Reese, or ONR was the source of it. Mina, an excellent mathematician actually was a friend of my family. She also ran a good part of the Office of Naval Research during and after the war.

HAIGH: And was that an internal University fellowship?

FOX: No. I'm not sure who funded it. I think it was probably ONR, because they were trying to get people into the field of computers, which was very hard to do. I know you can't believe it, but it was, to get people like the electrical engineers and the mathematicians who were not interested in computers. Philip Morse was a famous physicist, and I guess Mina worked with him, and they decided they'd create a fellowship and that would get somebody to bridge the gap. This is speculative. I don't know. I was the only one I knew who got this.

So then that made me go back, because there it was, why not go back, right? However, in the Math department at that time, at MIT, applied math was not so prestigious as all of these other, more abstract things. There were two applied mathematicians, C.C. Lin in hydrodynamics, a very good fluid dynamicist who came out of Caltech; and Reissner in elasticity. Those were the two fields. I had to work for one of them, but neither one of them had probably ever even heard or seen a large digital computer of any sort. So I had to do what they did. You can't win in that case. It was different in electrical engineering. I just did what I did, it didn't matter. But here, you had to do what they did, and I'm not a fluid dynamicist. Anyhow, I worked with C. C. Lin.

HAIGH: And so your Doctoral dissertation, October, 1955, did it have the same title as this paper, "Perturbation Theory of Wave Propagation Based on the Method of Characteristics"?

FOX: I think the dissertation was entitled "A Perturbation Solution. A Perturbative Theory of Wave Characteristics." That's the way it ended up. It started out another way, and I worked on this, sat in the library, day after day, for a year and a half, trying to do perturbation expansions which would solve the problem: one takes a small parameter and makes expansions in terms of the parameter, and plugs it into the equations to see if you can make the expansions lead to a solution .You'd do, essentially, an infinite expansion and try to get closer. My particular problem had to do with the nose of the air foil and the air flow going around it

And it didn't work after a year, and I almost quit, and I thought, "I have to go on. I've spent two, three years at this." So then we tried a different approach, and that got us a little further along. Lin had the idea, perhaps more than I, to do a perturbation in terms of the dependent variables, hence the characteristics, rather than a perturbation in terms of the geometry Did you ever take hydrodynamics?

HAIGH: I actually did very little mathematics or natural science at university.

FOX: Yes, okay. Well, characteristic directions in fluid flow describe the flow pattern. They're a new kind of parameter. And if the direction start crossing, you've got a shock wave. So that was why the characteristics came into it. So the perturbation was then in terms of the characteristic parameters.

HAIGH: So it's a topic that people would have been particularly interested in, at that point, in conjunction with high-speed aeroplanes?

FOX: Yes. There was a guy named Unwin in England who thought he had it beat, there wasn't known to be any more buffeting once through the sound barrier. And he flew it to try it out, and he died. I always felt bad about Unwin. I never knew him, but he was sure he had solved it, and they tried it and it didn't work

On another matter, somehow, although I had been given the Morse Fellowship for Machine Computation, the work I had to do had nothing to do with computers - - all theoretical because the professors were not yet interested in computation So I had to go in an orthogonal direction into these fluid dynamic things.

HAIGH: So were you able to do anything computer-related during this period?

FOX: Not during that time. That was an awful time.

HAIGH: Can you pull back a little bit. You've said there was no interest in machine computation in the MIT mathematics department at that point. What was the place of numerical analysis, and applied mathematics in general?

FOX: There was one course in numerical computation, and a professor in the math department wrote a book on numerical analysis. I don't know if he got tenure, but it was not what you did in that department. That was not a part of the culture.

HAIGH: Hmm. That's interesting. So the mathematics department wasn't specializing in the kinds of things that would be directly useful to engineers. Is that true?

FOX: Or computers. No, and that was on purpose, because they had been a service department at MIT, historically teaching calculus and other engineering-useful courses Well, they had some very good mathematicians who didn't like that, so the department went in a more abstract research direction. But that was not in the direction of computation.

HAIGH: So they'd really swung to the other extreme to get away from doing practical applied things.

FOX: Yes, on purpose

HAIGH: Now, as you pursued your Ph.D., had your ideas for your future career come into focus or developed at all, or were you still just pursuing education with the idea of becoming useful?

FOX: Well, that was enough. There was no more education to do. I liked computers and I wanted to work with them. I think I interviewed again, a lot of places. I can't name them all, unfortunately. But I think again it was Mina Reese who found me a job, and I think probably in a certain sense, under false premises. Because I had worked with the famous C.C. Lin and I did hydrodynamics, she got me a job at the New York University Courant Institute, an Atomic Energy Comission supported department. It had a graduate mathematics department. Courant ran it, but Courant, Kurt O. Friedrichs, Levy, all these famous, really, applied mathematicians were there, and I got a job. As what, I don't know. But I wasn't really a fluid dynamicist. They had bought a computer, a Univac. Now, none of these applied mathematicians really wanted to bother with the machine, but a physicist named Bob Richtmyer who came out of AEC and Los Alamos was there. He was interested in doing computations on the Univac. That computer was very difficult to run. You had to type things on magnetic tape to get them in. It had a liquid mercury memory device. Do you remember that? Where the pulses would go through mercury--

HAIGH: A mercury delay line.

FOX: Yes. The output of the computer was also on magnetic tape, and the site at NYU had a whole secret room of these big tapes. None of the tapes could be read very well, so the trick was for the machine operator to rock the tapes back and forth until they read. So they had operators that were very good, and rocked the tapes until some values could be read. Since time on the machine was hard to get, some of us (we were the scientists, not the operators – but they had to work too) sometimes had to work all night

And then I'd walk across Washington Square park to where I had an apartment in Greenwich Village, and go home. I don't know how I survived, So there was machine computation in that sense, but not a lot of numerical analysis. I think Richtmyer at that time just tried to figure out how to compute these things.

HAIGH: All right. So let's just address a few of those things in more detail, then. So I think you'd mentioned earlier that Mina Reese was a family friend.

FOX: Yes. That was because my mother's cousin got her doctorate in Chicago, probably in 1929 or 1930, and her friend was Mina Reese who was also getting her doctorate at the University. So Mina got to know my family, and she used to come to visit them. She was a professor at Hunter, a good mathematician. In the war Mina ended up running part of the Office of Naval Research.

She went into administration, and she did a lot for mathematics at the Office of Naval Research, supported a lot of good mathematicians and projects. She knew Courant was looking around for help because he had to staff the AEC center, and there were no computer people, and so she got me this job. I worked there for a while and it was okay, and I liked all the people very much. But my job there was clearly lower level. Computers were considered not quite de rigueur and research was were it was at. But I wasn't of that caliber, probably, and so that didn't work out.

HAIGH: So you had the Ph.D. already, when you arrived there?

FOX: Yes.

HAIGH: So can you remember exactly what you were doing there?

FOX: I had to get Q Clearance. I'm wondering about this. Let's see.

HAIGH: I think, at this point all the main people involved have published books, and any secrets are pretty much revealed.

FOX: At that time, the main problem thing they were looking for was controlled thermonuclear. Now this isn't the bomb. The controlled fusion, of course, is the source of all power, if you can make it work. Fine. Theoretically it was clean, and an infinite source of power, once you got it going. And the Russians were probably working on it, so it was very secret. But of course, the technique would apply also to Teller and his bomb. I wasn't in the abstract analysis part of the research, but I helped with the computer probably, and some of the analysis of the equations involved, because I had that experience from MIT.

HAIGH: So fusion power?

FOX: Yes. If you can contain the plasma until it really ignites a thermonuclear fission then you have power. But no one could contain the plasma. The stuff keeps breaking apart. Friedrichs and a lot of people tried to develop a theory that would work.

HAIGH: All right. Would that relate directly to nuclear fission power as it was in fact developed, or was it a different proposition?

FOX: Well, it's thermonuclear, so it's an ionized thing. It isn't really like a nuclear reactor, but it's plasma which has been ionized. It's like the difference between the bomb and Teller's bomb, to some extent. I'm not an expert in this. But the very secret aspect of it was this relation to the bomb, and secondly, that the Russians were probably working on it.

HAIGH: So you had to get security clearance for that?

FOX: Oh, in depth, yes, Q. I wanted to go Europe after I got my doctorate, and the Atomic Energy Department debated if I could go. They finally said, "Yes, but you cannot go to Vienna." I thought, this reads like fiction! But they meant it! I didn't go to Vienna. Then after a while I got married (this will come up) and my husband also had to be cleared. The people in Denver kept getting called on by FBI people about my clearance.

So I worked at the AEC center of New York University on these things, and I really liked the Univac, but I was a little late. The guy who ran the Univac had a friend who dealt with the system and the operating of it and the programming of it and a lot of things. And he had precedence, so alot of ideas I had, I couldn't get through this wall of the director and his friend. That was too bad, because I had good ideas and experience in this area. That was a problem. One might say that I was always seeing a wall, but those walls existed, I think. I don't think I made them up.

HAIGH: So I think that you said that you were doing low-level analysis.

FOX: Oh, of equations. I would try a perturbation or see if one change this term, and then tried running the computer if the results eould be better.

HAIGH: I'm just trying to get the sense of how the work of the different people would come together. So there would be mathematicians who would take the problem and formulate some equations.

FOX: With the physicists, yes.

HAIGH: And at some point, once they were fairly well expressed, they would give them to you.

FOX: Yes. And other people, Richtmyer did programming, too. And I would do some, and we'd be trying to get it running on the Univac, which was very time-consuming, because there were computer and other problems. But I didn't do much of the high level analysis. Other people would figure, "What's wrong, that it isn't showing reality?" And, "Maybe if we change the equation in an appropriate way, it will help." But I didn't know much about it.

HAIGH: So people would give you equations and you would turn them into computer programs? That's a big part of the job?

FOX: Yes. And Richtmyer would, so would a lot of people. But not the theorists.

HAIGH: Yes. So how big was the programming staff for the installation, roughly?

FOX: The programming staff was very small. Most of the theoretical analysis was done by the research mathematicians. There was research on hydrodynamics appropriate to the problem, on the development of shock and such, but I would say for the programming, oh, four or five people. The site tried at some point, to get all the very good mathematicians to learn programming, and they assigned someone to teach it. They assigned a guy who was actually the lowest on the totem pole there to teach a course in Fortran, but these famous people objected like mad, because they didn't want to learn this stuff. So it was an awful pull trying to get the scientists into computers, which nowadays no one would believe. But it was true.

HAIGH: And was there a separate group of people who were in charge of operating the machine and putting tapes on and off, and so on, or did the programmers also do that?

FOX: Maybe there would be one or two people who would run the console and rock the tapes and mount the tapes, yes, you could put the tapes on the ground and roll them across to someone who would put them on. But everybody helped. It wasn't very hierarchical around the machine.

HAIGH: And would you sign up for time on the machine?

FOX: People would, yes. Right. And that's why the night hours.

HAIGH: At that point then, the system was more like with a piece of laboratory equipment: you would sign up for a time slot and then have control of it. There wasn't a system where you would submit a job and someone else would give the results back?

FOX: It got that way later, although initially it was just everybody doing it.

[Tape 2, Side A.]

HAIGH: So we've just been talking about your experiences with the Univac, at the Courant Institute. What was it like to program the Univac, at that point. When you started there, in 1954, were people still working in assembler?

FOX: I keep thinking Fortran was around then. Sometimes people used some assembly language, but mostly it was Fortran. After awhile it must have been Fortran because those people took courses in Fortran. In 1955, '56, sure. So it must have been available on the Univac, they wouldn't have taught a course, otherwise.

HAIGH: I checked the dates, and at least, according to my notes, work on Fortran began in 1954 and was released in 1957 for the IBM 704 and 709. The Univac version must have been later.

FOX: But everybody knew the programmers and Backus, so they probably got a version. And it didn't take a lot to change it for Univac, I would guess.

HAIGH: So do you remember doing any assembly programming for the Univac?

FOX: No. So it must have been Fortran.

HAIGH: Now was this the first opportunity you had to actually write programs for a functioning computer? As you left the Whirlwind project before the machine was really operational.[2]

FOX: As a revision of our interview: I think I must have worked at Whirlwind, either paid or not at one or more times in the years around 1949, because I remember writing and running various programs on the machine, probably in machine language, but perhaps later in Fortran. I have no records of this era. I worked in Charlie Adam's group. I do recall meeting Wilkes and Wheeler and Gill from England when they came over to see Whirlwind and talk with Charlie and others.

(HAIGH: So you don't have a memory of running this program?)?

[2] Although comments she made when the tape was not running suggest that Fox does not recall having programmed the functional Whirlwind, a 1979 version of her resume mentions that as well as her thesis work on the system "Later I worked full time programming the computer."

FOX: At the AEC facility we didn't teach assembly language. We taught Fortran. Of course, when I first taught Fortran elsewhere, I used to teach always some assembly language first, so they'd see what code Fortran was turning out

HAIGH: All right. Do you have any memory of whether there was any kind of library of subroutines developed in this installation?

FOX: No, that was going to come much later, people wrote their own. When I was at the Courant Institute some of us went out to Los Alamos a couple of weeks each summer, and everybody working on these matters of controlled thermonuclear gathered in there, all the big names. It was very pleasant and there would be talks about things. All those people in the AEC wanted to get together and see how they could solve these problems. Not the bomb, but controlled thermonuclear, mainly, Computer programming and software didn't come up much.

HAIGH: Do you remember anything about the state of development of the mathematical methods that were in use at this time? You know, whether any of the things you were doing here fed into your later interest in numerical functions?

FOX: No, but I'm trying to think. Peter Lax and Bob Richtmyer wrote papers on solving PDEs, numerically. They certainly were trying numerical methods, but I probably didn't follow it in detail at the time. The papers and book came later.

HAIGH: So do you remember anything about the kinds of methods that you would be using personally, or any challenges that you faced in trying to get these problems on to the Univac?

FOX: Any general methods for numerically integrating PDEs took a quite long time coming. My work at that time mainly involved getting the codes to run and obtain results.

HAIGH: Now, did your work change at all over the four years that you were there, from 1954 to 1958? Or were you doing basically the same kinds of jobs?

FOX: Well, at first I was doing more theoretical work, but then that really went to the more theoretical people, and after that, I helped with the computer. Funny, I can't remember the details, but I think my work was mostly computer oriented. I don't remember numerical methods being available in any general sense. The programs were written very specifically for the partial differential equations under study.

HAIGH: So was the work of the whole installation really just aimed on this one area you discussed, or was it doing a wider range of programs?

FOX: Well, they did a lot of fluid dynamics, and wrote papers on fluid flow, shock waves and other aspects. Courant and others wrote a book on shock waves. Kathleen Morawetz, a very good applied mathematician working at Courant, edited that book.

HAIGH: Returning to the gender issue, were there other female mathematicians and programmers?

FOX: Yes. There was a professor at NYU named Lipman Bers, a mathematician, who was very supportive of women students. I think he had two or three women doctoral candidates at the time. They got their degrees with him. So women there were coming along, mostly thanks to him.

HAIGH: And so then in 1958, you left?

FOX: Yes, well I got married. In 1958 at a party given by a woman mathematician, (what else!), I met George Sternlieb who was working at Bloomingdale's. We were very congenial. We got married one Friday noon on our lunch hour. Then we decided that since he had an MBA from the Harvard business school, he should get a doctorate there and we would become professors, "Mr. And Mrs. Chips." [chuckles] So we went back to Boston and he got accepted into the business school to study for a doctorate in business. We moved, first to Brookline, and then after a while we bought a house in Lexington, Massachusetts. We lived in Lexington for a couple of years. To support us, I got a job with Jay Forrester, because he knew me from Whirlwind, and he needed someone in the new work he was doing. He was inventing a field he called "Industrial Dynamics". A man named Richard Bennett had been working with him on a program they called "SIMPLE". SIMPLE was the beginning of DYNAMO. Bennett and Forrester had split apart, a fission, you might say, for reasons I do not know. But there Forrester was, with this partial program, and he needed to get it done, so the timing was ideal. I showed up and I wanted a job, and I took it. I had two good programmers who worked with me on it, Jack Pugh, and Grace Duren were their names. They did a very good job, and we produced DYNAMO, a totally revised verson of SIMPLE.

HAIGH: Forrester, by this point, had moved across into the School of Industrial Management?

FOX: Yes. They were pleased to get him, this famous man. That's right. And he immediately applied servo-mechanism theory to everything. Servo-mechanisms tend to have cycles. That was just what he wanted. He could match them to whatever data was at hand in whatever field. He'd have meetings with his staff every week, but most of us were not believers.

(HAIGH: I haven't heard that he's died. But I don't think he's going to read this. You should certainly speak frankly, and you can edit this if you want to.)

FOX: Yes. I did. Besides the servo-mechanism approach, he used extrapolation, which is notoriously problematic, and unstable. You know yourself that you can't extrapolate forever. It doesn't work. And so most of us were disbelievers. But my job, I felt, was to get DYNAMO working. We did. The programmers were good and we got it going. And that was enough.

By then I had heard of Marvin Minsky and John McCarthy.

HAIGH: Let's just go a bit more about this era. So I think you've mentioned about four people. So is that the size of the group that Forrester had at that point, or just was there a larger group?

FOX: Oh, no. There were a few professors who would come to these meetings, about urban matters and industrial dynamics. So I would guess, maybe three or four profs. The department gave him a very good soft place there, you know, a big office and staff, and computer time and everything. And I just had these two people, programmers, working for me. But that was enough. We got it done. I always felt a little bad about Richard Bennett, because really it was his. I don't know if Forrester credited him or not. But we changed it a lot. I mean, he just provided the initial version. It was like the versions of Excel that have gone through about 20,000 developers.

HAIGH: So SIMPLE was the original package?

FOX: The precursor, probably, because probably Forrester told him what he wanted, and Bennett tried it, and maybe whether there was a falling out, or Bennett didn't believe, I don't know. But it ended.

HAIGH: Now was DYNAMO a language, or was it more of a package that you would call from other code?

FOX: It was a stand alone program.I named it DYNAMO, because it was a DYNAmic MOdel. DYNAMO was a set of program structures that you could put differential or difference equations into, and run it and see what the output looked like, and maybe it would match the data and maybe it wouldn't, but…

HAIGH: And how would the equations be expressed?

FOX: As ordinary differential equations turned into difference equations to represent a time-dependent dynamic system. Forrester was essentially an electrical engineer, with training in servos.

HAIGH: So you've suggested that you were skeptical about the utility of all this?

FOX: Yes, even from the beginning. I thought the results were due more to the equation use rather than the actual phenomena. Perhaps I didn't even think about it much. I thought, "Here's a job. I'll take it. I'll get it going." But as I looked into it, I wasn't so sure that that was going to solve the world's problems.

HAIGH: Can you remember what it was that DYNAMO did, that SIMPLE didn't?

FOX: No, I think SIMPLE was quite inchoate, if I can use the word. When I first got there, parts of it existed, but we had to rewrite it, essentially. I don't know which of the ideas were due to Bennett or to Forrester.

HAIGH: So you don't think SIMPLE ever really worked?

FOX: No, I don't think so. I'm not sure, you see. I don't know, because we didn't bother with it. We did ours.

HAIGH: I came across references to both DYNAMO and SIMPLE, on the Web.

FOX: Did you? I wondered where you got that.

HAIGH: And with your name attached to it.

FOX: Okay, good enough.

HAIGH: It's existence is documented, but I'm not sure how much people know about what was actually in DYNAMO. Now, even if you don't think the system addressed these kinds of grand problems it was supposed to, was there was anything interesting or personally rewarding, involved in producing it, maybe in terms of any of the programming techniques you used?

FOX: You mean the structure of it?

HAIGH: Yes. Anything that contributed to your later interests, for example?

FOX: Uh, no. I wouldn't say so. When you're in the middle of a project like that and you've got to get the people to do it and get it working, the deeper levels of it kind of vanish. So maybe that was just as well. I didn't delve into the theory of it. I just needed to get the whole structure working and turn out a program that would give answers.

HAIGH: Would you say that you were in charge of the Dynamo development?

FOX: Yes, sure. Bennett was, and then I was. One of my programmers, Gay Duren whose degree actually was in French, was an excellent programmer. Also Jack Pugh did programming for it, and he was very good.

HAIGH: Did your lack of faith in the project complicate your relationship with Forrester?

FOX: I didn't really bring it up. Some people tried, in those meetings, but it was a hard wall to get through. So, no, I just went along with it. And I was sort of quiet. Other people were too. And then I left. I hadn't wanted to leave before it was done. I got a job in the Computation Center at MIT where Marvin Minsky and John McCarthy were holding forth, working on artificial intelligence. I felt very lucky to be there

HAIGH: And that was in 1960. Now, can you just start talking about that place, describing in general terms, the computation center at that point, and the other people who were involved, and the mood of the place?

FOX: Yes, I'm trying to remember what they had at that point, in the way of computers. I can't remember. Probably 701, 702?

HAIGH: I think they had a 704 by then.

FOX: They had a 704 by then? Okay, I can believe it. It was run as a closed job: there was a God of the machine and a staff to run the machine. You would put in your cards and wait overnight, and then you'd get the results back.

HAIGH: So that was a difference from your experience with the Univac, earlier?

FOX: Oh, yes. Sure. Now, for Minsky and McCarthy, this pattern for running the 704 was no good. They had to go to PDPs. Incidentally SpaceWar was written by guys working on PDPs there. Do you remember SpaceWar?

HAIGH: Oh, yes. It was quite famous.

FOX: Yes, okay. So the trick was to keep all their group from playing Space War so they would work on the matters at hand. But at least they had their PDPs. Now, the version of the DEC PDP at that point, would have been a PDP six, perhaps? Or one? Maybe a PDP one.

HAIGH: I think that would have been very early in DEC's history, so a PDP-1.

FOX: These machines were much better for the group.When you're doing artificial intelligence, you don't want to wait for overnight for a run. You said this was discussed in the History of Programming Languages volume?

HAIGH: McCarthy gave a paper discussing the origins of LISP and the Computation Center at that point, which your name is mentioned in. So I got the general background from that. I'm just looking through my notes for a date on Space War. Oh yes, here we are. The so-called Technical Model Railway Club around about 1959 got access to a TX-0 computer that was no longer needed. I think it had been a test system on the larger project, and that was where they could experiment with these interruptive computing techniques. Then they got the very first PDP-1 in 1961.

FOX: That's right. Because those people at MIT came out of the Railroading Club. All those early-- yes, [chuckles] I know. They were wonderful. Okay. That's true.

HAIGH: Including a Steve Russell, I think, and--

FOX: Oh, Steve Russell, very smart, very smart, very straight-headed. They never could get him to get his degree, but he was wonderful, yes. Ah, but he wouldn't go to school.

HAIGH: So at the time that work on this was begun on LISP, what the AI people had was a 704. Oh, here we are. I'm just looking at the page on SpaceWar. So in 1962, Space War was finished, on the PDP 1.

I'm not sure if anything LISP related ever happened on this TX-0 or not, but as you know it's famous as one of the first computers that people were just were able to sit around with and play with interactively.

FOX:. Yes, and that got them going, I think. Now have you ever seen a page of LISP?

HAIGH: I've done a little LISP programming.

FOX: Oh, you have?

HAIGH: I had a master's AI course with it. I had to write a program to play the game Othello. I must admit, I preferred Prolog. The LISP parentheses were too much for me.

FOX: Because one's parenthesis keys wore out—they didn't work after awhile. LISP programs are loaded with parentheses. You've done it. I'm impressed. That's not easy writing.

HAIGH: So you say that you wrote the first LISP manual?

FOX:. Now, this was not because I was a great LISP programmer, but they never documented or wrote down anything, especially McCarthy. Nobody in that group ever wrote down anything. McCarthy was furious that they didn't document the code, but he wouldn't do it, either. So I learned enough LISP that I could write it and ask them questions and write some more. One of the people in the group was a student named Jim Slagel, who was blind. He learned LISP sort of from me, because I would read him what I had written and he would tell me about LISP and I would write some more. His mind was incredible. He could give lectures. Have you ever seen a blind person lecture?

HAIGH: No.

FOX: They write on a black (or white) board, and then they put a finger on the board at the point they have stopped to keep the place. Then they talk some more and then they go on writing. His mind was remarkable. He was very helpful to me. But I wrote those manuals. I would ask questions from Minsky or McCarthy, and I got it done. I think it was helpful for people to have it. I guess, essentially I'm a documenter. If you're looking for it, that's what I am.

HAIGH: So that by point, they had a functioning LISP system?

FOX: Yes, yes.

HAIGH: But they had nothing written down about how to use it?

FOX: People learned how to write it somehow. Perhaps (as I encountered later at Bell Labs when the language C was being developed) one just studied a written LISP program and tried to figure it out.

HAIGH: And what were your impressions of McCarthy?

FOX: Oh, that's an interesting guy. He was very, very, smart. So is Minsky but in a different way. There is an interesting collection of papers, entitled "Automata Studies (Annals of Mathematics Studies, Number 34)" published by Princeton University Press in 1956, edited by Claude Shannon and John McCarthy, containing a paper by McCarthy entitled, "The Inversion of Functions Defined by Turing Machines." Marvin Minsky has a paper on Finite Automata in that publication.

HAIGH: I'm looking now at a copy of the LISP manual, and this one is dated March 1st.

FOX: That's not the actual manual, though. It's up here. You don't have a copy of it.

HAIGH: I'm looking at a document that says LISP One Programmers' Manual on the front. And that one is dated March 1, 1960, which is actually a year earlier than the one shown on the resume. And this could be interesting because it has a number of penciled in corrections.

FOX: Oh, "Mainly written by Fox, on the basis of information from McCarthy and the other authors." That's right. I guess, I remembered it differently, but this may be it. So anyhow, that was fun. I liked LISP.

HAIGH: And do you remember anything about, your reactions to LISP? It must have been quite a change of pace from Fortran.

FOX: Oh, yes. And I didn't get good at it. But I thought it was a marvelous sort of language, except that it was somehow impractical. You can't ask people to do that; you need a translator. Such programs for LISP may exist now. I don't know. A program you could give simple statements to that would be automatically be translated into the multiply-parenthesized code.

HAIGH: As I understand the history, they had actually originally planned to have something like that, and use all the parentheses just for the internal representation. But then people just got into the habit of writing it like that, and they never got around to [chuckles]--

FOX: I believe it.

HAIGH: Do you remember anything about the strengths and weaknesses of the first LISP implementation?

FOX: A daily changing item, how can you ask? [Chuckles] But they were all phenomenal programmers, so they all had it in their minds. I don't know if it's stabilized yet. I suppose no one uses it now.

HAIGH: There have been several standards for it. I think it's still in use in some areas. I don't think it's used as much as a general-purpose language. Now looking at your resume, I also see two publications before that. One with Anthony Ralston in *The Journal of Mathematics and Physics* in 1958. So I presume that was coming out of your work at the AEC Computation Center.

FOX: No, I think that article was not related to the AEC work. How did I know Tony? I guess from MIT. He got his doctorate probably at MIT, so I must have known him there. He wrote a book too, and we wrote a couple of articles. Also I think something I wrote was included in an encyclopedia that he put out at some point.

HAIGH: Oh, that was later.

FOX: Was it later? Okay. He was at Buffalo at some point. Yes.

HAIGH: Now, you left the Computation Center in 1962.

FOX: Yes, Minsky and McCarthy were still there. You know, Minsky had done the right thing with his education. To get his doctorate, he realized you couldn't do a dissertation in his field of interest at MIT, so he went to Harvard and did his own unique dissertation. I didn't have the guts to do something like that. That takes a lot to just throw up everything and go somewhere else. Sometimes students could tell Harvard what they wanted to do and Harvard would let them do it.

At MIT McCarthy was in charge of a beginning computer course, course 6.41. Sometimes when he was away I would teach the course for him.

HAIGH: So you taught at MIT? Do you know what year that would have been?

FOX: Probably 1960, ’61.

HAIGH: Why did you leave MIT?

FOX: Because my husband, George, got his doctorate. So then he had to get a job. He got two offers: one from Berkeley as assistant professor, and one was at Rutgers, the State University of New Jersey in Newark, as associate professor.

HAIGH: What field were those jobs in?

FOX: George’s doctorate was from Harvard Business School, and his field, I guess, was retailing, essentially. He explained to his advisor at the Business School that the downtown department store was dead. That was not popular with his advisor, who, of course, consulted for these places. But he was right. George took the associate professorship and our green fields campus where we had thought of teaching turned out to be Newark, New Jersey. Do you know Newark?

HAIGH: Yes.

FOX: Right. But I think it worked out very well. So we came to New Jersey.

HAIGH: You got a job at the Newark College of Engineering.

FOX: Yes. First of all, I thought I would mention one more thing about MIT. When I was there, we had a couple of babies. I always kept working. To help we had 13 au pair girls from Finland, one after the other. They’re all friends and they’re all like daughters to me. Anyhow, so I had two sons. One was born in 1960 and the other in ’62. When I was pregnant with the second one, McCarthy and others had a project. MIT was planning to get a new computer, and the Artificial Intelligence group wanted a computer that would be able to running problems in that field.The salesmen at IBM were interested in getting the MIT contract of course, but none of us thought the IBM machine would be appropriate for our work at the Computation Center. The CDC company at that time was making a 6600 machine that seemed to us would be appropriate in a number of ways for our work. But IBM kept up the pressure. So I was assigned to do research on this matter and go to a lot of labs and see what they wanted to do, what kind of research they did and what kind of computer would help them. At this point I was getting more and more pregnant. I’d be sitting on a high stool in somebody’s lab, finding out about their work, and they would be a little nervous. (I worked up until I had a baby.) But in our investigation of computing needs we found out that there was no question that it had to be a CDC or equivalent to do the work most people wanted to do. So MIT got an IBM computer. You couldn’t beat the IBM push. I don’t know what the deal was; it all went on at a level far above us.

HAIGH: Can you remember what model that would have been?

FOX: They had a 704. So it must’ve been the next one. 709? But I don’t remember.

HAIGH: So then after the move down to New Jersey?

FOX: Before I went to the College of Engineering, there was a project I got funding for. I think it was supported by the Ford Foundation. I had little babies at that point. We kept moving; we had three houses in one year. It was a little difficult to work, but I could do the work for which I'd gotten funding: to teach women who had majored in mathematics, computer programming and other things, but had stopped working, about current mathematics and computers so they could go back to teaching in high schools. There was a big push on education at that time. And I taught a course to these women. They were smart, all of them. They took to programming in a marvelous way. Do you think any of them became high school teachers? They all went out and got wonderful programming jobs.

HAIGH: So that was between leaving MIT and coming to the New Jersey College of Engineering?

FOX: Yes, but I wanted a fulltime job, so I just went out and got a job at Newark College of Engineering, now New Jersey Institute of Technology. NCE, now NJIT. A job in the math department.

HAIGH: What was the college like at the time you joined it?

FOX: The teachers worked so hard. It was a commuting college mainly. Now they have more dorms. The students were mostly men. They did let in women, and the few women students that were there were very, very good. Many of the students worked full-time jobs, getting bachelor's degrees at night, which took them eight years. Hardworking people. Not a great education, but enough to do all right. Like many of these schools, I guess, in the world.

HAIGH: Did you have a heavy teaching load?

FOX: Quite, but it got ameliorated by the fact that I got a job as assistant or associate director of the computing center which had an IBM 1620, a good machine. The center was technically run by a professor in the Civil Engineering Department, but I ran it. I had a secretary, which was the nuts. I had an office and a nice secretary, and it worked very well. There were some very smart students in the computing center who spent all their time on the machine, and wouldn't go to classes. They found the computer addictive.

Later, when I went to Bell Labs, one of my students from NCE came by my office to say he didn't like the job he'd gotten at Bell Labs. Can I do something about that? But when he saw that Bell had put me in with another person in a small office, when he remembered me with a secretary and an office at Newark College of Engineering, he turned around and walked away. Still, Bell was fine.

HAIGH: So how large would the total staff have been for the computing center?

FOX: A staff?

HAIGH: You mentioned there were some students there. You had a secretary. You had a computer. Presumably you had some computer operators.

FOX: No, the students ran it. They just did it. They loved it. Did it for free. For example, those students looked at what was in the memory of the 1620 and figured out how the operating system worked. Smart. They were that involved in it, and they were very good. One of them went to work for the CIA. But it was hard to get them to go to classes so they could graduate. Also they were not numerical analysis people.

HAIGH: How was the transition from a research associate to someone with a research program of your own? (On your résumé you list your title at the computing center at MIT and at the School of Industrial Management as Research Associate.)

FOX: Oh, I see. Yes. And here I list it as Assistant Professor in the Math Department.

HAIGH: Right. And one of the differences of that, presumably, is instead of doing what Forrester tells you, it's your responsibility to make your own research program and figure out what you should be studying.

FOX: That's right. Let's see, what did I do? Well, there was a summer thing that is discussed later. Anyhow, at one point, I tried to get a grant because I had an idea to do some research. Here was this idea; it was on car-following.

HAIGH: Oh, yes, I see. It looks like in 1967, there are two publications in that area. There's "Digital Computer Simulation of Automobile Traffic" published in *Traffic Quarterly*, volume 21, number 1, January 1967. And then there's "Safety in Car Following: A Computer Simulation," Newark College of Engineering, 1967, which says it's a monograph covering research carried out under a particular grant from 1965 to '67.

FOX: Here's the idea I had: I knew about certain psychological things of reaction times for humanity. There are factors and there are certain equations that deal with reaction time. It seemed to me the equations might have an application to the development of shock waves which can occur in car-following (hydrodynamics again). Shockwaves appear in traffic as you may have seen on the highway or in the Lincoln Tunnel or wherever. So I worked up the equations of car following based on speeds and spaces between cars and reaction times and God knows what. This was the rough idea I presented first to the Russell Sage Foundation. I didn't get a grant from them, but later we got support from the U.S. Department of Health, Education, and Welfare, HEW. I did this and I worked out the programs. I got it to work on the computer, and it gave very good results—very well in line with actuality. So I was pleased with that. I guess HEW was too.

FOX: So I used part of the time on that research, and also I taught. In between times, since I was interested in numerical analysis, I read in numerical analysis and I put together various programs that were good for computing numerical things. So I began to do it then, at NCE.

At that point I found out from somebody that Bell Labs sometimes gave summer jobs to faculty people. I guess they don't do that anymore, but they did then. I went over to Bell in Murray Hill, NJ and interviewed various people. Joe Traub was one and he seemed, to me, lively. So I said, "I'd like to work with Traub." Traub, in turn, had found a wonderful numerical method for solving differential equations using extrapolation to make it stable. He thought that would be good to work on and get it working there, test it and everything. So it was the ideal summer job for me. That's when DESUB came into it. This work is discussed further a little later.)

[Tape 2, Side B.]

Session Two continues on afternoon of June 7, 2005

HAIGH: So we've just been discussing your period as a professor in the Newark College of Engineering. You said there were a few other things that you just thought.

FOX: I was just going to say that though it wasn't all so great at NCE, yet, since I didn't really know computer science and such—simulation and various languages, I found a way of using my teaching to learn many new subjects. So I used the graduate courses I taught at night to learn all those things, all the subjects that you have in a computer science department. It was very helpful to me. Hard on the students, but the best way to learn something is to teach it, so I taught a lot of things. I did teach numerical analysis, but also simulation and languages, for example SNOBOL, SIMULA, and all the marvelous languages that were coming out at that time. I knew many of the people working in those fields and I could get early versions from them.

Now, one other thing I wanted to mention was, as you see from this document you have, and you can call it something if you want on the tape …

HAIGH: Actually, it's dated October 1966 and it's titled, "Rough Draft for a Proposal for a Department of Computer Science at New College of Engineering."

FOX: So that got written by me, and it has it all, even the staffing and everything else in it. So I did that and submitted it, probably unsolicited, to the administraton. But I don't think they ever even acknowledged it. They weren't really interested in that. Much later, Hazell, who was then president, called me in and he told me, "We have decided to start a department of computer science." He said, "You know some people in this field. Can you find someone to head it for us?" Yes! So that does make one want to leave. It was a very local and not terribly interesting school at that point. They irritated me in many ways, for example once they had invited Grace Hopper, who was a friend of mine, to give a talk and they got her to visit there, but they never let me see her. Ah, people! That didn't matter. She would've understood. Phooey. That was enough.

HAIGH: There was never a department of computer science during your time there?

FOX: No.

HAIGH: Was there a program or a specialization that students could get a degree in computing?

FOX: No, no. Later it happened later of course. Having deciding that, Hazell found someone to head the Computer Science Department, somebody from RCA. Later, after I had left, someone in the computing center found all these numerical programs for doing all sorts of computations—differential equations, linear equations, roots, and everything. Whole bunch of programs I had written to start a software library. They were there, and we could use them for computing on the 1620. The person who found them asked "Whoever did this?" He asked someone and it got back to me. So there was an initial impulse there—a seed, sort of.

HAIGH: You mentioned that you had been teaching graduate courses in a number of areas of computer science. Were the students who were taking those doing a mathematics degree and just focusing on computer science? Was there an official specialization?

FOX: No. No, I think they were engineers, all of them, and they probably got a master's in engineering, perhaps in electrical. I don't know.

HAIGH: All right. So you were in the mathematics department. Most of those students would have been coming from engineering.

FOX: Yes, I think so. There wasn't a math major. It's quite different now, of course. They have doctorates and whatever.

HAIGH: You mentioned your work on car following as a research project. But in 1965, I'm also seeing on your résumé paper called "Rational Approximation on Finite Point Sets." You presented it to a symposium on the approximation of functions at General Motors Research Laboratories in Warren.

FOX: I don't remember that.

HAIGH: Was that perhaps connected with the General Motors summer schools in numerical analysis?

FOX: Oh, no, I know where that was. At MIT, The artificial intelligence group had found me an office which I shared with Al Goldstein, a mathematician, a very good one. He and I wrote a paper, which I think I didn't contribute to much at all, but he felt that I should share. He was a generous guy. It was fun to share an office with him because at that point at MIT, I was pregnant out to here and he was a weight lifter and in our office were all these weights lying about, these big things, He was often out of the office so when people would look in, there was this very pregnant woman with all these weights all over the floor. It was diverting.

HAIGH: So that wasn't something you were actively researching.

FOX: No. I understood it, I did a little, but I didn't help out on that very much.

HAIGH: You mentioned that you knew Grace Hopper. Where did that relationship come from?

FOX: At every computer meeting, there she was, Grace, the other woman and me. She was a little shy and diffident. Hard to get through to, but very purposeful. She always had a group of acolytes that were with her. She did COBOL. I think she did a lot of very good stuff.

HAIGH: Would you say that the small number of women who were involved in computing tended to know each other?

FOX: Oh, of course. Frances Holberton at Naval Research.

HAIGH: Was she involved with ENIAC or Univac in some way? Anyway, the name is certainly familiar to me.

FOX: Grace was at Harvard initially. I don't know where Francis was, but sure, we knew one another.

HAIGH: You had mentioned that in 1967 you became a summer consultant to Bell Labs, and you met Joe Traub, and that out of that the DESUB routine developed. Perhaps you could begin by describing Joe Traub.

FOX: [chuckles] Well, you've got him on tape. He wasn't very involved in our DESUB research once he he had found us the method. He was pleased to have someone doing the work. Ideal boss, there.

Later addition: I have now found some of my notes on the origins of the DESUB work: Morven Gentleman and Joe Traub had been considering the development of a library of programs for numerical computation at Bell Laboratories. They called the project, "Numerical Mathematics Program Library Project." It was to contain the best programs in many areas. The programs would be well-tested and carefully documented. Traub knew about a very good program for solving numerically ordinary differential equations. It was an extrapolation method developed by R. Bulirsch and J. Stoer in Germany, and was described, together with an Algol program, in Numerische Mathematik, Vol. 8, 1966, pp. 1-13. A FORTRAN adaptation of the program was made by Nancy Clark at Argonne National Laboratories. We obtained a copy of her program either from her or from Joe.

So I worked on that one. I had a programmer, even. Penelope Payne-Crockett was her name. She was very, very good. I did some planning and analysis, and she did most of the programming, but we worked together very well and we got an article or two out of it. The program ended up much later in the Port numerical library at Bell. It's a good program. So there was DESUB

HAIGH: Was DESUB your first published piece of software?

FOX: I guess so, yes.

HAIGH: You mentioned that you had previously developed a number of routines for internal use.

FOX: Oh, these ones at NCE. They were just left at the college.

HAIGH: Do you remember anything about working on a piece of software that was intended for publication, and eventually incorporated in a library, that was harder, or required a different kind of approach, from just developing these routines for internal use in a small computer center?

FOX: I didn't think of that at the time.

HAIGH: For example, testing. Do you remember anything about how the code was tested?

FOX: No, there wasn't anybody else at NCE interested in numerical programming The students around the computer were interested in the operating system or whatever, and I don't remember any faculty people who were interested. So I did whatever programming and testing that was done. Just to have it to have a library there. Very local activity.

As for the work at Bell, we did quite extensive testing of DESUB on a variety of problems. The routine was installed on the GE 635 computer that Bell had at the time. The routine could be called from either FORTRAN or ALGOL programs. A paper describing the work appeared in

John Rice's *Mathematical Software* proceedings. [Fox, Phyllis. "DESUB: Integration of a First-Order System of Ordinary Differential Equations. In *Mathematical Software* , John Rice, Ed., Academic Press, 1971].

HAIGH: Now, as a result of this, you presumably attended the mathematical software conference to present the paper.

FOX: I guess so, yes..

HAIGH: Yes. And that's remembered by many people in the field, including John Rice himself, as an important meeting that really led to a number of people involved in mathematical software coming together for the first time.

FOX: Yes, it was nice.

HAIGH: Do you remember anything about coming into contact with other people with interest in mathematical software and feeling that there was a community of people?

FOX: Oh, I certainly knew them by then because there was the SIGNUM and all the publications. I think it was in the air, and I got to know everybody and what they were doing.

HAIGH: Do you remember when you would've first come across SIGNUM?

FOX: Didn't Traub start that journal? Was that a journal or a group? I can't remember.

HAIGH: It was a special interest group, but I think the main thing it did for most of its existence was to publish a newsletter. The newsletter, I think, would have started around this time, actually, around 1966 or '67.

FOX: Yes. Probably by that time I would have known many people in the field, even from Los Alamos and Courant and MIT. You get to know all the people that are working in it.

HAIGH: So you think that you have met most of those people during the late 1960s?

FOX: Probably.

HAIGH: Were there any people within the mathematical software community that you had a particularly close relationship with?

FOX: To work with, you mean?

HAIGH: Yes. Either that you became friends socially or that you collaborated with academically.

FOX: Or competed with.

HAIGH: Yes.

FOX: Sure. Library people were always that way because you're all bristle. Here comes Argonne moving into our territory, and then there's this IMSL. So I knew these guys especially well. But we all had our own turf and we put out libraries. We never thought of combining together; that wouldn't have made sense. But things like LINPACK or EISPACK that were being done separately didn't come into it because of being supported by some government agency, I don't remember which one. They were excellent libraries and a great help to many people.

HAIGH: So you viewed the commercial libraries as competitors, but not the public domain efforts.

FOX: Yes.

HAIGH: I have a letter here dated March 17, 1967, offering you the summer consulting position. It says that you would be a member of technical staff associated with our mathematical physics department at our Murray Hill Laboratory.

FOX: Oh, I think that was just for the summer initially for DESUB, but the further testing of the program and documentation went on into the winter -- on a consulting basis, because I was still a professor at NCE.

HAIGH: By the way, are you aware of DESUB being used in any particular places or whether it was incorporated later into any libraries other than PORT? Was it code that people took up and did things to.

FOX: I don't know. They could have; it was all published in the open domain. I haven't checked into the matter.

HAIGH: So in 1967, you did the summer work, you consulted a little bit, then according to your résumé, you received tenure in 1972, and then the next year in 1973 you quit the college and went to work at Bell Labs.

FOX: Right.

HAIGH: You've hinted, I think, at some reasons earlier. But why was it that you quit at that point?

FOX: The reasons I've given were sufficient motivation, but I didn't want to quit until I had another job. I had been trying to get a job at Bell and it was taking quite awhile, but I certainly had had enough of NCE. Fortunately my husband was, by then, a full Professor at Rutgers University in New Brunswick, and was running a Center for Urban Policy Research that he had developed, so we had money coming in. If I had been the sole support of the family, of course, I could never have left.

The uncertainty about me at Bell, i.e. the reason it took so long to get a decision probably had to do with the mystery of my leaving a tenured position. It seemed strange to people. Also it may have been unusual at Bell to hire someone who was 50. The process was a little iffy. It took quite a while.

HAIGH: So you quit the college before you were offered the job at Bell.

FOX: Yes.

HAIGH: I'm just seeing a couple of documents related to that. Here's a letter. It's not dated.

FOX: Is that from George Baldwin or somebody?

HAIGH: This is from you to W. Stanley Brown of Bell Labs. It says, "One of the things I am interested in is numerical computation with emphasis on the development of computer program libraries. I developed a sort of mini-IMSL library at Newark College of Engineering. I also like programming and computer languages. Another area of interest for me is simulation, which, for my purposes, means the use of computers in representing dynamic behavior of a postulated model of a system."

So I guess this is you negotiating with Bell Lab about what you might do there

FOX: Yes. Fortunately for me, I think they were desperate because if they wanted to do this library, they had to have someone to do it, and we were scarce on the globe at that point.

HAIGH: Right. By 1973, I assume both Traub and Morven Gentleman had moved on from Bell.

FOX: I don't recall, but I didn't see them around.

HAIGH: Do you think, in a sense that was what created the need for you to be there?

FOX: That's how I got the job, probably. I had to give various talks during the interviewing process, and the only thing besides program libraries I had worked deeply in was car following, so I gave a talk on that. To all these researchers, Aho and these people, they all came to my talk. I tell you, that was frightening. But I didn't talk on libraries; I talked on car following and the motivation and how I finished that, because it seemed wiser to do it that way. I finished projects, you see. That helped, perhaps.

HAIGH: You just suggested that even before you arrived there, Bell had a commitment to doing a library.

FOX: I think it had died when Gentleman and Traub left. Bell Labs had a good statistics section that was quite famous. John Chambers there, who has done a lot of statistical work and written a language called, I believe,"S," is very well known. The numerical analysts were sort of scattered around and they weren't good documenters, so there was a need, really, to bring the numerical library all together.

HAIGH: So you think there was officially a program, but in practice it lost all its momentum.

FOX: Yes. The way things worked, When I first went to Bell, perhaps during that summer consulting, individual computer routines were available as decks of punch cards. If you wanted a program to find roots of an equation, you went to the computer library to get the deck of cards. There was emphasis on research, and physicists were the ones who ruled the roost. They had programmers to do this other grungy stuff.

HAIGH: So there was a need to keep an up-to-date library of internal purposes.

FOX: It seemed to me that, yes, the punch card overnight thing wasn't going to work.

HAIGH: Do you remember what your official job title was?

FOX: "Member of technical staff." That's a standard Bell phrase.

HAIGH: Organizationally, were you directly associated with a centralized computing center?

FOX: Well, I wasn't in research, unlike most of the people I worked with. So I was at the computing center, that's right.

HAIGH: What kind of computing center did Bell have at that point? Was it a large, centralized facility?

FOX: It was centralized. There were PDPs around in the physics department and elsewhere, but there was perhaps a push to concentrate things around the 7090 or whatever they had then. The 360? Something big.

HAIGH: This is 1973. But at that point it was probably a 370.

FOX: 360 or 370. Yes, whatever they had, they always had the big IBM computer. That's what it would've been.

HAIGH: Do you have an idea of how big the overall computer center staff was?

FOX: No, but have you ever seen a Bell Labs telephone book? It's a most instructive thing to look at. The departments with the most prestige, probably physics, had departments numbered starting with a 1. In the phone book they appear all the way to the left, with members of the department below set out in varying layers to the right with numbers starting with 11.. or 12… etc. Other departments start with higher numbers. Sometimes I would get a call from a physicist wanting immediate attention. He would state,"I am so-and-so in one, one, one," and that meant this is God. One. Research. But then you get down to computing technology and people. And then you get into the lower levels. But everything is indented appropriately. No doubt other large companies are similar. I was in Department 45231, Computing Technology, and there were perhaps 5 or 6 or more "Supervisors" under the Computing Technology Department head at Murray Hill, and each supervisor would have 4 to 7 people in their group. These are not exact figures. There also were big computer centers at the various branches of Bell labs such as Holmdel and Chicago.

HAIGH: It's very hierarchical.

FOX: Yes.

HAIGH: Does that give you an idea for how big the computing center was?

FOX: Oh, that would probably be 45. Well, let's see if we can see it in a hurry here. 45 is probably about right for Murray Hill. I don't know.

HAIGH: So you're there from 1973 to 1984, and your résumé shows that from 1981 to 1983 you were supervisor of something called the User Interface Group. What was the User Interface Group?

FOX: We would help people. By then, maybe we had a Cray computer, so there was a lot of dealing with libraries and the machines. The scientists would have their programmers who would come to us with something that didn't work, and we'd have to deal with them from a programming and numerical point of view, to solve the problems. Like they do in India these days, you know.

HAIGH: So that's user interface in the sense that you're the group that interfaces with the end users, not in the sense that you're designing a user interface as we think of it today.

FOX: That's right. We were the ham in the sandwich. For example, if you've got a Cray computer, you have to give a lot of talks and lessons to people on how to code so the program will vectorize. Do you know how the Cray works? It vectorizes. If your program deals with vectors with many components, the Cray is able to automatically sequence the computation through the components, giving rise to a very fast computation. So we had to tell people how to write FORTRAN that would take into account vectorization. That would have been one kind of project. We'd learn how to do that and teach people about that.

HAIGH: Now, through the rest of your time there, did you have people who were directly reporting to you?

FOX: Before I was a supervisor? No, not really. I was mainly working on the development of the PORT library which we'll discuss later.

HAIGH: So you were more coordinating library things than having a team of dedicated programmers working for you.

FOX: Yes.

HAIGH: I'm thinking we should probably leave discussion of the PORT project itself for tomorrow. I think we've covered your arrival at Bell labs. I'm just wondering if you have anything else to say about the kind of projects that the computer center would be working on, and perhaps also about the relationship with the end users internally? You've suggested that the physicists can be quite aloof, for example.

FOX: That one was an isolated example. Most people were very nice and grateful for help. The computation center had very good programmers to advise people, and also in my group there were two excellent physicists with doctorates that the computer center didn't exactly know what to do with: Wes Peterson and Howard Okrent . They were both very, very good. The physicists and scientists in other departments found these guys and they came to work with them. I don't know if the computing center especially liked that, that these people were working with the physicists, but it worked out very well. All this was not advertised specially, but it worked well. Other people in my group worked with the Cray and were teaching things about it, and other people answered questions, and you haven't even gotten into the Unix crowd.

HAIGH: Yes, and that's really what computing at Bell Labs is famous for now.

FOX: Yes. The Unix group were in research and worked on PDP and VAX machines up in the attic. Doug McIlroy, Brian Kernighan, Dennis Ritchie, Ken Thompson, Mike Lesk and others, were creating Unix and the C language and all the rest of it. They would come in about 2:00 in the afternoon and have lunch and then work until 2:00 a.m. or 3:00 a.m.

HAIGH: Now, did they have a high profile within the lab, or did most people not know they existed?

FOX: No, I suppose not a lot of people knew initially. They were like my students at NCE (though at rather a different level!) addicted to the machine, and finding wonderful things to do with it.The language C and UNIX itself soon became famous, at the Labs and elsewhere, as you know.

HAIGH: Now, did the computer center have any programmers who could actually do the scientific application programming for people, or was the assistance just limited to training courses and…?

FOX: No, people like the physicists hired women to do the programming, initially, and after a while the smart scientists learned how to program and they did it themselves.Of course that works much better.

HAIGH: So there was no application development done by the computing center staff?

FOX: Not really, no, strangely. We gave them the PORT libraries and set up other libraries for them on the computer. That was, I think, very successful. It worked very well.

HAIGH: Well, I think that might be an appropriate time to stop the tape, then. This is the end of session two, and we will finish here for now.

[Tape 3, Side A]

Beginning of Session 3, held on the morning of Wednesday, June 8th, 2005.

HAIGH: Now, I think we talked in general terms about your transition to Bell Labs. What we haven't talked about at all, other than in a brief mention of your 1967 summer project which produced DESUB, is the state of libraries at Bell Labs and the PORT project. So before we start talking about what you did with PORT, maybe you could give a general sense of what kind of library facilities were available at Bell when you arrived.

FOX: When I first went to Bell on that summer consulting job there really weren't program libraries available. If someone wanted a program to do something, say, find roots of equation, they would go to a room where they could be given a deck of punch cards which they could incorporate into a deck containing cards for their program to be submitted for running on the big machine. When I was hired there much later the situation was of course different.

HAIGH: At the time you arrived, if they were with IBM, I think it probably would have been some kind of 370.

HAIGH: And I know a Cray 1 was installed while you were there.

FOX: Yes, a Cray-1 computer was installed at Bell Labs December 7, 1979. It cost $92 million.

HAIGH: And I think also some small PDP machines and probably some VAXes, I would guess.

FOX: Yes, there were increasing numbers towards the end of my stay; there were PDPs in the various departments of physics; people liked to have their own machine so they could run things, and at that point you could get PORT up on these, so there was a certain move to get away from the main machine and its ministers and go back to their own machine in their lab.

HAIGH: So at the time you arrived, before the PORT effort got underway, do you know if any of the routines that you've mentioned being stored on the punch cards would have come from outside the lab, or were they all produced internally?

FOX: I wouldn't know for sure, but I would guess internally, mainly by scientists in research. Bell Labs was very internally structured, which had advantages and disadvantages. Since the programs were written by people there and got a lot of use by people there, it was a good way of monitoring them and debugging them. Bell was more that way when I first went there than when I left. People didn't go to meetings so much and it was a very self-contained situation.

HAIGH: So you think that was a feature of the culture at the laboratory that in general people were quite inward-looking?

FOX: Oh yes, that's right, and there were certain problems if you wanted to write a paper and get it published somewhere, it took phalanxes of lawyers to get it through and it took a long time to get permission to get the paper out, so it was very self-contained.

HAIGH: Now, do you remember the origins of the PORT project, where the impetus came from, perhaps who you had to convince it would be a good idea?

FOX: As we have discussed, the Traub/Gentleman thing had started in that direction, and also I had done it on my own at New Jersey College of Engineering or NJIT. I think I was hired with the idea of continuing the project, because we wanted some way of getting a good library of good routines for everybody.

HAIGH: As well as the earlier internal project you mentioned, I know that by 1973 IBM's SSP had been around for about five years. Do you have any experience with that library?

FOX: No, not at all. I know of its existence, but I'm not sure that we used it. Perhaps people did, but I didn't have experience with that.

HAIGH: Yes. I'm trying to remember the exact dates. It may have been around the time that EISPACK was coming out; LINPAC was a couple of years later.

FOX: Yes, I don't know those dates either.

HAIGH: And then also in terms of general purpose libraries, I think it was about 1972 that the first versions of the NAG and IMSL libraries appeared. Do you know if that would have been in something that people would even have been aware of in Bell Labs?

FOX: I don't know how aware they were of these libraries which must have come as PORT was being developed. The scientists at Bell tended to use programs they already had for rather targetted computations. However, due to the problem of running programs on an assortment of computer types, the great push for portability started coming along. Researchers at Bell who had PDPs or other computers wanted programs that could be run on them.

HAIGH: Do you remember how the first version of the PORT library was produced?

FOX: I guess I found various programs for different numerical uses and put them together. But at the same time, we wanted to have a library which was portable that could be installed on a variety of computer types. Also Bell Laboratories had many other branches, Parsippany and Holmdel, various places, with their own computers, so it was would have been desirable to have a library that everyone would know about installed at these places.

HAIGH: Yes. In fact, I see a list of the platforms that the first version was used on. It mentions an IBM 360 and a 370, a Univac 1100, and some Honeywell 6000 series machines.

FOX: And it was important that the library could be installed easily and be well-documented.

HAIGH: Now, you mentioned that an early step was gathering together the decks. So did a lot of the code in the first version come from the existing Bell Labs libraries?

FOX: Oh yes, written by people who needed them for various research needs. They were mostly written by people in research. For example Jim Blue had written some excellent programs for numerical integration.

We started collecting the programs and getting them on tape. If there was a problem with the program or its application it could be solved quickly because the person who had written it was at Bell. We could fix it right up. So I think it made for quite a robust initial library, which probably added to its acceptance.

HAIGH: So in many cases you would be taking internally developed routines that were already in use, and then going back to the original author and working with them to rework this into a version suitable for the libraries. Is that correct?

FOX: That's quite correct, and if one looks through the PORT library and looks at the authorship, to a large extent they were people working at Bell Labs.

HAIGH: Now, you've mentioned that the library, from the beginning, was intended to be used across a number of different Bell sites. So was it necessary for you to reach out to people at different installations and produce some kind of consensus on what was needed?

FOX: No. Rather we just worked hard getting together a well-balanced set of programs. I got to one person, say, at Homedale, one person somewhere else, whom we could give the library to. Mostly they didn't need help installing it because I had written an installation manual and they could use that to put up the PORT library and use it. If they had questions they would contact us, but it seemed to go without many hitches, as I recall.

HAIGH: Were the authors of the routines in the first edition all at Murray Hill, or were some of those coming in from different installations?

FOX: They were at Murray Hill so far as I can recall.

HAIGH: I did gather a list of the names that I'd seen associated with the project, so I think I will just ask you about the people one at a time, and if you can just say who they were and what you recall their main contributions being. So the first one is A.D. Hall.

FOX: Yes, Andy Hall. He came out of research. After a while, he became the head of the computing center. He was very helpful to the PORT project. and contributed a lot. He was a very good programmer; not necessarily a mathematician. But the concepts of having portability due to the machine constants was one aspect of PORT that he and I and others worked out. He supported the library very well up until the time he had to go elsewhere. And funding for the PORT library at the end was very awkward. I'll come to that later.

HAIGH: So he was your boss, was he?

FOX: Yes.

HAIGH: B.G. Ryder?

FOX: Barbara Ryder and Stu Feldman developed a Fortran compiler called PFORT, which was a key part of PORT. PFORT, used with PORT, would check that a Fortran subroutine was written in particular subset of 1966 ANS Fortran as defined by PFORT, before it was put into PORT.

HAIGH: So was Feldman someone inside Murray Hill?

FOX: Yes, he was in research. Very good programmer. He was peripherally part of the Unix group, but he was good at languages and wrote a Fortran checker that was very good. He was involved in other kinds of programming too, that I don't recall now.

HAIGH: So was development part of the computer center, or was that in a separate division?

FOX: No, at Bell Labs if you're were in "research", that was it. Otherwise, you were in "development". And if you haven't done marvelous things in research, there's a possibility that you will go into development.

HAIGH: So development was pretty much everything that wasn't research.

FOX: There's no middle ground. The computer center was in development.

HAIGH: All right. Norm Schryer.

FOX: Yes, Norm Schryer, marvelous man. He's a very good mathematician. He did some phenomenal routines in partial differential equations of various sorts, for stiff equations. He was an excellent programmer and a very big part of PORT, very helpful, excellent, wonderful. Much of the code in the partial differential equation section is due to him. He also had a great deal to do with the machine constants in PORT. As a slight digression, one of the aspects of installing PORT was to specify three machine constant values dealing with the smallest numerical value for the machine, the largest, etc. both in single and double precision. Once these were specified (we gave the values for the most common computers) the tape could be run and the installation completed. Norm Schryer wrote a most incredible and extensive program that could be used after PORT was installed to test the computer's floating-point arithmetic unit. These matters and other aspects of PORT are discussed in a paper written by me and Andy Hall and Norm Schryer for the ACM Transactions on Mathematical Software, Vol. 4, No. 2, June 1978, pages 104-126. The paper is titled, "The PORT Mathematical Subroutine Library."

HAIGH: And where in the lab did he work?

FOX: He would have been in research.

HAIGH: And W.S. Brown?

FOX: That's Stan Brown. He was the next level up. I think he was a department head, and some of these people were under him, such as Stu Feldman. I can't remember the structure, but maybe Norm was in his division. Stan had a set of people and he was busy with the numerical aspects of PORT and the portability thereof. He put the numerical model specified in PORT on a firm theoretical foundation.

HAIGH: So I think those were the names that I found on early papers concerned with PORT. As I looked in other places and at the remaining little Bell Labs page on the project I found some other names, so I'll go through those as well. Jim Blue.

FOX: Jim Blue was our integration expert. He could do integrals of various sorts and he got very good at that. He was a physicist, I think, by training, but very good mathematician, and a lot of PORT was contributed by him. People such as Jim, obviously, if they had to solve a problem, they would consult the literature in some depth until they got to know the field very well. Then they would write excellent programs to do the work, and these routines, in turn, became part of PORT. So he was very helpful especially at integration, but in other areas as well.

HAIGH: Wayne Fullerton.

FOX: He had less to do with the PORT library. I don't know which program you found his name on. I don't remember any particular one, but he was a guy around in numerical analysis in general. I think he was specially interested in special functions.

HAIGH: Was he in the computer center, or was he in one of the other areas of the lab?

FOX: These people that you mention probably were all in research.

HAIGH: David Gay.

FOX: David Gay, another expert. He was very good on things like linear programming, optimization and operations research and did a lot of good work in maximization and minimization. He's a superb programmer, very good at writing code and a very intelligent man.

HAIGH: Eric Grosse.

FOX: I don't know of many programs he contributed to PORT, but he did a lot of things with the structure of PORT and getting it to go on various machines. He wasn't one of the major contributors, I think, but an excellent programmer, smart guy.

HAIGH: Andy Hall.

FOX: Andy Hall we mentioned earlier. He was initially in research, and later became director of the computing center.

HAIGH: Linda Kaufman.

FOX: She came to the labs a little later. Her specialty was linear algebra and she wrote a lot of the matrix and eigenvalue codes for PORT, a tremendous amount of this was due to her, including all sorts of band matrices. There's a huge section, especially in the last version of PORT mostly due to Linda, routines on linear equations, matrices, eigenvalues. And even though LINPAC and EISPACK were available, still we felt she had done certain excellent routines that were useful in the labs and that were very good to put into PORT library.

HAIGH: And Wes Petersen.

FOX: Wes Petersen. A physicist. He was in the computing center. He was a physicist, also a good programmer, and, as I mentioned earlier, he worked with a lot of the physicists in research and he could tell them what to use and help them solve their problems. So he was a great addition to the computing center.

HAIGH: Now, do you recall anyone who made a significant contribution to PORT at any point who I haven't just mentioned?

FOX: I'd have to go through how many hundreds of programs. I think you've hit the bulk of it. It's a historical thing. Some of the early ones were Jim Blue and David Gay and Norm Schryer, and then later, people came in and did other things. Also we took a few things from the literature adapted them (with the authors permission of course) to PORT

HAIGH: Now, you've mentioned that many of the routines, especially in the first version of PORT, were coming from those that were already in the informal library within Bell Labs. I was wondering, were there any main areas where you either added things that hadn't been covered at all or you really had to toss out what was there and came up with something pretty much new for the first version of the library?

FOX: Strangely, it didn't really develop like that. It was based on things that physicists wanted and that were already running at Bell Labs, so mostly, that's what there was. There were big gaps; for example, I think that we didn't have some of Traub's ideas of root solvers, so I tried to plug the gap there. Differential equations and PDEs came along a little later. For differential equation solution Bell had particular needs, for example, for their research in transistors for stiff differential equation solvers. Norm did work on these. Especially he wrote very good programs for partial differential equation solution. All these were added to PORT. So it was, I would say, mostly internal.

HAIGH: Were there many cases of things being written specifically because there was an area that PORT didn't have good coverage of? Or did it continue to follow the model that you would take things that had already been produced anyway, because they were needed, and tidy them up and make them more portable?

FOX: Mostly the latter. I think we felt that if something wasn't much in demand, and if there were other libraries available that had it, we didn't do it just to make a complete library.

HAIGH: Right. Now, you've mentioned how sensitive you were to the things that the physicists were demanding. So I wonder, do you think that there were any important differences between the kinds of things that your physicists and other users at Bell Labs were demanding and the kinds of things that people in Livermore or Argonne or Los Alamos would have been required to provide for their users?

FOX: Not a great difference. I mean, there were these hard areas of computation, stiff equations, for example, as I mentioned before. When there was an area that was not very adaptable to a standard solution, then that would have priority, because people were going to write programs for it. When the programs were working, we put them in the library. Some other places like Los Alamos probably had a few more PDE programs and other similar things that were local to their particular research. But PORT actually got fairly complete over the years because the researchers wanted everything.

HAIGH: Do you think that the typical kind of size of the problems that Bell Labs people were running would be similar to those of the National Labs, or do you think in most cases they may have been smaller or larger?

FOX: Well, weapons research, of course, is very different. Take a transistor versus a bomb. Well, there you have it. All such problems come in many different sizes, many of them are challenging in the sense that there aren't programs that are able to deal with the edges of numerical analysis, the stiffness or the big discrepancy in speeds that occur in certain situations. A transistor for example has many different scales, so there's a spread in there that's very awkward to program.

HAIGH: So a similar and broad range of problems, but with less of a bias compared with the National Labs towards very large problems?

FOX: Yes, that's right. They would have, I think, more likely large self-contained programs.

HAIGH: Right. Some of the publications cover Port library itself in some detail, including the standards for coding and documenting and the constants and so on. But I wonder if you could just talk about the kinds of work that you would have to do to take one of these pre-existing routines and turn it into a form that was completely documented, portable, maintainable, and ready for inclusion in the library.

FOX: Yes, you're speaking of the grunt work. Bell Labs astounded me when I went there from NCE, the amount of help was nothing. There was one secretary for large departments, and the guys did their own coding; they did their own typing for papers and books they were writing; they did their own photo typesetting, they did their own everything essentially, so there wasn't a way to delegate and distribute things. I would keep track of the PORT tapes and the latest versions. I don't know if you want to get into it, but the documentation was amongst the hardest things. The people up in the Unix group (I call it the attic) had gotten an early version of a phototypesetter, which was an early way of making something that looked like a typeset page. It was an optical thing. You would type out in a Unix editor what you wanted, you'd run it through the phototypesetter which had transparencies of various fonts, and you'd get out something that was a wet piece of paper, and you would hang it up on a line in the attic with pins. For example, McIlroy, a fabulous and very smart and well-known guy, who was a department head over the Unix group, would himself help keep things clean and he would hang pages up (though of course I did my own!). So the first PORT documentation was put out by me with this process, which was slow and painful. And those Unix guys were smart. They once typeset pages on the phototypesetter of an article that looked just like *Phys Rev*. They sent it to *Phys Rev,* but the journal didn't know what to do with it. They wouldn't take it. The Unix guys set it all themselves, and it was all identical. That's not an easy feat in a case like that. So they branched off into that, wasting their time, perhaps. So, for PORT, I did all the documentation and all the keeping track of programs and some writing and fielded some requests, and whatever.

HAIGH: So would that system you're talking about be the ROFF system?

FOX: The which?

HAIGH: As I understand, there were a lot of Unix text formatting systems in the area with names that ended in OFF.

FOX: NROFF, TROFF, EROFF they're very good. Unix had a lot of things to help one do very good editing. Especially, for us, EQN, written by Kernighan I think – a program to allow one to produce very complicated equations. You had to put in a lot of characters; it was quite primitive. So that development also went on up in the Unix group in the attic. It was fortunate for me it came along just in time that we could do this kind of thing for PORT.

HAIGH: So that was the typesetting and production of the manual. Now how about actually writing the documentation itself? Is that something that you would be able to get developers of the routine to do?

FOX: It was very important for them to do the documentation. Mike Lesk and I helped them by writing a set of macro instructions available on Unix with procedures for writing, typing, and printing PORT reference sheets. We didn't have too many pages for each PORT program, three or four: the numerical method, the variables, an example of using the program, and the references. We wrote an internal report describing how to use the documenting procedures.

HAIGH: I will try and edit in a reference to that one, then, at this point in the transcript. Ah, yes, actually, I just found the relevant on page 356 in Cowell, and it says, "The documentation for the first edition of Port was done using B.W. Kernigan's program EQN."

HAIGH: He's one of those famous C and Unix guys.

FOX: Yes, and he's a wonderful teacher and writer. I think he ended up at Princeton, perhaps. His books helped people learn C. He was marvelous. But he also wrote a lot of code. He's very good.

HAIGH: You say that was very primitive and you had to pull each sheet through, and it says by the second edition of Port, 1976-'77, things have picked up, and together you were able to keypunch it and set the whole thing relatively easily on the computer sensor's phototypesetter.

FOX: Did it say keypunching?

FOX: But weren't we online by then? I don't know. Most of the time, I would input the EQN and TROFF and NROFF files that would go through the PDP and produce the output.

HAIGH: It could be maybe the data entry people were still working with cards.

FOX: It would surprise me, yes, but could be, because there were tapes then on which we wrote the programs. That was a production problem, mostly.

HAIGH: And how about testing of routines? Did you assume that the routines had been tested, or did you have to do a lot of the testing?

FOX: Well, what I consider the ideal way is what we did. The subroutines had often been around quite awhile with researchers using them, and they used them hard. They used them all around the edges. To set out and do a real thorough testing, as we did with DESUB, is a several months' project. There certainly wasn't time for one person to do that. So, for example, if people used Blue's quadrature thing and had a problem, they would confer with Blue and we would change the code as he saw fit, and then we would get a better version out. But I think often by the time I got the programs, they had been already tested a bit, because all this research was going on all the time. It was a good way, sort of a self-testing situation.

HAIGH: So you don't remember much formal testing of the PORT versions of the routines?

FOX: I don't, and in fact the people who wrote them did it themselves in some detail. Linda Kaufman, for example, all of her eigenvalue and linear equation things she would have tested very thoroughly, and if people had trouble, then she'd fix it. So it was very robust code by the time it got to PORT.

HAIGH: Okay, so that's testing and documentation. Let's talk a little bit about the techniques that were used to make the code more portable.

FOX: There are various articles on this. One main aspect was that the version of Fortran used in PORT should work everywhere, and the other main portability device was the use of machine constants. All the machine-dependent values used in PORT had to be written in terms of the machine constants. (The following is excerpted to some extent from the PORT article in Cowell's book: These constants were specified for real(single-precision) and double-precision floating-point quantities and for a couple of more values. A programmer or program wishing to gain access to these values could call on three functions provided in PORT: R1MACH for reals, D1MACH for double-precision, and I1MACH for integer values, indicating by the argument to the call which value was needed. (Notice the PORT convention of using a digit as the second character of the function name to help prevent name conflicts.) All the code in the library was based on these constants, which is what made it portable. When the library was being installed you would take the Cs for comments off the constant statements that related to your computer and install the library.The version of PORT we sent out provided machine constants for most of the computers around, but also people could put in their own values. So those were the main things, really. It sounds very simple, but the concepts weren't. You have to be careful because the values for the machine constants for a particular computer, e.g. the largest and smallest are always a little tricky. Initially we had left it up to the installer to find appropriate values, but later the PORT tape included values for a great many computer types. PORT also had various testing programs for testing the machine constants, testing the integer real and double-precision, and testing them at all the edges for overflows or largest or maximum. These remarkable tests were due to Norm Schryer. So PORT was quite robust in that sense.

HAIGH: Were these constants handled using a pre-compiler that would insert the appropriate blocks of code, or were the values determined at run time by a call to a function?

FOX:A pre-compiler was not used. If a constant was needed by a running program, it was obtained using calls to the appropriate machine constant subroutine, e.g. R1MACH.

HAIGH: I think that's a different approach from the one that was used by NAG and IMSL, where they actually produced a separate set of source code files for each of the platforms that they supported, but they automated the process of producing source code branches with some specialized tools. So it sounds like your version, the code that is compiled into every executable differs only in terms of the constants.

FOX: It seems simpler to have only one version of a library!

HAIGH: That's right. Now, was that a deliberate decision to use the approach that you did, relying only on constants instead of developing any kind of compiler tools? Was it just something that you never considered?

FOX: Portability was the big word, and portability means you don't want to have a lot of pre- or post-processing. We just have one library, strip the "C"s on the relevant constant cards, and install it. Also at each computer we recommended that the PFORT program be used to check to see if the target computer was using the version of Fortran that we assumed was a standard.

HAIGH: So when it was necessary to move the library to a new platform, obviously from what you said, one of the things the recipient would need to do was determine the new machine constants.

FOX: Yes, though most of the time these values would already have been made available in the machine-constant comment cards provided with PORT. Just strip the "C"s for your machine and install! As I said we had developed, after a while, quite a complete set of machine constants for all of the machines we knew about. If we found more, we'd put them in. So if someone wanted PORT, we would send over a tape and the installation manual and they would do it at their site, whether they had PDPs or whatever.

HAIGH: So that was really the only thing that you need to do. As long as they had a compiler that was able to compile the standard subset of FORTRAN, then all they needed was the appropriate machine constants.

FOX: Yes, and there are even ways of expanding the common area in PORT. It was used by all the programs that used scratched storage, and there were ways of expanding it if you needed more space.

HAIGH: Okay, so while we're talking about portability, as I'd said before, the initial platforms were the 360, 370, the Univac 1100 and the Honeywell 6000 series and some PDP 11s. That would have been circa 1975. Now, I know that a range of new platforms would have become important over the next 10 years or so.

HAIGH: Yes, well one of the machines I know appeared at the lab was a Cray 1.

FOX: Yes. That was in 1979. So we got a Cray 1, a marvelous machine. Actually before we got it we wanted to evaluate it from the point of view of user needs at Bell for large-scale "number-crunching." We collected a set of test programs from relevant fields, primarily linear algebra and differential equations, and ran them with timings and costs on a variety of computers. We built these test programs over the PORT library and were able to install PORT on the Cray so we could easily compare the results with various of our other computers running PORT. We were particularly pleased that the people at Cray Research, Inc. were able to run the benchmark programs very quickly, with no trouble at all. After setting the three functions to return the appropriate machine-dependent constants PORT was compiled on the CRAY-1 in 10 seconds (41,000 lines of Fortran) and the tests run immediately. The results we obtained from the various computers seemed particularly "fair" to us in the sense that the code had not been changed between the sites.

HAIGH: So even after it was moved to the Cray, you continued with the approach where the only thing that you changed were machine constants?

FOX: Yes.

HAIGH: Did you work closely with the Cray company? Did they have training courses and seminars and good ways of explaining what techniques would help performance, or was it something that you had to discover on your own?

FOX: I don't think they were great for documentation, but I think there were manuals that told you some things, and you could figure out, once you knew how the thing worked, how you could make it so it would vectorize, so it was mostly a self-help situation. I had one guy (I won't name him) who wasn't good at a lot of things, but he was a marvelous speaker. He kept taking courses on public speaking and he gave good talks. He would go and give talks around on how you wrote Fortran for the Cray. He was very helpful to people. There was a lot of interest with this because you could get fabulous work out of the Cray.

HAIGH: Do you recall the Cray making a big difference to the kinds of problems that it was feasible to tackle?

FOX: In my experience many of the programs run by research at Bell were well adapted to vectorization. But what areas of physics they might be in, I wouldn't necessarily know. A lot of this was transistors, and various differential and partial differential equation solution. In its early days Bell Labs dealt more with analog computations: e.g. wave forms and noise on lines, rather than digital matters, but all such computations, analog and digital, involve variables in the form of vectors which vectorize nicely on the Cray.

[Tape 3, Side B]

HAIGH: So we've just discussed the issues involved with the arrival of Cray 1 in the labs. Now, it seems that the library was available on the PDP 11 from the beginning. Do you remember any challenges involved with the profusion of mini computers of different kinds, such as the VAXes and the Data General machines?

FOX: Well, most people managed to get it up on those machines. The computers had increasingly good memory sizes by then, so PORT ran on a lot of the smaller machines in the physics labs. Probably the people who were using it would have coped with any problems more than I would. Once they got it up, it was okay.

HAIGH: Do you know if it would have been widely used on the smaller machines by the late '70s?

FOX: It's hard to know because they were self-contained, but I think so. Maybe there should have been a falling off on the in the computation center, but of course then the problems always gets bigger, so they would come toddling back with the bigger problem.

HAIGH: Right, so that was an area where the portability of the library was important.

FOX: Yes, that's been very important.

HAIGH: Did you have any experience with multiprocessor machines?

FOX: No, not really. They didn't come along until when? I think later than that, perhaps.

HAIGH: Well, I know at places like Los Alamos they were very interested in them, even in the late '70s, but they had enormous amounts of money to throw around, and I think were seeking out the technology before it was even offered commercially. So multiprocessor Cray machines started coming along at some point in the early '80s. There was also, in some areas, I think, a lot of interest in hypercube machines and massively multi-parallel.

FOX: Yes, this is beyond what we were doing.

HAIGH: So no multiprocessor machines had entered use in Bell Labs during your time there, as far as you're aware?

FOX: Not that I know of or remember, but it could have happened.

HAIGH: But the library stuck with single-processor things?

FOX: Yes.

HAIGH: Also, the early 1980s saw a spread in the use of personal computers and workstations.

FOX: Oh, yes. And Mathematica came along. I don't know the dates of Mathematica, but that was a marvelous program.

HAIGH: Was that something, then, that was widely used within Bell Labs?

FOX: Well, it was just beginning, I would guess, by then, but I think the physicists found it, and liked it; it's pretty powerful.

HAIGH: That's right, I think that came out in the late 1980s, actually. I don't think that would have been available while you were at Bell Labs.

FOX: Probably not, because I remember that you could get it after a while at home.

HAIGH: MAPLE was slightly earlier, I think, on the symbolic side.

FOX: MAPLE, yes.

HAIGH: And there was a system called REDUCE.

FOX: Yes. We were in our little bailiwick, not looking about.

HAIGH: All right. So I did see a 1984 memo confirming that the library had been used successful on an IBM PC/XT, one of the second generation of IBM PC computers.

FOX: Yes, I seem to remember that was Leonilda Farrow. She was a physicist and a very good programmer, and she might have gotten it up.

HAIGH: So are you aware of whether workstations and personal computers would have emerged as a significant platform for PORT during your time there, or do you think that would be the most experimental?

FOX: I think it was increasingly used on the minis, but not yet on personal computers, (If there is a difference!)

HAIGH: So just concluding with the portability issues, I know that you presented something at the 1974 Oak Brook workshop on portability and mathematical software. Portability was becoming a hot topic in a number of areas in the early 1970s. So I wonder if you have a sense of what might have been novel or influential about the particular approach to portability that you took with PORT?

FOX: I think it was different. I think Brian Ford of NAG was very interested in this; we all talked to Brian. I think he began doing similar things, I don't know. But, if I have described it correctly, it was so simple that it was very popular. It was a very easy way to do it and it worked. And I think that for others, the concept wasn't there. They wrote a version for the Univac or a version for IBM or a version for this or that. But this concept that you really could move a single version around was, I think, the contribution of the Bell Labs to this activity.

HAIGH: And as you began to present the PORT library at various mathematical software conferences and workshops, do you remember anything about the reception that it received from other members of the mathematical software community?

FOX: Oh, I think it was very positive. We got inquiries and people liked the idea, and pretty soon most people knew about it and knew how we did it. It wasn't a patented thing at all. I think it was well received. Initially, there was a problem, of course, getting it out from Bell Labs. It finally became a licensed product.

HAIGH: Yes. Can we talk in a little bit more detail about this licensing issue, then? Initially was it a problem to distribute the library outside the lab at all?

FOX: Licensing depended on the perceived value of a program or a paper as it related to Bell Labs. For example, Unix, at first, was not thought of as very important by upper management, so the Unix group were able to send their C compiler and operating system around to many universities. (Unfortunately the Unix guys always sent the latest version which got them into some trouble because all the recipients were running different versions.) But Bell did have a large staff of lawyers and they were careful about sending things about, so it took a while. Yet, since PORT had been discussed so openly at meetings, we were able to get it out and licensed. I have a letter dated May 1984 from licensing that for commercial installations there was a one-time license charge of $5,000, or a $1000/year license charge for PORT. Universities were charged an initial $1,500 with an upgrade fee of $300/year. The same letter says that 82 educational licenses had been purchased and some 5 commercial ones (e.g. Xerox, GTE, and others.) Another document I have, (unfortunately undated) says that PORT "has been installed on computers such as the CDC 6000/7000, Data General Eclipse, Harris S220, Honeywell 600/6000, IBM 360/370, PDP 10., PDP 11 and UNIVAC 1100." This letter says "A nonexclusive license to use PORT 3 for internal business purposes only (is) $3,500 for use on an initial Central Processing Unit (CPU) at a specified location. In addition, your company may exercise at any time, or from time to time during the term of the agreement, its option to use PORT 3 on additional CPUs at a fee of $1,750.00". Surely written by the lawyers.

HAIGH: So after haggling with the lawyers for a while, that was the arrangement you came up with.

FOX: Yes.

HAIGH: And it says there's a copyright date of 1976 on the library. So had you originally hoped that you might just be able to give the thing away free in the way that software like LINPAC and EISPAC were distributed?

FOX: Or the way Unix was.

HAIGH: Why was Bell Labs so cautious about distributing software?

FOX: I have no way of judging that.

HAIGH: Now, do you remember anything about external users of the library. There is the list, but I was just wondering, were there any users or installations outside Bell that you remember as being particularly important to the development of the library?

FOX: No, I don't, and I don't even remember a lot of phone calls, I must have gotten questions about things. But no. And it may be that I wouldn't have gotten them if it was an issue with a particular routine. In that case they may have gone directly to the author at Bell Labs to ask the questions. So from my point of view, I wouldn't have gotten so many questions, unless there was something wrong with the structure, and or if some of the machine constants had to be changed.

Perhaps as a side note, interest in PORT traveled to Europe to some extent. After I left Bell, I was forwarded a copy of a letter from someone at the Technische Hochschule in Darmstadt, dated 1988, who wished to contact "Mr. P. A. Fox about the PORT library." Also a letter, dated 1989, came from a professor at the Faculty of Electrical Engineering at a university in Ljubljana, Yugoslavia wanting to know about the library.

HAIGH: Do you recall any cases where users outside of the labs might have helped with testing or debugging, or contributed any code to the library?

FOX: No. I assume that they tested the constants. They used the constant testing routine and they could give feedback if it didn't work, but they didn't contribute code.

HAIGH: So for commercial users, there was this royalty fee of $3,500. Do you know what the concept behind that fee was?

FOX: I don't know what their reasoning was. It seemed, I guess, a reasonable amount for the value of the product they were getting and it's use. Free or $3,000, who could judge that? I don't know that they even looked at the charges for other libraries. I don't know what IMSL cost then, for example, or NAG.

HAIGH: IMSL and NAG had a different model. I can't remember exactly what their fee was, but both of them had a subscription model, which was a smart way of doing it, because you needed to renew the license to use any of the code that you had compiled the new routines into. So unless you're going to go back and rip out all the calls you are a customer for life.

FOX: Oh my gosh, really? Oh, no. Could they make it stick?

HAIGH: Well, I don't think they had a problem with people canceling. The plus side was, I think, the subscription fee was quite low, so they just wanted to keep their ever-growing base of happy users rather than exploiting them.

FOX: Interesting. Yes, I think we were sort of understaffed, it being me and that was it, so we weren't eager to do that.

HAIGH: Yes. Now, do you know if any steps were ever taken to promote or advertise the availability of the library as a commercial product?

FOX: No, there's one small blue brochure that talks about it, but that's about it. I don't remember advertisements in any mathematical journals or anything.

HAIGH: So theoretically, it was commercially available; but in practice, it something people would have to hunt down and demand to buy.

FOX: They'd have to know about it, yes.

HAIGH: Do you think that the commercial license fee may have prevented the library from being as widely used as it might have otherwise been?

FOX: Well, the fee was small for the universities. I don't know how many commercial people would have gotten it. I suspect the commercial places would go to IMSL or possibly NAG,

HAIGH: That's true. So say in the late 1970s or early '80s when it was available in return for the license payment, do you have a sense of what the strengths and weaknesses of PORT were compared with IMSL?

FOX: I'm not experienced enough in IMSL to know. There could be areas that are missing in PORT, I think that IMSL would have covered more than we did. But we didn't, as I recall, have IMSL up. I'm not sure about the other sites, but I don't think we used IMSL. So PORT covered what Bell Labs' needs were presumably.

The computational areas listed in the sections of the two three-ring PORT documentation are: approximation, differential equations, partial differential equations, linear algebra, quadrature, roots, special functions, transforms, and framework and utility. Some of these sections are obviously more complete than others.

HAIGH: Let's just talk about the changes in the second and third version of the library.

FOX: PORT 2 in August of 1977 had 41,000 lines of Fortran, 550 programs and 125 documented programs. In June of 1984 the new PORT 3 had 130,291 lines of Fortran, 1,516 programs and 325 documented programs. So PORT 3 was about three times larger than PORT 2.

The library grew to the addition of new sections and many new programs. When Linda Kaufman came to the labs she did a lot of linear algebra and eigenvalue work that we incorporated into the library. Blue did much more quadrature work. Anybody who came introduced something. Wes Petersen was very good at FFTs. I didn't give him credit before, but FFTs were one pf the many things Petersen knew a lot about, and so we put FFTs in. And splines became popular, so I guess Norm Schryer and other people were writing spline routines, least-squares fitting.

HAIGH: Then one thing you're promising was a portable uniform random number generator.

FOX: I think we had that in there, yes. An interesting challenge to make portable.

HAIGH: B-spline least-squares fitting package?

FOX: Yes, so those things mostly got in there, B-spline, least-squares, yes. I think they did.

HAIGH: So those areas that you were promising in 1976, were those pretty much the areas that the library in the end did grow to cover?

FOX: I think they mostly did. I can't remember. It got very tricky at the end, as I will tell you as we go into the next phase here.

HAIGH: Okay, so I think we can move to the next phase soon. I've just got one outstanding topic here. So we've talked about documentation and we've talked about the techniques used for portability. I'm just wondering, were there any other efforts that you had to make in terms of setting down coding practices, maybe in terms of structured programming or indentation or variable naming in order to make the code in your library more standard?

FOX: No, because the Fortran compiler would catch these things, presumably. The people who wrote the routines would, I hoped, fill out the documentation and then I would correct it, so there was sort of a schemata, a template that you could fill in the blanks, sort of a definite scheme for all the documentation so it all looked the same. As I said earlier, there was a set of macros to help define the stylized headings, and the paragraph formats, and so on. I wrote an internal memo on these; "Producing PORT program reference sheets," explaining the macros and their usage.

HAIGH: So that was the printed documentation. How about comments within the code itself? Did you have any new requirements there?

FOX: I haven't looked through. No, I didn't remember anything. That perhaps should have been included in that memo. But mostly nobody would delve into the code, I think, at all.

HAIGH: All right. So you don't recall making any significant efforts to make the code itself more readable? You would basically let the authors use whatever coding style they felt most comfortable with?

FOX: Yes, but the code had sometimes gone through RATFOR, I suppose, and then PFORT, so the appearance of the code was pretty good.

HAIGH: That's right, and as I understand it, RATFOR was intended to make Fortran more structured and readable.

FOX: Yes. I can't remember RATFOR's specs, but it was an excellent program, very good for writing legible and well-structured code.

HAIGH: Yes. Actually, on the same topic, I think you implied earlier that you shifted over to online editing of code rather than keypunching it.

FOX: Oh, yes, that was way back in the beginning. When I first went to Bell, they had DEC computers and we had teletype terminals to access them. If you shared an office, that was awful. Then later, we got directly connected so we could go online to the DEC computers and I could do all the documentation using the ED and TROFF and EQN tools from Unix. This way we achieved quite elegant printed reference sheets. For editing and debugging code, researchers and programmers used the Honeywell computer or whatever computer they were working with.

HAIGH: Well, let's talk about the eventual fate of the library. I know you left Bell Labs in 1984, and that must have been roundabout the time that PORT 3 appeared.

FOX: Yes, well, now I will tell you the drama of the situation—at least from my point of view! By then there was a different head of the computer center, not Andy Hall. Suddenly there was no money available for the PORT library. I don't blame the head of the center; the directive had probably come down from above. The higher ups at that point probably considered a numerical library of little interest since, by that time, they must have been caught up in the throes of divestiture of the company!

So PORT 3 was supposed to be stopped. At that point PORT 3 was in its penultimate stage, it was about to come out. It was, as I have said, three times as large as PORT 2 and very successful. So, I didn't stop. I just went ahead and finished it anyway, and I got it to the people who published it.

Being Bell, they would have found something else for me to do, but I couldn't take that. When you have a thing that's epsilon from being issued…. I still got a salary. But it was really very unpleasant. When PORT 3 was safely out, I submitted my letter of resignation, and I didn't want the classic luncheon with these people who had suddenly chopped PORT like that, which seemed (to my somewhat prejudiced point of view) to be asinine for the labs. Divesture was coming along; everyone kind of knew that. Bell Communications out in Morristown had a PORT library and used it a lot, and not to have finished the last little pieces, I couldn't take it. I didn't fight them out loud, I just went ahead and did it. I suppose they didn't find a way to stop me, and the publishers were very good, and they got it out. So PORT 3 appeared. It was in defiance of everything, so it was the time to leave. With divestiture, everything was breaking apart anyhow. But it was very implicit between that guy and me, because of course, what could he do? He didn't have money for me, but I just was doing it.

HAIGH: So do you think the managerial decision not to support PORT was related to the divestiture, or did it have anything to do with changes in the library marketplace or in computer architecture?

FOX: From the management point of view, they didn't even know what PORT library was, essentially. They weren't computer people so much, and there just wasn't money for anything, so they just chopped everything, because it was clearly sort of the end of things for the company. I didn't belly under to this; I just let PORT get finished. And later it turned out to my advantage, because I was able to do consulting at Bell Communications research. They needed PORT. They needed someone who knew the library.

HAIGH: So while we're still on your time at Bell, I've got a couple of things to discuss from your papers that you said. I just looked through your annual performance report.

FOX: Oh, those performance reports. "She seems to like the money, all right." Yes, it was very funny. Those were a riot.

HAIGH: And so it was one of those things that you were active with over the years was the affirmative action activities and recruiting. Do you remember anything about that?

FOX: Oh, yes. Well, there was the Women's Rights Association at Bell Labs, mentioned below. At MIT, I did recruiting for Bell at the master's level and I talked to a lot of good candidates. They were wonderful. I got them to talk about their master's work; it was very advanced and exciting. And they wanted to come to Bell. This was early on, before Bell was in trouble. Then I found out that if they got to Bell, management put most of them out in Chicago doing machine language coding for telephone switches. Well, [chuckles] you know, I mean these were guys who had done phenomenal work, like doctoral level research at the master's level. They were very excited about it, but when they found themselves doing machine language programming for telephone switches they were NOT happy. So I didn't want to recruit at that level at MIT any more. There was a real big gap in understanding of things.

So far as affirmative action was concerned, there were three programs at Bell: CRFP, the cooperative research program for minorities, GRPW, a graduate research program for women, and SRP, the summer research program for minorities and women. Good programs, but the difficulties working women encountered were not well-understood by management. Once when they had found a woman speaker (from outside) who was known to be studying these matters, management said to her that there wouldn't be an honorarium, of course, since she was so interested in the area. Some of the women staff formed a Women's Rights Association at Bell, in which I was fairly active.

HAIGH: What kinds of things was that group doing?

FOX: Well, we got some of the numerous men's rooms made into ladies rooms, [chuckles] because some of the women were pregnant and in need of those facilities. But more fundamental, for working women is child care and some household help. I knew, from my life, that a woman cannot be successful unless she has good help. The Women's Rights Association did a survey and got names of day-care places around this region. Bell published it as theirs! So I fought in various ways and finally got myself up to top management somewhere at Bell Laboratories and went to talk to him, I think, with somebody else. But I did it and I said, "It seems that to get these excellent women, giving them full-time work, you should have some kind of help in arranging help for daycare for their children.". I talked to this guy about it and he looked me in the eye and he said, "Bell Labs is not going to do anything about *child* care!" Well, it was a different time, but it is that really that makes all the difference in a woman's career. So I lost that battle.

HAIGH: One of the comments that comes up in a number of them, is that you feel that weren't enough resources made available to support the Library effort. That seems to be a running thread through this.

FOX: Did I say that? You know after a time I got used to it, I think, so I just did it. I probably got tired of typing and documentation, but, ah, there seemed to be no choice.

HAIGH: This is the 1979 evaluation that was given to you that you mentioned.

FOX: Who was it? Was it Rossler? Who was the evaluator?

HAIGH: Yes. L. Rossler.

FOX: He was okay. He was my supervisor. He was okay, a very smart man.

HAIGH: It seems that during your time at Bell Labs, you were also involved, I think as Secretary-Treasurer with the ACM SIGNUM Group.

FOX: I don't know what I did, but yes. SIGNUM was a fine thing.

HAIGH: Do you remember anything in general about your involvement with ACM or SIAM, or any of those groups?

FOX: Not much. I was pretty busy with work and kids. No, I don't remember a lot of that. I tried to get to the meetings, of course. That was the main thing.

HAIGH: Now, the other document that we have is a 1982 proposal for a "common software department."

FOX: Oh, you remember the one from Newark College of Engineering? Well, this, I tried again with a guy, McLaughlin. He was very good, and we thought that this would be just the thing for Bell. But of course, there was no response.

HAIGH: So it seems the idea there was to reorganize things to have better support in terms of system tools and libraries for the larger Bell Lab machines.

FOX: In general, it was to have a software center, but software was still outside the ken of upper management. They didn't really know about software.

HAIGH: And within this there would have been the languages group, the numerical and statistical tools group, what you described as the "TROFF/NROFF/SROFF" group, and the applied research group. So the document talks a little bit about all of those. So what did you feel the advantage would be of bringing all those things together instead of having them spread out in different places?

FOX: Well, I think there was redundancy of effort to some extent, and help, and at one place you could probably get further with these things contributing back and forth to each other. Everybody would know one other and they would know how things worked, and how to do the Unix stuff. The Unix people I got to know pretty well, and they were very helpful. So that worked, but if you have a little structure, it sometimes just oils the wheels, and people all get together and know what's going on better. Anyhow, I would say it was probably tending toward that, but it was too early to recommend it. There wasn't a lot of response to this thing, which I thought was quite clearly set out. Because those things should all fit together—it's like a department in a University of course. But then it all blew apart anyhow.

HAIGH: Oh, yes. This I should also say, it's dated February 8th, 1982. So why do you think it fell on deaf ears?

FOX: Oh, they were so busy with their troubles with divestiture. I mean it was clear everything was going to go, which probably they knew better than we would have. So I don't know if it would have done any good to do that earlier because there wasn't very much understanding of software, I felt, at the upper level.

HAIGH: So you mentioned that after resigning from the lab, you remained active as a consultant?

FOX: The main job I had from 1984 to 1993 was as a contract consultant with Bell Communications Research ("BellCore"), one of the offshoots of Bell Labs. Mainly I helped getting program libraries, e.g. PORT, IMSL, NAG, LINPACK, SPARSPAK, EISPAK, MINPAK, QUADPAK, ALTRAN, MATHEMATICA, and getting them installed on the great variety of computers BellCore had, such as IBM, DEC, SUNS, STARDENT, CONVEX, MOTOROLA and more. I fielded questions arising from routines in these packages mainly at the Morristown, NJ branch of BellCore, but also the Navesink and Piscataway and other branches. The job became easier when I got a modem on my computer at home and could get in remotely onto BellCore's machines. I tried, to some extent to get documentation on line, but again that hope was too early, the companies weren't providing it.

There was interesting work going on at BellCore, not necessarily numerical. Many people were investigating the possibilities that MATHEMATICA presented. Also there was talk of writing a converter for Fortran programs to translate them into the C language. I don't know anything that came of it, but surely it must have been done someplace.

Unfortunately there was not as much financial support for research at BellCore, as there had been at Bell labs, so later the scientists and mathematicians began to leave.

On another topic related to consulting: at the request of a man who was considering investing in a new company called Compusonics, I did research on the company and its products. They made mainly Digital Signal Processors. I wrote a report on my findings and a brief evaluation.

HAIGH: Did you maintain any other interests related to mathematical software after retirement?

FOX: Not really. I became very interested in the new home computers that were coming out, and initially I had both an IBM one and an Apple. I subscribed to all the magazines and tried the new software that was being produced. Currently I have a PowerMac G5 running Mac OS 10.4.5.

HAIGH: So have you had any other important interests or activities in the twenty years since you retired?

FOX: [Chuckles] Currently I'm doing the first volunteer job of my life. I'm a member of the Friends Group of the Library in Morristown. Of course since I'm the mathematician they made me the Treasurer of the group, and it takes a lot of time. But I like those women. Lots of them have never worked, and some of them don't even have email, but it's fun. Another interest for me is playing the piano and harpsichord and a clavichord (that my husband built). At Bell Labs there were lots of people interested in music (the usual correlation between science or math and music). For five or ten years three of us played music at my house with the harpsichord. One was a mathematical physicist and a fine flutist, the other, Dave Slepian, who is quite famous was on oboe. Do you play anything?

HAIGH: Uh, no. I had to play the recorder when I was still young, but I really never had any talent for it, unfortunately.

FOX: That's a shame because the recorder is nice with the harpsichord. So I enjoy the music. We go to concerts and ballet and modern dance in New York and we have traveled quite a lot. I would say it's a nice life. But the impulse to pursue science isn't there any more. I still take

Science and *Nature*, and all the computer magazines, and I'm trying to learn a little bit about biology.

I do the computer part, email and stocks and such for George, my husband, who has never touched a typewriter, much less a computer. Nonetheless he was a professor at Rutgers in New Brunswick, NJ where he really founded and ran the Center for Urban Policy Research. He wrote innumerable books, lots of books. He would dictate to a staff of typists who knew his lingo, so he got books out galore. He had a lot of data. He is very good at data and analysis, even though he is not a mathematician. So he did that, and I said, "Oh, he's retiring. I'll have to type the books," but I haven't done that. Anyhow, he took up gardening in a very serious way! So he gardens. He cooks. I cook. Nice life. You'll get there in your time.

HAIGH: Hopefully [chuckles]. Now, we'd conclude by asking actually the same two questions that I finished earlier interviews with. The first one would be, looking back on your career, what do you think your biggest regret would be? Either in terms of things you could have done differently or things you wish had worked out differently, or in terms of opportunities you wish had been more open to you?

FOX: You know, I can only think of positive things. Isn't that interesting? Well there were these barriers I hit on account of being a woman. I'm not sure in my case that it was bad. It gave me a little more gumption. So I think I probably wasted a bit too much time at Newark College of Engineering, sort of battling them. At Bell Labs, in spite of my, perhaps adverse comments, everyone agrees it was about the best time to be at Bell Labs, for those ten years. Research was flourishing, and just it was a marvelous time. It was very exciting. Some of the best people around in the world were there doing research. The Unix team was there and all sorts of programming and development were going on. It was a wonderful time until it all went "fooey." That's probably your second question, actually.

HAIGH: Well, actually, before that, I'll ask a follow-up. Remembering what you said yesterday, it seemed that originally, you really wanted to be a practicing corporate engineer, which was what sent you back to get more education. Then if MIT had let you, you would have done your Ph.D. in Engineering. Do you think that if you had been a man, you probably would have continued on that path instead of taking this more circuitous route?

FOX: I think it would have been a bad idea. I hope I didn't say that I wanted to be a corporate engineer, because I'm not one for management. But I wanted to be an engineer. I had to get an engineering degree so I could get jobs. That was it.

Yes, but on this matter of the education, the best thing that ever happened to me was that the EE department turned me down for a doctorate, because all the people who got doctorates in that era were specialists in tubes and electronics, in what has because an archaic field. Mathematics lasts longer.

HAIGH: And so the second of the questions would be then, in looking back on your career, what do you think the single achievement is that you would be proudest of?

FOX: Well, I suppose the PORT Library, being the most recent, stands out the most. I'm glad I have been in many diverse occupations, the differential analyzer, and numerical work, and programming. I think I go more for the spread than for the peaks in certain things. So I guess, the PORT Library and the articles I wrote, and surviving motherhood and children [chuckles], yes. It was some great luck that got me just to these various places at the right time, Los Alamos or Bell or NYU. So I was lucky to get into each of these locations. That was not a credit to me; I think it was chance. I don't have any particular pride in all this, but, you know, I tend to finish things when I get started and that worked out for PORT.

HAIGH: That concludes the questions I've prepared. If you have anything else you would like to say, now would be the moment.

FOX: No, I find it very interesting, because I never would have gone through all this stuff on my own.

HAIGH: Thank you very much for taking part in this interview.

Appendix: Phyllis Fox Resume March 2006

66 Old Short Hills Road
Short Hills, NJ 07078
973 379-6922

Education

M.I.T., Sc. D. (Mathematics) 1954
M.I.T., M. S. (Electrical Engineering) 1949
University of Colorado, B. S. (Electrical Engineering) 1948
Wellesley College, B. A. (Mathematics) 1944

Employment

1984-93	Bell Communications Research, Morristown, NJ Consultant for Mathematical Program Libraries
1973-84	AT&T Bell Laboratories, Murray Hill, NJ Numerical consulting and mathematical program library development June 1984, PORT 3 library project completed (some 130,000 lines of Fortran, and 1200 pages of documentation for over 300 subprograms (supported by 1513 subroutines modules))
Summer 1967	Bell Laboratories, development, testing, and installation of a program for differential equation solution. Continued at the labs on a consultant basis to July 1969.
1963-73	Newark College of Enineering (now New Jersey Institute of Technology) Professor of Mathematics (tenure granted 1972)
1960-62	Computation Center, M.I.T. Research Associate Worked in the fields of numerical analysis and artificial intelligence (wrote first LISP manual). Member of the Committee on Long-Range Computation planning.
1958-60	School of Industrial Management, M.I.T., Research Associate Developed the simulation language DYNAMO for use in industrial dynamics.
1954-58	AEC Computing Center, Courant Institute, New York University, NY Computer methods for the solution of partial differential equations.
1949-53	Whirlwind computer, M.I.T., Research Assistant
1944-46	General Electric Company, Schenectady, NY Operated the Differential Analyzer

Publications

The PORT Mathematical Subroutine Library, in *Sources and Development of Mathematical Software*, Wayne R. Cowell, Ed., Prentice Hall, NJ, 1984, pp. 346-374.

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On the Numerical Solution of the Equations for Spherical Waves, (with Anthony Ralston), *Journal of Mathematics and Physics*, Vol. 36, No. 4, January 1958, pp. 313-328.

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Perturbation Theory of Wave Propagation Based on the Method of Characteristics, *Journal of Mathematics and Physics*, Vol. 34, No. 3, October 1055, pp. 133-151. (doctoral dissertation)

Propagation of Shock Waves in the Generalized Roche Model, (with P. A. Carrus, Felix Haas and Zdenek Kopal). *The Astrophysical Journal*, Vol. 113, No 1, January 1951, pp. 193-209.

Propagation of Shock Waves in a Stellar Model with Continuous Density Distribution, (with P.A. Carrus, Felix Haas and Zdenek Kopal), *The Astrophysical Journal*, Vol. 113, No. 3 May 1951, pp. 496-518.