An interview with
BILL BUZBEE

Conducted by Thomas Haigh
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ABSTRACT

Bill Buzbee discusses his career as a mathematical specialist within Los Alamos and the National Center for Atmospheric Research (NCAR) and his involvement in the field of mathematical software. Buzbee served in the Air Force before gaining undergraduate and masters' degree in mathematics and numerical analysis from the University of Texas at Austin where he studied with David Young and Bob Gregory. After graduation in 1962, he went to work at Los Alamos where he remained until 1987 aside from a brief stint at Esso in 1967-8 working on three-dimensional reservoir simulation. At Los Alamos he worked first on weapons simulations, but soon joined the central computing group, where he worked on implementing algorithms from Jim Wilkinson's Algebraic Eigenvalue Problem. During this period he came into contact with the emerging community of people interested in mathematical software, including Cleve Moler, Jim Pool, Gene Golub and Jack Dongarra. He obtained his Ph.D. from the University of New Mexico in 1972, after which he took over the management of a group within Los Alamos working on mathematical software and programming services. In 1974 Buzbee became a member of the Common Math Library Subcommittee of a new inter-laboratory program called SLATEC, devoted to encouraging technical collaboration between the computing departments of atomic weapons sites. This program eventually produced the SLATEC Library, a merged and consolidated collection of mathematical routines. During the early 1980s, Buzbee rose into more senior management positions at Los Alamos and played an important role in advising the federal government on the new field of supercomputing. He also emerged as an expert in the new field of parallel computing. In 1987, Buzbee moved to the National Center for Atmospheric Research (NCAR) where he headed its Scientific Computing Division until 1998. While the division thrived under Buzbee's leadership, in 1996 he found himself in the center of a political storm following a decision to procure the Japanese-built NEC SX-4 supercomputer for climate simulation, rather than an American model. Buzbee discusses the issues involved and his experiences at this time. Buzbee retired not long after the procurement was halted following actions by the U.S. Department of Commerce to create trade barriers against the importation of the Japanese machine. He has remained active as a consultant to several organizations, including the Arctic Regional Supercomputer Center.

Keywords:

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

BUZBEE: My pleasure.

HAIGH: I wonder if you could begin by saying something about your family background and early life

BUZBEE: I grew up on a small dry-land farm in Central Texas. Fortunately, when I was around nine or ten years old, my dad went to work in a nearby oil field for an independent oil producer—otherwise we would have probably starved to death and moved to California. (That was a joke, by the way [laughs]). So I began my life with manual labor. I not only worked on the farm but also in the oil fields once I got to be a teenager.

HAIGH: And what kind of work was your father doing with the oil company.

BUZBEE: He was what was called a "pumper." You may have noticed, even here in Colorado, as you drive up I-25, you'll see what we call jacks, they're pumps going up and down. Those require daily maintenance—that was his job.

HAIGH: I saw in some of the material you provided that as a teenager, you were a roustabout. What does a roustabout do?

BUZBEE: Whatever there is to do [laughs]. That means laying pipeline, cleaning out storage tanks—whatever needs to be done. Generally it's pretty grimy work, but the pay was good, so I was happy to be doing it.

HAIGH: Do you think those early experiences had any influence on how you approached your later career?

BUZBEE: Well for sure, the fellow I worked for was a civil engineer and he provided a lot of encouragement to get an education. And having experienced manual labor and so forth, that also motivated me to get an education. My parents provided a lot of encouragement as well.

HAIGH: As a teenager did you have any kind of interest in science or mathematics?

BUZBEE: Oh yes, as a matter of fact at that point in time—this was back in the '50s—the State of Texas had a statewide competition in mental arithmetic. You were given a hundred problems and three minutes to solve them and all you could do was write the answers down. I placed seventh in the State of Texas in that competition, I think it was in either my sophomore or junior year,. In the process, I got to visit the campus of the University in Austin, which I found very alluring, and later went to study there. So relative to science and mathematics, that was the most significant thing as a teenager.

HAIGH: Now I understand from the material you sent that before you went to the University, you first were in the Air Force.

BUZBEE: That's correct.

HAIGH: As you approached graduation from high school, were there any other options that you considered? Service was voluntary at that point, wasn't it?

BUZBEE: Actually after graduating I did one semester in college studying mechanical engineering. And then three of my high school buddies were home for Christmas holidays and the GI Bill was about to come to an end, so we decided that the four of us would enlist in the Air Force together and go through basic training together. We did it to get the GI Bill.

HAIGH: Was that because otherwise it would have been difficult financially for you to attend college?

BUZBEE: Yes it would have.

HAIGH: What were your experiences in the Air Force?

BUZBEE: The Air Force gives you a number of aptitude tests, as part of basic training, and they encouraged me to attend what was called "pre-lang school," to become a linguist. The school happened to be located in San Antonio where I went through basic training, and it was a six or eight week school. The purpose was to go there and study Russian, and if you were able to do well, then you would be assigned to a longer study program. So I went to Kelly Air Force Base and went through the pre-lang class successfully. There were seventy-six in my graduating class; fifty-six of them were assigned to go to Syracuse University to study Russian, ten were assigned to go to Yale University to study Chinese, and ten were assigned to go to the Army Language School in Monterey, CA, to study Korean. I was assigned to study Korean. While it was permissible to trade assignments with one of the other graduates, I couldn't find anyone who was willing to trade an assignment to study Russian or Chinese at a university for an assignment to study Korean at a military installation. So I went to Monterey for nine months and then went on to Japan and Korea.

HAIGH: Now prior that, had you had any interest in languages?

BUZBEE: Not really. I had not studied a foreign language prior to that.

HAIGH: So that was quite a departure from your previous interests in science and mathematics.

BUZBEE: Indeed it was.

HAIGH: During your school days, you mentioned your mental arithmetic competition. Beyond that, had there been any scientific topics or hobbies you had been pursuing during your teenage years?

BUZBEE: Certainly I was always interested in science and math and did well in those. Like many teenagers of that time, I had an old car that I enjoyed putting a V-8 engine in and souping up a bit. So I was certainly mechanically inclined.

HAIGH: Did the aptitude test turn out to be right about your linguistic abilities?

BUZBEE: I guess it did. Fortunately we only had to listen and understand the language, we didn't have to speak it. Yes, I could do it well.

HAIGH: Following the linguistic training, were you sent to Korea?

BUZBEE: Indeed, I spent nine months in Japan and then nine months in Korea.

HAIGH: How did you find these experiences?

BUZBEE: Japan was delightful at the time. The dollar was very strong in those days, and so even though I was an enlisted man, I could enjoy the culture of Japan. When I was there in 1957, Korea was still recovering from the war. It was terribly shot up during the war, and in fact there was lots of poverty, so it was a much different environment than Japan.

HAIGH: Did your experiences in the Air Force change anything about the way you thought about your future career or your interests?

BUZBEE: Not really. When I was in the Air Force, I took several correspondence courses from the University of Texas at Austin and managed to get some additional credit at the entry level of college. Again, the GI Bill then funded my college education.

HAIGH: Did you enroll at the University of Texas immediately upon leaving the Air Force?

BUZBEE: No, the semester before I went into the Air Force I attended what was then called Arlington State College, now it's The University of Texas at Arlington in the Dallas-Fort Worth area. When I came back from overseas, I was stationed in Ft. Worth, so I continued taking classes out there and then when I got out of the Air Force I did one more semester full time at Arlington and then went on down to Austin.

HAIGH: I saw in what you have written that you originally planned to major in physics.

BUZBEE: Initially I majored in mechanical engineering. When I came out of the Air Force and did all the paperwork for the GI Bill, the VA (Veterans Administration) was giving aptitude tests. Sputnik had just been launched. In fact, when I arrived in the States from Korea, one of the first things I got to hear was the "beep-beep" of Sputnik. And so the VA provided a lot of encouragement to go into physics. There was a need for physicists. And I bought off on that and switched over to physics after I left the Air Force.

HAIGH: So let's just get the dates on that. What year…?

BUZBEE: I was released from the Air Force in September of 1958, then in January of 1959 went on down to Austin.

HAIGH: So at what point did you switch to physics?

BUZBEE: In September of 1958.

HAIGH: And what made you do that?

BUZBEE: Again, this aptitude test I took through the VA.

HAIGH: So it wasn't a matter of personal interest; it was an idea that you might be better at physics.

BUZBEE: Indeed, the VA was encouraging those who had the right aptitude to go into it because there was a need in the nation for more physicists.

HAIGH: So you felt at that point that the career opportunities would be better in physics?

BUZBEE: Indeed, yes.

HAIGH: And did you enjoy physics?

BUZBEE: I enjoyed classical physics very much. Modern physics I found less interesting. Fortunately, about the time I got into modern physics, I discovered computers [laughs].

HAIGH: Do you want to tell that story now?

BUZBEE: Well, the gentleman my dad worked for, the independent oil producer, his name was E.M. Howard. Quite a character. A civil engineer. He had a nephew, Robert Gregory, who was an Annapolis graduate during the war or at the end of the war. Following Annapolis he did his active duty and then he took his GI Bill and enrolled at the University of Illinois at Champaign to study numerical analysis. Of course, Illinois had the ILLIAC, at that time one of the few academic computers in the country. After he finished his Ph.D., and studied numerical linear algebra, he joined the faculty at the University of California Santa Barbara and was there several years. And lo and behold in the fall of 1959 and at the invitation of David Young, he joined the faculty of the University of Texas at Austin. Now given our mutual connections with Mr. Howard, we made sure we got acquainted. I knew a little bit about computers. They were novel and mystical.

HAIGH: At that point had you ever seen a computer?

BUZBEE: No, I had not.

HAIGH: Can you remember what you had read about them?

BUZBEE: I had read about them and it certainly piqued my interest. As I said, I wasn't all that enthusiastic about modern physics, and computing sounded like a new and interesting deal. Again, I had certain mathematical skills and I just felt like it might be a better place to be, so I went to it.

HAIGH: So your choice to switch your major to mathematics was motivated specifically by your desire to be involved with computing.

BUZBEE: Yes.

HAIGH: Before we move on to discuss your first involvement with computing, I have a few more questions generally about your experience as an undergraduate. Coming from your family background, was it an easy transition to be a student at the University of Texas Austin? Or did you feel that you had to work harder?

BUZBEE: No. Actually the first semester there I think I aced every course. I was studying classical physics at the time. No, I was very comfortable at Austin. I enjoyed it.

HAIGH: Beyond what you've already mentioned about your enjoyment of classical physics, were there any courses that you took or things you were exposed to that had a particular influence on you, other than computing?

BUZBEE: I did take geology, the usual two-semester introductory course to geology. It's a wonderful part of the country to study geology with lots of formations and outcroppings and what have you, and UT was a major university in that area, so I enjoyed that.

HAIGH: You mentioned that through Robert Gregory you were exposed to the idea that computing might be something that would be interesting. Can you talk to your first actual exposure to computing?

BUZBEE: I don't recall whether I first took a programming class and then changed majors or vice versa. But at that point in time the University had an IBM 650, the first mass produced computer. It actually had Fortran on it. We began learning to program in assembly language, using the old classical coding sheets in a very careful and methodical process for laying out and implementing the program. I think I had a certain natural skill in that area, so I enjoyed it.

HAIGH: Do you think this was a standard part of a numerical analysis course at this point?

BUZBEE: At that point in time it was. Again, Professor David Young was already in Austin. He recruited Bob Gregory (I think they knew each other in California). I don't recall whether my first numerical analysis class was taken under Gregory or Young but I certainly did take courses from the two of them.

HAIGH: So would you have been using the machines to complete course assignments?

BUZBEE: Oh yes, indeed, yes.

HAIGH: How did that access work, physically? Would you sign up for a time slot for the machine and then operate it personally?

BUZBEE: No, you submitted your card deck to an input shelf in the computer center. Actually when I first started the machine was located over at the physics building, as I recall, but again, thanks to Dr. Young's influence, a computing facility was built on campus and by the time I was in grad school the equipment was all in it. But in both cases you had an input shelf, an output shelf, key punches, and the usual array of equipment.

HAIGH: Would you punch your own cards?

BUZBEE: Oh, indeed yes.

HAIGH: Can you remember how long would elapse between dropping off the deck of cards and receiving the output?

BUZBEE: I think an hour was not at all unusual, maybe longer.

HAIGH: I think that would be a relatively fast turn around for that era. I've heard a lot of people talk about it being an overnight thing, not getting the results back until the next day.

Now, can you talk a little more about your personal reaction to the experience of programming? Was it something you found that you really enjoyed or did you see it more as a means to an end?

BUZBEE: No, I really enjoyed it and still do.

HAIGH: Do you have anything else to say about the kinds of satisfaction you get from it or anything about your personal approach?

BUZBEE: I find it challenging. Certainly formulating a program such that it executes efficiently as opposed to inefficiently—there's a number of nice challenges to that

HAIGH: Do you remember how many students would have been in the numerical analysis course at this point? Was it a big course with dozens of students or was it quite small?

BUZBEE: The graduate courses, I think we had about 8 or 10 people in the course.

HAIGH: Were you taking this graduate course while you were an undergraduate?

BUZBEE: No, I think in my senior year I concentrated on programming, and then when I went into grad school ~~is when~~ I started taking numerical analysis courses. There might have been one course as an undergraduate, but I'm not sure.

HAIGH: So you must have already been programming before you took the numerical analysis courses, in that case.

BUZBEE: I believe that's right. I think the first course was in programming as opposed to numerical analysis.

HAIGH: So as an undergrad you took programming and then when you were getting your masters, you took the numerical analysis courses.

BUZBEE: Yes. In fact, I'm not sure if there were any numerical analysis courses for undergrads at that point in time.

HAIGH: And you were inspired to take the programming courses because of your personal contact with Bob Gregory?

BUZBEE: Yes. The degree was in math—at that point in time there was no computer science degree.

HAIGH: Were you unusual at that point, among undergraduate mathematics students in having an interest in computers and programming, or was that something that many students were developing an interest in?

BUZBEE: It was still new and novel. I think the programming class might have had 20 or 25 people in it. As I recall, it was taught by Gregory. HAIGH: I understand that you went straight from your undergraduate degree into the master's program.

BUZBEE: That's correct.

HAIGH: What made you decide to stay around and do a master's?

BUZBEE: Well, I still had a year's worth of money under the GI Bill and I just thought it was a good thing to do, to get a master's degree and learn more about computing.

HAIGH: Was numerical analysis a major topic in the master's program at that point?

BUZBEE: Yes, it was focused on that completely.

HAIGH: Can you remember what kind of topics would have been included in the curriculum?

BUZBEE: Numerical solution of PDEs taught by Young was a big part of the curriculum. And Gregory's area was numerical linear algebra. I actually did my master's thesis under Gregory in numerical linear algebra.

HAIGH: Do you remember what topic you worked on for your master's thesis?

BUZBEE: Yeah, I have it right here. This was before the days of word processing. It seems amazing that you could get these things done in a reasonable amount of time by having someone type them manually. The title is "A Survey of Methods for Solving Systems in Simultaneous Linear Algebraic Equations and Inverting Matrices."

HAIGH: Did the work you did on your master's thesis have any kind of impact on how your interests developed later in your career?

BUZBEE: For sure. Having done this survey, I felt very capable and generally knowledgeable about linear algebra and numerical technology at that point in time.

HAIGH: While you were working on your master's degree, did you have a clear idea of what you wanted to do next in terms of your career?

BUZBEE: Being a good Texan, I assumed I would end up in the petroleum industry somewhere. In fact, in 1961 I had a summer job at Texaco in Houston.

HAIGH: What sort of things did you do there?

BUZBEE: Texaco -- at that time -- had an Underwood Computer in the research lab. I think only about ten of these computers were manufactured. It was a peer of the 650. So I developed some software for analyzing laboratory data during the summer when I was there. Of course, after I had been at Los Alamos for about five years, I went back to Houston and worked for Esso on 3-dimensional reservoir simulation,-- models.

HAIGH: I'll ask you about that later. So you'd had that experience of summer employment in the petroleum industry, it was what you thought you'd do, yet in the end it was not what you did when you graduated. Why was that?

BUZBEE: One of my co-students in graduate school was a fellow named Thurman Frank, who ~~is~~ was on leave from Los Alamos and he was at Austin to do a Ph.D. under Dr. Young. So naturally we ended up in class together and became fast friends. In the fall of '61, he began to lobby me to work at Los Alamos. He was a very nice fellow. He served in the Merchant Marine during World War II—just a classy guy. So based on his influence and encouragement, I visited Los Alamos over the Christmas holidays for a job interview. Los Alamos is a remote site, but it paid well with five weeks of vacation a year in contrast to the usual two weeks for other places. All of these things combined with his enthusiasm for the lab caused me to take their offer.

HAIGH: That meant moving out of Texas… was that something that was traumatic for you?

BUZBEE: No, having been in the military, I'd spent a considerable amount of time out of Texas already.

HAIGH: So you're saying compared to Korea, it was almost Texas.

BUZBEE: [laughs] Well, for sure Los Alamos was a fairly remote area at the time. But again, having grown up on a small farm it didn't bother me.

HAIGH: Presumably you had to get security clearance for that. Was that a straightforward process?

BUZBEE: While in the Air Force, I had what was known a crypto clearance. I was working with the intelligence community. So it was fairly straightforward to get the DOE clearance.

HAIGH: I understand from your materials, that your initial job was inside the weapons division.

BUZBEE: That's correct.

HAIGH: What was that like?

BUZBEE: Los Alamos was, and I hope still is, a wonderful place to work. Everyone was a staff member, i.e. all the professionals had the same job title. They were all peers and you didn't have the kind of ranking you have in, say, academe—associate professor, full professor, all that sort of stuff. Computing had already established itself as a key resource to the weapons program and to the laboratory, for that matter. Another thing that the lab was doing in those days was nuclear reactor research and so forth. I found it a very comfortable, supportive place to work. Good equipment, interesting people to work with. It was a good choice for me.

HAIGH: What projects were you working on?

BUZBEE: I was working on a one-dimensional hydrodynamics code, which was used in the initial phases of simulating a nuclear weapon detonation, how you bring the material to criticality. So this was an initial value problem, with equation-of-states for various materials and all that kind of stuff, much different than linear algebra and PDEs. So I very quickly got well-acquainted with Mr. Richtmyer's book on initial value problems. It was referred to as "the Bible" at Los Alamos.

HAIGH: So for the tape I will read the citation. The book is called *The Difference Methods for Initial Value Problems*, and the author is Robert D. Richtmyer, published by Interscience Publishers, Inc., 1957. Was the work you were doing involving writing programs for a computer?

BUZBEE: Absolutely.

HAIGH: What kind of systems did they have at that point?

BUZBEE: At that time, Los Alamos had two IBM 7090s

HAIGH: Were you working Fortran or assembly?

BUZBEE: At that point in time, almost all of the simulation codes at Los Alamos were written in assembly. Indeed, my first assignment there was a numerical integration involved in this particular code, and it was taking a huge amount of time. So my supervisor asked me take a look at this and see if I could understand why it was taking so much time. If you do a numerical integration, you step from point to point in an interval A to B. Well, instead of stepping from point i to i+1, it stepped from A all the way out to i+1 and then it would step from A to i+2. The good news is that I quickly diagnosed the problem and fixed it.

Fortran was just beginning to make its presence felt at Los Alamos, so much of the two or three years that I worked in the weapons division was spent both converting assembly language to Fortran as well as the usual enhancement of the computational processes.

HAIGH: Can you remember what your job title was?

BUZBEE: Staff member.

HAIGH: That ties in what you said about the lack of hierarchy.

BUZBEE: That's right. We were all "staff members.".

HAIGH: You were hired as a mathematician?

BUZBEE: Yes, mathematician/programmer.

HAIGH: So how was the relationship with the physicists?

BUZBEE: Quite good. Many of them programmed, many of them didn't. In the particular group that I was working with, we were all working on simulators related to nuclear weapons. In fact in our group, we had a computer. It was an IBM, a small one, I believe it was a 1620.

HAIGH: Was being directly involved in weapons simulation a topic that any of the members of the group would have agonized over in terms of morality or the possible results of their use?

BUZBEE: Well, as you probably know this is quite a story in itself. Back during the Manhattan Project, there was a great deal of agonizing over whether to drop a weapon on Hiroshima or offshore so that the Japanese could see its power rather than applying it directly. I think Truman made the final decision. The U.S. did not have a large supply of nuclear material at that time, and that was one of the factors that entered into it. Certainly, the prospect of having to invade Japan was horrendous and horrific. People were anxious to end the war. The good new is that nuclear weapons did end it very quickly and saved a lot of lives in the process.

By the ‘60s , the Soviets had their own nuclear weapons technology, and having seen what nuclear weapons could do in World War II, I think the people at Los Alamos were comfortable and at peace with themselves working on these things.

HAIGH: So if I can summarize what you said, the people within the weapons group would have thought about the implications and basically were happy with them.

BUZBEE: I don’t like the word happy…

HAIGH: Okay, comfortable.

BUZBEE: I think they had seen the reality of war; they knew what nuclear weapons were capable of. By this point in time, we had fusion weapons. The Cold War was very much under way. I think that they felt that they were being of service to the country.

HAIGH: You said that in 1965 you switched from the weapons division to the computing group. Was that part of a general reorganization or was that just a personal transition?

BUZBEE: It was a personal transition. . By that point I had gotten acquainted with many of the people in the computing group. I enjoyed working with the weaponeers, but I saw in the computing group the opportunity to do more numerical analysis.

HAIGH: So what was the distinction between the kind of work that would be carried out by the computing group and the work in the weapons division and other parts of the lab?

BUZBEE: The weapons group was developing software specifically to simulate the processes involved in detonating a weapon. Whereas in the computing group our role was primarily that of developing software tools, libraries and so forth that would be of value to all users, regardless of what particular problem they were working on.

HAIGH: And you found that your own interests were developing in the direction of these reusable general purpose programs rather than application specific tasks? Really getting deeper into the computational mathematics and further away from the physics.

BUZBEE: Yes

HAIGH: So at this relatively early point in the development of standard libraries for mathematical software, what was state of the Los Alamos libraries in 1965?

BUZBEE: Certainly all of the special functions, as they should have been, were written in assembly. It’s remarkable the ingenuity that one finds in special functions -- for example, Pade approximations. ~~:~~Also, you take advantage of the exponent in the floating point representation and so on. You know that the mantissa lies between a one-half and one. You just exploit all kinds of things to get speed into a special function.

We had some basic linear algebra capability at that point in time, but as I recall, it was fairly narrow. Again, Fortran was just emerging, and prior to Fortran, if you were in assembly language, every time a new computer came to town, you had to rewrite a lot of software.

[Tape 1 of 4, Side B]

HAIGH: So from what you've said, I understand that at that point the standard generalized codes were mostly in assembler and covered low level things like special functions, but there wasn't much in terms of a library for things like linear algebra capabilities.

BUZBEE: Again, Fortran had just come of age, so we were beginning to accumulate a small critical mass of Fortran mathematical routines.

HAIGH: Were there any routines in use at the lab that were coming from outside sources such as IBM or the SHARE organization?

BUZBEE: My impression was that a lot of the software that IBM offered with its systems actually came out of the labs, so the flow was the other direction. [Chuckles]. Well, it was in both directions. I was surprised, and pleased.

HAIGH: Do you have any impression of whether the lab was an active member of SHARE during this era?

BUZBEE: I believe it was. HAIGH: So you feel then on the technical level that the transition from assembly to Fortran made it much more feasible to try and write general purpose codes.

BUZBEE: No question.

HAIGH: Can you talk a bit about the size and composition of the computing division at this point and its relationship to the rest of the lab?

BUZBEE: Again, we had two IBM 7090s which were then subsequently upgraded to IBM 7094s. By the time I joined the computing group, they probably had the 7094s. It was hands-on super-computing in those days. If you were developing software, we had a dispatcher who managed both machines; so when you were ready to make a run, you called the dispatcher and asked for a five minute debug run. He'd tell you which machine to go to and what time. You would go to the machine a few minutes before your designated time with your deck and your tapes and so forth; mount the tapes and get ready so that when the person ahead of you finished with the machine, you were ready to run.

HAIGH: So you would personally walk up to the machine and mount the tapes?

BUZBEE: Absolutely.

HAIGH: So there was just a “dispatcher” allocating machine time. There was not much in the way of an operating system, and there also wasn’t a human operator that would handle jobs for you.

BUZBEE: At nights and on weekends there was, when we were doing long runs. Typically, each machine would only be running one program at a time at night and on the weekends.

HAIGH: Debugging runs would be done during the day, directly, and long production runs would be done at night with an operator?

BUZBEE: Yes.

HAIGH: Were the machines working 24 hours a day?

BUZBEE: Indeed.

HAIGH: As the computing group was building up a collection of reusable code, how did you go about alerting the users in other parts of the lab to the existence of new routines and persuading them they would be better off using your code rather than trying to write their own?

BUZBEE: Of course, there was no Internet or anything of that sort. As part of the user documentation package, there would be some segment on the available subroutines along with the point of contact. And if the user wanted to know more, they got in touch with that individual.

HAIGH: So there would be a kind of catalog of available routines and documentation? And that would be issued to all the people working on programming within the lab?

BUZBEE: Yes

HAIGH: And then if they wanted to use the code, they would call up and ask more about it?

BUZBEE: Yes

HAIGH: Did you have the impression that the users were taking full advantage of routines that were available or were they sometimes more attached to writing them themselves from scratch?

BUZBEE: Certainly, by the 1970s, the users were very much interested in standard packages, software that they didn’t have to write and maintain and understand—the black box, shall we say

HAIGH: And during your first stint in the computing group from ’65 to ’67?

BUZBEE: Yes. HAIGH: So basically the computing group was making these capabilities available to people who wanted them, but there wasn’t a lot of marketing or push to encourage people to use them.

BUZBEE: That’s correct.

HAIGH: Can you say what kinds of routines you were personally working on during this ’65 to ’67 period?

BUZBEE: I'm sorry, I don't recall specifically. I think it was linear algebra; there may have been some special function as well.

HAIGH: Can you remember about how many people within the computing group were working on these kinds of standard routines at that point?

BUZBEE: A small number, maybe a half a dozen at most.

HAIGH: Was there anyone there during this period whose work particularly stands out or you remember them as being very involved in this topic?

BUZBEE: Tom Jordan, who later became Deputy Leader of the computing division. He was trained as a numerical linear algebraist, and he was certainly a valued colleague in those days.

HAIGH: A follow-up question on this shift from assembly language to Fortran, just in terms of how much fun it was or the kind of satisfaction you get out of programming. I'm interested in how you experienced that personally as a programmer. Were there things that you enjoyed about assembly language programming that weren't there to the same extent with Fortran or was it something that you viewed as completely positive?

BUZBEE: Certainly I enjoyed assembly language programming, but Fortran was a lot more fun and a lot easier. You had to be careful with efficiency in Fortran. Memory was a very precious commodity in those days. And again because I was very well acquainted with Fortran coming out of Austin, in some sense that gave me a nice advantage with regard to some of my peers. I was fairly proficient in it.

HAIGH: When you were debugging the program, did using Fortran add significantly to the time it took to get your results back in terms of compilation?

BUZBEE: It didn't add to the time it took to get results back, but it could make debugging more difficult. Because with assembly language we had trace programs and all that kind of stuff that you could just literally step one instruction at a time. You couldn't do that in Fortran.

HAIGH: So at that point you didn't have any source language debugging capability?

BUZBEE: Other than the old classical debug print [laughs] and memory dumps.

HAIGH: You would really need to know your way around the machine and the assembler to make sense of that.

BUZBEE: Oh, absolutely. Yeah.

HAIGH: In 1967, you moved back to Texas and went to work for Esso. Why was that? Were you beginning to lose interest in what you were doing at the lab or was it the purely allure of Esso?

BUZBEE: Well, it was a couple of things: One was that all the family was back in Texas, and I was a bit concerned about budget trends at Los Alamos. And then secondly, I got an offer with the 3D reservoir simulation group at Esso, which had close ties to Don Peaceman, Henry Rachford and Jim Douglas --- very esteemed colleagues. . While I was in Houston that year, Jim

Douglas taught a seminar at Rice University to about seven or eight of us, in which we worked our way through Wilkinson's The Algebraic Eigenvalue Problem. And that proved to be a tremendous resource once I went back to Los Alamos.

HAIGH: That's Clarendon Press, 1965. So that would have appeared quite recently at that point.

BUZBEE: Yeah, it was new, just off the press.

HAIGH: Was it already widely known?

BUZBEE: Much of it was, or parts of it, I think, were. But certainly having all that material under one cover and being able to work from cover to cover over two semesters was great.
HAIGH: And were you implementing algorithms from the book or doing any actual programming?

BUZBEE: Doing mostly analysis. I don't think we did much computation at all.

HAIGH: Let's move back to Esso: 3-D reservoir simulators. I presume that that's trying to analyze the oil that's still in the ground.

BUZBEE: Indeed, in those days and still today, you have injection wells and, of course, the wells via which the petroleum is removed. And so the objective was to optimize the recovery of oil by looking at various configurations of the rates of injection of water or what have you and how it would push the oil to the well you were pumping from—very complex processes.

HAIGH: Was that an area where simulation was well developed at that point or was the work more speculative?

BUZBEE: It was certainly developed to the point that it was in use in the field, but again, with computers being as limited as they were in those days, this was a tremendously demanding computation.

HAIGH: So you said that you joined Esso Production Research. Can you talk about that organization, its place within Esso, and the culture, those kinds of things?

BUZBEE: The group that I was in was focused just on producing oil from the ground as opposed to the various other issues -- processing, refinement, and what have you. One of the most interesting aspects of the lab was the director who routinely asked projects to brief management every six months or so as to how things were going,. Of course, you tend to present positive results, what you've accomplished, and he invariably-- after the presentation --- would say, "Now, tell us what didn't work." And, indeed, you often learn more by looking at what didn't work [laughs]. That was a fascinating aspect of the management there. But it was a very good, very positive management, and I enjoyed working with them.

HAIGH: Was the research group large?

BUZBEE: Well, in the lab itself, there were, I guess, 200 or 300 people working there. For the reservoir simulation project, I think there were maybe a dozen of us at most.

HAIGH: How would you compare the facilities and working conditions and culture with Los Alamos?

BUZBEE: Well, at Los Alamos, I was accustomed to being able to make several debug runs a day. At Esso,-and just prior to my arrival, they had installed a new IBM 360. -For whatever reasons, they had not gotten around to offering relatively fast turnarounds for debugging. So I had a heart-to-heart with my management on that a couple of months after I arrived; I was getting a couple of runs a day, and it's very difficult to make progress at that pace. Fortunately, there was a service bureau nearby that had a Control Data 6600, and management agreed to let a few of us drive over and use that machine. We could make a dozen runs a day and this was very helpfulHAIGH: Was the slow turnaround at Esso because the debugging and production runs were going in the same queue?

BUZBEE: As I recall, there was no priority given to debug runs.

HAIGH: Would you ever have physically touched the computer at Esso?

BUZBEE: No, no.

HAIGH: So that was another difference. How about the relationship with colleagues—did you find that it was more hierarchical?

BUZBEE: Yes. Corporate America.

HAIGH: How would you describe the overall plusses and minuses of working there as opposed to working at Los Alamos?

BUZBEE: Well, I certainly enjoyed working there. I didn't enjoy the traffic and certain aspects of urban living that I had to contend with down there. So when Los Alamos in the Spring of 1968 decided to elevate the computing group to a division making it a major organizational component of the lab, one of my colleagues who I worked for at Los Alamos called me to tell me about that. He was basically hinting that there would be quite a bit of opportunity now with this change. And so combined with various frustrations of living in Houston, I decided to go back.

HAIGH: The frustrations were more to do with the place and those kinds of personal preferences than anything intellectual about the work you were in?

BUZBEE: That's correct.

HAIGH: Now before we return to Los Alamos, you mentioned that at Esso there was a 360 series machine and then also that at the service bureau you were working with a CDC 6600. Now I know that many elements of the 360's arithmetic support for computing were criticized when the machine came out. What were your personal experiences with the transition from the 7094 to the 360?

BUZBEE: JCL. I used to contend that you should have been able to get a Ph.D. in 360 JCL. [laughs]. It was horrendous. It was assembly language in which you communicated with the operating system.

HAIGH: So when you submitted your job, would you punch a card with JCL instructions and put it in with the deck…?

BUZBEE: One punched several cards, as I recall. [laughs]

HAIGH: Was there anything in terms of the numerical properties of the machine that made you adjust the way you were working?

BUZBEE: It had less precision. As I recall, it was a 32 bit machine, so it had a 4 byte word: -- one byte was exponent and three bytes were mantissa. So we often used double precision.
HAIGH: Was all the programming at Esso being done in Fortran?

BUZBEE: Yes.

HAIGH: Can you remember anything about the state of the Esso mathematical libraries at that point?

BUZBEE: No, I don't recall any great deficiency. Again, in dealing with the PDEs in reservoir simulation, there wasn't much linear algebra or things of that sort. But we did use standard tri-diagonal solvers that were tailored to alternating-direction techniques, special functions, etc.

HAIGH: Was it about a year after you left that you returned to Los Alamos?

BUZBEE: Yep.

HAIGH: Now, what practical impact did the elevation of the computing group to divisional status have?

BUZBEE: First of all, it confirmed that lab management had a growing appreciation of the value of computing. This reflected the growing utilization of numerical simulation in the design of nuclear weapons. Field testing a nuclear weapon is a very expensive and time-consuming process. Whereas with numerical simulation, you can do in days what takes months in the field.

HAIGH: So that allayed the concerns you had had previously about future funding?

BUZBEE: Indeed. Yeah. It was clear that the Lab and the Department of Energy appreciated the potential role of computing and was prepared to make the appropriate investments in it.

HAIGH: Did the elevation mean more money or better facilities directly for the computing people?

BUZBEE: I believe it did. The elevation made the computing organization comparable to the weapons division whereas, previously, computing had just been a few small groups in the theoretical division.

HAIGH: So were there just those three divisions?

BUZBEE: Oh no, there were a dozen or so divisions at the time, and many of them used the computing facility. Fusion energy for example was a big user.

HAIGH: You mentioned that "Roger Lazarus of MANIAC Computer Fame" was the leader of the new division.

BUZBEE: Yes. I should have said the "famed MANIAC computer" [laughs]. By the way, this is an aside. Given your interest in the history of technology, if you'd like a copy of this [waves paper], I'll mail you one.

HAIGH: Thanks, so this is a short paper from 1978, "Computing at LASL in the 1940s and '50s"?

BUZBEE: Yes, and there is an article in there by Roger.

HAIGH: So this was an internal technical report?

BUZBEE: No, actually back in the '60s and '70s there was the National Computer Conferences, NCC I believe it was called?

HAIGH: Oh, yes. Organized by AFIPS. That was 1978, and it's got a code LA6943H.

BUZBEE: In '78, the national conference was held in Dallas, and I was invited to organize a session on the history of computing, so I recruited these four guys to give papers. Then we wrote them up and put them in a report.

HAIGH: How would you describe Lazarus?

BUZBEE: As I recall, he was in Los Alamos as part of the Manhattan Project; a very astute physicist and a nice guy. I liked Roger.

HAIGH: And had his personal interest turned more from physics to computation?

BUZBEE: Yeah, the MANIAC Computer, which he and Metropolis and others… and there was a series of them as I recall. I never worked with the MANIAC, but they did some amazing things with those machines back in the '40s and '50s. One of the things you'll see in this report that is an article by Mark Wells. They had a two-dimensional programming language called Modcap as opposed to the one-dimensional Fortran that we've all come to love. So you could write program code that looked a lot like a mathematical text,--- subscripts, superscripts, and what have you—very innovative.

HAIGH: When you returned to Los Alamos, was basically it to the same job that you had left or was there a difference in your new responsibilities?

BUZBEE: I was in a new group that included responsibility for mathematical software. Also, my colleague, Tom Jordan, was Deputy Leader. Tom wanted to hear about what I had done in Houston, and we agreed fairly quickly that I should concentrate on implementing much of what was in Wilkinson's text for the Los Alamos library. So I did that.

HAIGH: You wrote here that you were a member of the applications group. Is that the name that you mentioned a second ago or was it something else…?

BUZBEE: Let's see, in 1974, I became head of the applications group. I don't recall the name of the group that I was in prior to that, but it had a lot of interactions with users. So not only was I writing software, but when users came to the division with questions about any particular numerical techniques, they often found their way into my office.

HAIGH: So in a sense you were serving as an internal consultant?

BUZBEE: Yeah, good word.

HAIGH: So was that a change from how it had been in 1965 and 1966?

BUZBEE: Yeah, I think the division was putting a lot more emphasis on outreach and support to users than had been the case in the older organization.

HAIGH: So you were acting more to push the results of your work out to the users instead of letting them come to you.

BUZBEE: I'll just say, we were available to help, and that fact, as well, was certainly publicized.

HAIGH: You said that Tom Jordan had encouraged you to work on the algorithms from Wilkinson's book. So obviously these were viewed as being of potential value even before good implementations had been produced. At that point, what were their main advantages seen as being?

BUZBEE: Wilkinson had done a very careful error analysis, stability analysis, so we had a lot of confidence in those.

HAIGH: Had these algorithms been widely implemented prior to that?

BUZBEE: They were in progress. I'm trying to remember the dates that EISPACK and LINPACK were being built at Argonne.

HAIGH: EISPACK was definitely before LINPACK. And my impression is very early 1970s, so if you started this in 1968 that might have been a little before that project got underway.[1]

BUZBEE: Yeah, we were probably either parallel or a little ahead of it.

HAIGH: How would you distinguish between the work that you were doing and the work the EISPACK team finished up doing?

BUZBEE: There was a lot of commonality, for sure. Eventually we adopted EISPACK and LINPACK.

HAIGH: So your routines would have covered basically the same ground and used for the same purposes?

[1] The first version of EISPACK appears to have been released in 1972.

BUZBEE: I doubt that our routines had all of the capabilities that EISPACK had. HAIGH: What kind of code was included in Wilkinson's book?

BUZBEE: I don't think there was any code in Wilkinson's book, just the algorithms. Again, in those days we didn't have the Internet, and it was good to have someone who knew the software so that when the user got into difficulty with it, you could expedite resolving what the problem was. If you were using a piece of software originated from far away, that could be a much lengthier process, a more tedious process.

HAIGH: I'm just checking some details from my earlier interview with Cleve Moler, and that's been kind of the impression I have, that they used as a starting point for their project the Wilkinson and Reinsch *Handbook for Automatic Commutation*, *Volume Two*, *Linear Algebra,* which as I understand, it did include code in Algol. [Wilkinson, J. H. and C. Reinsch (1971). Handbook for Automatic Computation, volume 2: Linear Algebra. New York City, Springer-Verlag] And they were re-implementing in Fortran. So you were working independently of this effort, using Wilkinson's earlier book?

BUZBEE: I just worked directly from the algorithms, yes.

HAIGH: At that point, were you aware of any other people who were working to implement these algorithms?

BUZBEE: Cleve offered summer seminars at the University of Michigan and, because of this work, I attended. I went to a couple of them and, in fact, it was there that I discovered what was going on at Argonne. I had not been aware of it before. So then, indeed, I began visiting Argonne, and we began collaborating and what have you.

HAIGH: Can you talk in more detail about the summer seminars and your experiences there?

BUZBEE: Cleve was a very effective mentor, and I was glad to go.

HAIGH: Can you remember the atmosphere there, the kinds of topics that were covered, any relationships you developed…?

BUZBEE: Well, of course, it was through those summer seminars that I then got acquainted with the people at Argonne, including Jim Pool who later went to DOE headquarters and became a program manager.

There are two interesting aspects to that. Jack Worlton of Los Alamos was an astute observer of trends in technology and his research showed a diminishing rate of growth in the speed of supercomputers by the mid-1970s. And that caused us first to get very interested in vector computing, but then shortly thereafter get very interested in parallel computing. Once Jim Pool arrived at DOE headquarters, he got interested in the subject and managed to expand his program to include funding for research on vector and parallel computing. So the point is -- going to Michigan and eventually getting acquainted with Jim Pool paid off nicely.

HAIGH: How would you describe your impressions of Jim Pool at that point? I understand he was serving as Acting Divisional Director to Argonne at a quite young age.

BUZBEE: I found Jim to be very effective at Argonne and at DOE as well and at Caltech.

HAIGH: At the summer school, beyond these relationships that developed with the Argonne people, do you remember anything about the kinds of topics that were covered there or any interactions you might have had with other people?

BUZBEE: in addition to Jim Pool, I met a number of interesting people at Argonne, Jim Cody and others, including Jack Dongarra, who at the time hadn't done his Ph.D. After Cleve left Michigan, he joined the faculty at the University of New Mexico. Shortly thereafter Jack left Argonne and went to Albuquerque to do his Ph.D. under Cleve. So naturally we offered Jack a part-time job at Los Alamos, which he took. He'd fly up a couple of days a week and work with Tom Jordan and others on linear algebra and what have you. To some extent, I take credit for having contributed to Jack's education [laughs].

HAIGH: That would have been the early 1980's, wouldn't it?

BUZBEE: Yeah, I think so.

HAIGH: What was Jack Dongarra like in those days?

BUZBEE: Oh, he was just about the same as he is today: Lots of energy and very productive.

[Tape 2 of 4, Side A]

HAIGH: This is the beginning of Session Two in the afternoon of Friday, April 8, 2005, at Dr. Buzbee's home in Westminster, Colorado.

To pick up where we left off, the main project you had been involved in after you returned to Los Alamos, was your own implementation of a package of linear algebra routines based on Jim Wilkinson's work. We were discussing your involvement in the Michigan summer school with Cleve Moler and the Argonne people. So I wonder if we could now return to talk a little bit more about the project itself. Did you finish up producing a full set of usable routines?

BUZBEE: Indeed, I did.

HAIGH: Did this project have any kind of name?

BUZBEE: No, not really. The other major activity that began shortly after the return from Houston was the Fast Poisson Solver work. It was a collaboration with Golub and others.

HAIGH: So that was where more of your energy finished up going?

BUZBEE: Oh, indeed. Yeah.

HAIGH: So just before we talk about that, did these routines make their way into the Los Alamos software library?

BUZBEE: Oh, yeah. Yes.

HAIGH: Were they ever used or distributed outside the lab?

BUZBEE: Occasionally. , As I recall, the routines were available both from the system library or from the division software office—you could get the hard copy, you could get a deck if you wanted it.

HAIGH: But you don't think they were widely used outside the lab?

BUZBEE: No, I think they were occasionally incorporated into models that people in the lab shared with their colleagues and other organizations.

HAIGH: Were these routines ever described in publications?

BUZBEE: I don't think so. HAIGH: So to summarize: the project was completed successfully, the routines were added to the lab's internal library, the code was used in some specific applications that may have been distributed externally, but beyond that, it didn't make much of a splash and was eventually replaced by the Argonne EISPACK code.

BUZBEE: Well, I think some of these routines did find their way into the SLATEC library, but at some point in time, yes, they were displaced by Argonne stuff.

HAIGH: And, just before we move on from this topic, is there anything you can recall (other than the fact that they were implementing Wilkinson's algorithms) that you think was novel or interesting or that you're proud of about that piece of software?

BUZBEE: They were highly regarded within the Los Alamos user community. They were widely used within the lab.

HAIGH: Did you write them for the CDC machine?

BUZBEE: Indeed, by the time I returned from Houston, Los Alamos had 6600s across the board, and of course, we went from that to 7600s to Cray-1s.

HAIGH: At this point, was the lab basically just using one platform at a time?

BUZBEE: No, typically when a new machine came in, the older ones would be on board for a while, so there'd be an overlap between the two.

HAIGH: So the older machines would be phased out, but there was never a case where there would be two platforms both being actively encouraged at the same time? Really you tried to move from one standard to another, albeit with a period of transition between them?

BUZBEE: Certainly, that decision was up to the users, but by and large they would migrate to the newer equipment when it was proper to do so.

HAIGH: So the portability of code to a large number of platforms wasn't as much of a priority for the internal Los Alamos library.

BUZBEE: No, eventually it did become that because eventually the VAX, for example, begin to appear throughout the lab, and routines of this sort were certainly used by people on most machines.

HAIGH: So that was something that would have become more of an issue in the late 70s?

BUZBEE: Yes

HAIGH: Let's discuss the paper that you alluded to a moment ago and the work that led to it. ["On Direct Methods for Solving Poisson's Equation," by B. L. Buzbee, G. H. Golub and C. W. Nielson, and published in Society for Industrial and Applied Mathematics (SIAM), Journal Numer. Anal. 7, 1970, pp. 627-656.] For this you collaborated with Gene Golub and C.W. Neilson. What were their positions at that time?

BUZBEE: Clair Nielson was a scientist in the fusion energy division, and Gene Golub was a consultant to the lab. After returning from Houston, one of the scientists in the fusion energy division, whose name was Ralph Lewis, was using one of the Eigen Analysis routines. So he and I had become pretty close colleagues. Clair was a colleague of Ralph's and, about 1968 or '69, had this very fast, direct technique for solving Poisson's equation on a rectangle. The routine had been written by Oscar Buneman at the Stanford Linear Accelerator and Buneman didn't document it. So Clair and others at Los Alamos had picked this routine up and tested it and found that indeed it was extremely fast. Since there was no documentation for it, Ralph suggested that Clair stop by and quiz me about it, i.e. what algorithm did it use?

HAIGH: So what you had in this case was a piece of code, but not any kind of mathematical description of how it worked.

BUZBEE: Yes. It was a very ingeniously written piece of code at that. I mean, you talk about compact!

HAIGH: Was this in Fortran?

BUZBEE: As a matter of fact it was. So Clair came by, and we weren't able to decipher this thing. It was non-iterative and was MUCH FASTER than widely used iterative techniques at that time, e.g. over-relaxation. Fortunately, about a month later Gene made his first visit to Los Alamos, and I got on his list of people to talk to. ~~; so~~ I arranged for Clair to come over, and then the three of us discussed this mysterious piece of software that Buneman had produced. It was clearly solving a hierarchy of tridiagonal systems. Gene surmised that this was some sort of cyclic odd-even reduction, which he had some familiarity with.

HAIGH: Can you say that again?

BUZBEE: Cyclic odd-even reduction. You start out with bunch of tri-diagonals and you reduce them down to a single tri-diagonal system, solve it, and expand back out. HAIGH: And was Golub already a very prominent figure by this point?

BUZBEE: Indeed, he was. So over the next couple of months, we worked out what we thought were the details of the algorithm and wrote it up, i.e. our algorithm was similar to, but different from what was in Buneman's subroutine . I decided that -- just to be sure -- I would program the odd-even reduction algorithm we had developed and verify that it would do what we intended. It turned out to be catastrophically unstable, so that left us scratching our heads. By that point in time, given what we had done with odd-even reduction, I was able then to decipher the Buneman code, write out the equation, prove that the so-called Buneman Stabilization of odd-even reduction was in fact very reliable and very stable. I programmed it, confirmed it numerically, and we incorporated that into the manuscript. So we were all delighted that we didn't rush to publication before we actually got odd-even reduction worked out, that we went ahead and took our time to test it and eventually unraveled and documented Buneman's scheme.

HAIGH: Looking at your résumé, it appears that that was the first paper you that you had published.

BUZBEE: That's correct.

HAIGH: So do you think that was a difference between the lab environment and an academic environment: less pressure to publish things?

BUZBEE: There was not a great deal of pressure at Los Alamos to publish. It was certainly an important notch on your gun if you did, but unlike academia, it was not "a publish or perish environment"

HAIGH: It seems that over the next few years after that you had a number of other publications on what appear to be related areas.

BUZBEE: Indeed. Colleagues at Stanford immediately got interested in Buneman-type schemes, and likewise, at Los Alamos, e.g. Fred Dorr who was a graduate of Stanford and a colleague at Los Alamos. So we sort of all came together and began looking at various ways that we could apply these new things. That led to several papers over the next two or three years. For example, Dorr and I wrote a paper using these techniques on irregular regions. [Buzbee, B. L., and F. W. Dorr, "The Direct Solution of the Biharmonic Equation in Rectangular Regions and the Poisson Equation on Irregular Regions," SIAM J. Numer. Anal., 11, 1974, pp. 753-763]. And then I did my dissertation. Gene encouraged me to get a Ph.D. after all of this. I did so at the University of New Mexico working with Al Carasso —his specialty was inverse parabolic problems. Al studied under Seymour Parter at the University of Wisconsin. In my dissertation we applied direct methods to the inverse parabolic problem and got some nice results out of it.

Then, by that point we were getting increasingly interested in parallel computing at Los Alamos, and I saw some opportunities with Hockney's scheme for parallel computing, so I wrote a paper on that. And then we used the same techniques on the biharmonic. [Buzbee, B. L., "A Fast Poisson Solver Amenable to Parallel Computation," IEEE Trans. on Computers, C22, 1973, pp. 793-796.]. So there's a series of about a half dozen papers that were written over a period of four years or so.

HAIGH: And then that series of publications seems to dry up at that point. Was that with your transition into a more managerial role?

BUZBEE: Yes.

HAIGH: All right, so we'll return and talk about that aspect later. Can you say a bit more about your Ph.D.? You received it in 1972. At what point did you enroll and start work?

BUZBEE: The University of New Mexico had a graduate center at Los Alamos, for the most part offering night classes. Faculty flew up from Albuquerque and taught at night. Even before going to Houston, I had taken a couple of graduate classes at Los Alamos and took more when I got back. So all I needed was to satisfy the residency requirements, which I could do in a single year.

HAIGH: So did you physically leave the lab for one year?

BUZBEE: I worked half time. Lab management was very supportive. Los Alamos has a long history of encouraging graduate study among its employees.

HAIGH: So the selection of the University of New Mexico was because of this special relationship that meant it was much easier to study there than anywhere else?

BUZBEE: I had a family to support. By commuting back and forth to Albuquerque three days a week (or maybe it was two days, I've forgotten) I could continue working half time and pursue the Ph.D..

HAIGH: How would you describe the computer science and applied mathematics community at the University of New Mexico at that point?

BUZBEE: Good. They had a couple of fairly prominent mathematicians there. . And of course, the university was in close affiliation with both Sandia and Los Alamos, and that helped a lot.

HAIGH: Was your motivation to get your Ph.D.? And you mentioned that Gene Golub had encouraged you. Were you doing it because you thought it would be interesting? Or was it something that would help you directly in your career at Los Alamos?

BUZBEE: It was my expectation that the Ph.D. would open doors, and I was right.

HAIGH: So without the Ph.D. it would be hard to rise into a managerial position within the lab. Is that correct?

BUZBEE: No, I wouldn't say that. But, certainly professionally, it has always been to my advantage to have a Ph.D..

HAIGH: When was it that you had your first sabbatical? Was that during this period?

BUZBEE: Well, in fact, I went to Sweden in the Fall of 1973. I got the Ph.D. in the August of '72.

Gene had (and probably still has) a worldwide network of colleagues, and typically, he visited them with some frequency. So he was in Sweden in the Fall of '72-at the Chalmers Institute of Technology at Gothenburg, and one of his colleagues wanted to take a leave of absence for a year. But the Swedish system is such that in order for him to do so, he had to find his own replacement. So with that information at hand, Gene contacted me and asked if I might be interested. Los Alamos management was very supportive, so I did.

HAIGH: Did you assume his teaching responsibilities?

BUZBEE: I did teach while I was in Sweden. That was my primary responsibility.

HAIGH: What were you teaching?

BUZBEE: I taught special functions; I taught a seminar focused on high performance computing. And I taught a seminar on direct methods for PDEs.

HAIGH: And did you enjoy teaching?

BUZBEE: Indeed, I enjoyed that year. It gave me a lot of time to read as well. HAIGH: What did you like about teaching?

BUZBEE: In this particular case, take for example the seminar on high-performance computing, I was communicating information about high-performance computing to people who were not well acquainted with the subject, and I enjoyed it.

HAIGH: How were the students?

BUZBEE: In Sweden, students have to speak English to get admitted to college, so I taught in English. Good students!

HAIGH: Had that been your first visit to Europe?

BUZBEE: I had been in Europe earlier in the summer in ’73. That was my first trip. I went to a conference in Marseilles and then went back in August to Gothenburg.

HAIGH: Were there any relationships or ideas that you developed during this period that enhanced your career later?

BUZBEE: I mentioned I had time to read and that included some of Gene Amdahl’s publications and the so called Amdahl’s Law, which basically says that if you have a two state machine ~~for~~ in which one state is fast and the other is slow, then the slow component will tend to dominate performance. And that can be formulated mathematically. So when I came back from Sweden I did so, and it helped me and others understand how important it is, for example, in vector machines to have short start-ups and high-bandwidth to memory and things of that sort.

HAIGH: Now, just before we move on from that. as a general question, having had this experience as a visiting professor, were you tempted at any point in your career to switch to an academic position?

BUZBEE: Only after I got into retirement, but I found that I really couldn’t balance that with consulting, so I’ve never done it.

HAIGH: On your return to Los Alamos, it must have quite soon afterwards that you became leader of the Computer Research and Applications group within the Computing Division.

BUZBEE: In fact, I got that title before I went to Sweden. Tom Jordan served as acting group leader while I was away, so when I came home, I came home to that job.

HAIGH: Was that a newly formed group? Or was that something were you were taking over from someone else?

BUZBEE: The division was reorganized in the Summer of 1973, a new division director was appointed, and he created this group, which had the responsibility for math software and for a programmer-for-hire service to the lab. We started out with about ten people, and over the next six years, it grew to about 35 people and expanded to include various components of research.

HAIGH: What led to this expansion in that area? Were there new kinds of challenges, new responsibilities?

BUZBEE: We were becoming increasingly concerned with the fact that the growth rate and the speed of the single processor was diminishing. And by that point in time, and again, as I mentioned in some of the material I gave you, people were really beginning to understand that computing and modeling or simulations were the third leg of science. This is particularly valuable when you're dealing with processes such as nuclear energy. It's not only expensive and time-consuming to perform experiments; there are also environmental issues and what have you. So we were really concerned about the possibility that computers might be approaching some sort of maximum performance level. That led us first of all to get very interested in vector computing, but then shortly thereafter that our best hope lay in parallel computing. So we began to put lots of priority and lots of advocacy into parallel computing.

HAIGH: Had this shift of emphasis towards vector and parallel computing been foreseen with the creation of the group, or was it just an accident—the group had happened to be created shortly before these became major issues?

BUZBEE: These things became major issues after the group was created.

HAIGH: But it was these concerns that led to the rapid expansion and size of the group?

BUZBEE: That was one of the factors that led to it, yes. The programmer-for-hire service grew nicely as well.

HAIGH: This period, 1974 to 1980, would you say that there were (other than the SLATEC project, which we'll talk about after this) any important developments in the Los Alamos software library? Perhaps the sources of routines for it or the way it was used or supported? Or was it just a continuation of what had been happening before?

BUZBEE: A continuation.

HAIGH: Was it during this period that code from the LINPACK and EISPACK projects would have entered use within Los Alamos?

BUZBEE: Probably so. I can't say with certainty, but probably so.

HAIGH: Do you know if any use was made within the lab of the commercial NAG or IMSL libraries in the ‘70s?

BUZBEE: Once I became deputy division leader, we began producing an annual development plan, which I have copies of here. Here are some excerpts from in ‘84 and ’85, and in particular, the topic of mathematical libraries. We had this problem that we were often using machines that had just entered the marketplace. However, by this time, we also had a sizeable local network of VAX class machines and other mini-computers. So we wanted a lab-wide license as opposed to single-machine licenses.

HAIGH: Now, the dates on here are in the mid-1980s. You think that it would only be in the 1980s that those commercial, externally produced libraries would really have introduced within the lab.

BUZBEE: IMSL, as I recall, was available earlier than that.

HAIGH: I think that is about right, and it was rapidly ported to several platforms.

BUZBEE: We probably had that thing, yeah. We had a colleague who worked with them, so we probably had it in house. I don’t know if it was our first line of defense, but we probably had it.

HAIGH: So your impression is it would have been there, but it wouldn’t have been the primary source of routines for most users.

BUZBEE: Correct.

HAIGH: As leader of the computer research and applications group, what were your personal responsibilities?

BUZBEE: Eventually, the group assumed responsibility for benchmarking and evaluating computers that were of potential interest to the lab. A Cray-1 arrived in Los Alamos in 1976, and it came there for evaluation. It wasn’t purchased. Our group was heavily involved in the evaluation and testing of it, and that led us then to take responsibility for benchmarking using a suite of codes we put together.

HAIGH: So this responsibility for benchmarking was added to the responsibilities of maintenance of mathematical software.

BUZBEE: And also managing the pool of programmers. And then, certainly by the time the Cray-1 arrived, we were very much interested in parallel computing and began to advocate it. And, one of the benefits of Jim Pool going to Washington in around 1978 was that he began to fund research in parallel computing. We competed for and got some of that funding, and it was a tremendous help.

HAIGH: All right, so I’ll ask you more about that topic later as well, then. You said before that one of the attractions of the lab was that it was not very hierarchical. So what did it mean to be a manager in that environment? Was it more like an academic head of department where you can suggest things but you can’t force people around so much, or was it more like a corporate manager where if people get fired if the don’t do what you say?

BUZBEE: It was more of a corporate manager. For example, part of my responsibility was to write annual reviews of all the people in the group; I was the one who proposed raises for them; if their performance was below par, it was my responsibility to document that and lay out a process by which they could remedy the situation. That's management as you typically know it.

HAIGH: Did you enjoy that kind of managerial work?

BUZBEE: I did. At the same time, one of the nice things about the job was that I had a fair amount of visibility both within the lab as well as within the community. At that point in time, the high-performance computing community was relatively small. Supercomputers, and in particular parallel computers, were esoteric at best, and so it was a small community. Even though people were beginning to appreciate the value of high-performance computing, we were very much involved in advocating that point of view.

HAIGH: So you became the representative of Los Alamos to the broader high-performance commuting community?

BUZBEE: I became one of several [laughs]. Jack Worlton, for sure, was a very articulate spokesman for us. Bob Ewald, as well.

HAIGH: You have mentioned you were involved in evaluating the Cray-1 with serial number one, and that the Cray-1 with serial number four was purchased by the lab. Do you think that when the Cray team was designing the computer, they would have had Los Alamos in mind as a key customer?

BUZBEE: I think so. We had been very happy with the CDC 6600 and the 7600. The deal to bring serial one to Los Alamos came about partly because it was the first machine that looked like it would perform well on short vectors, which Los Alamos needed. Most of our applications did not have long vectors. Short means a hundred or less in dimension.

HAIGH: Was that primarily for these weapons simulation applications?

BUZBEE: That was just the nature of the models that had been developed. HAIGH: Did the machine live up to your expectations with its performance on these short vectors?

BUZBEE: Indeed, it did. One of the things that Seymour discovered on the serial one was that he needed SECDED in the memory.

HAIGH: He needed a what?

BUZBEE: Parity checking on memory, called SECDED.

HAIGH: In Unison: Single Error Correction Dual Error Detection.

BUZBEE: Yes, thank you. So at the point in time that that was discovered, serial two was already being built. Seymour cannibalized it and went ahead and built serial three. In the meantime, NCAR had obtained cash for a Cray machine, and he needed the cash so NCAT got serial three, and Los Alamos got serial four.

HAIGH: That's "cache" as in memory, not as in money?

BUZBEE: No, that's cash as in money. NCAR wrote him a check --- that paid off all of his debts, put him in the black [laughs] with that one machine.

HAIGH: Obviously Cray himself was coming from having designed these CDC machines. From your point of view as a user, was the Cray-1 just a continuation of that high performance scientific computing kind of family of machines? Or was it making a whole new kind of niche that was much more specialized than the earlier CDC machines had been?

BUZBEE: For Seymour, throughout his career, simplicity was the essence of his design. One of the neat things about the 6600 relative to the 7094~~:~~ was that the 6600 only had 64 instructions. The 7094 had 300 or so instructions. Seymour's machines were easy to get your brain around, to comprehend. And certainly the Cray-1 was in that category. At the same time vector was a new capability. We had in the 7600 pipelining, and in fact, Livermore had developed a library called Stacklib that created virtual vector computation on the 7600. That had simply moved into hardware on the Cray-1.

HAIGH: How difficult was it to get your existing code moved over to the Cray?

BUZBEE: For many of the applications, it was a straightforward process. Monte Carlo, on the other hand, took some considerable effort; particle and cell took some reformulation, so it depended on the application.

[Tape 2 of 4, Side B]

HAIGH: Chronologically, do you think it would make more sense now to talk about the SLATEC project or then to move on to discuss parallel computing?

BUZBEE: I suggest we deal with SLATEC and come back to parallel computing.

HAIGH: So the main source of information I've been able to find about this project is your article, The SLATC Common Mathematical Library, in Cowell's book, *Sources and Development of Mathematical Software*, which gives a fairly thorough explanation of the scope and motivations behind the project. So I wonder if you could begin by discussing what the general state of relations had been between the different national labs prior to this project. I understand that some of the labs considered other labs to be competitors and that the sharing of software was not widely practiced.

BUZBEE: Up until the mid to late '70s, for software that was in any sense vital you really wanted to have someone on your staff who was knowledgeable because of communication issues. If you had to depend on someone in another organization, then clearly someone without a Q clearance was even a greater complication.

There was competition between Los Alamos and Livermore in weapons design. That was intended—that was why Livermore was created, so there would be competitive designs. They competed against each other in a variety of ways, and I think that was a healthy. But I don't recall competition in software. For example, on the Cray-1, Los Alamos started out to write an interactive operating system for the Cray-1, but that proved to be a Herculean task. So once Livermore had converted its LTSS time-sharing system to the Cray-1, Los Alamos adopted it. Why? Because we wanted compatibility with Livermore.

HAIGH: So you think that feeling of competition between the labs on the weapons design level wouldn't have filtered through culturally to the people in the computer groups of the different labs?

BUZBEE: I don't think so. Mathematical software engineers were in short supply; the computing division managements at Los Alamos and Sandia and others decided to collaborate in a number of areas. Once that decision was made, it was quite natural to do mathematical software.

HAIGH: You had mentioned this reluctance to depend on routines that had been written by people who weren't working directly for that specific lab.

BUZBEE: Having vital software and not having someone expert in it was a concern. Whether they wrote it or not is another subject, but we needed people who were very knowledgeable about it.

HAIGH: Vital software in this sense, would that include operating systems, mathematical libraries, utility routines, in general?

BUZBEE: Well, the labs always had vendor representatives on site in operating systems and in compilers.

HAIGH: Would those people have security clearance?

BUZBEE: Yes, they did. When there were areas that were vital to us, such as compilers, and operating systems, we had the vendor's people sitting right beside us.

HAIGH: So the labs were relying on externally produced compilers before they were willing to rely on externally produced mathematical routines?

BUZBEE: But they had experts in those areas on site.

HAIGH: Okay. So was the concern about relying on off-site people because of the physical difficulty in communicating with them….

BUZBEE: The length of time it would take to get a problem solved.

HAIGH: But would it be a function of the difficulty of doing it by telephone or the fact that if they weren't on your staff they might just have other things they would have to deal with before they could help you?

BUZBEE: Lots of possibilities. First of all, they need to see the source code; it may or not be classified. Second, how long does it take to get it to them? HAIGH: At the beginning of your paper, it mentions that the SLATECH confederation was set up in 1974 and had a general mission, beyond just mathematical software: to foster the exchange of technical information among the three computing departments. Before we talk about this specific work on the shared mathematical library, can you give some examples of other topics the consortium might have been involved in fostering exchange of?

BUZBEE: There may have been an exchange of data storage technology, probably in the statistical area. Also in the area of help desk, user support…I think there was probably a great deal of exchange there.

HAIGH: Can you remember whose idea it was to create the consortium?

BUZBEE: No, I'm sorry. I don't.

HAIGH: So the name was pronounced "slay-tech," was that a kind of dark humor? "Slaying" being what one does with a weapon, and these being weapons labs?

BUZBEE: It was Sandia-Los Alamos Technical Exchange Committee; it was just an acronym. [Smiling]

HAIGH: It also says in the article in Cowell's book that a common mathematical library sub-committee was created early on, and in 1975 the sub-committee considered the development of a common mathematical library, but rejected it. It says each participating site had an active math library project, and the objectives of these projects were significantly different. Can you remember anything else about the arguments that might have been made at that time against a common library there?

BUZBEE: As you see here, Sandia had a smaller library, thoroughly documented, whereas we had a relatively large library with brief documentation. I think it was just simply that at that point in time no one was ready to take the risk.

HAIGH: You just said that people might not want to take the risk. What was the risk?

BUZBEE: Well, it's a new idea. And given the importance of math software at that point in time, you wanted to have a high degree of confidence before you plunged into it.

HAIGH: So the risk was that people might be giving up reliable routines for unreliable routines?

BUZBEE: Yeah, there's a changeover, surprises that come with changeover. How many problems would you encounter when you changed over?

HAIGH: So the risk was not so much to that the new routines might not be good mathematically, but that the period of transition might bring disruption and introduce variances in application software.

BUZBEE: The learning curve. Yes.

HAIGH: The risk would have basically have been the organizational risk of transitioning to some unknown software?

BUZBEE: Yes. Anytime you go to new technology, there's risk.

HAIGH: Were you a member of this common math library subcommittee upon its foundation, which seems from the article to have been in 1974 or ’75?

BUZBEE: Yes.

HAIGH: Can you remember who the other founding members were?

BUZBEE: I believe that Mel Scott was on it from Sandia, Ed Oliver might have been from the Air Force Weapons Lab, and there may have been other people from Los Alamos. At that time, I was in management, so I probably had to have someone at my side.

HAIGH: So in 1977, just two years later, the shared library idea was reexamined, and this time it was adopted. What had changed in that two-year period? Was this connected with the rise of the vector machines?

BUZBEE: For sure, they were new on the scene. As we note here on page 303, by 1977, no single lab had sufficient breadth of expertise to cover all the bases we needed to cover in a math library. Also (Item four) there was a lot of free software --- LINPACK, EISPACK, and so forth, and we all had to deal with integrating that on to our machines. And it was a growing trend to share models amongst the labs.

HAIGH: Those reasons are all listed in the paper. Can you remember anything about which of those might have seemed the most important to you personally at the time?

BUZBEE: Well, I think number three and number six were.

HAIGH: Those are “resource sharing” and “the limitation on commercial software because of proprietary restrictions.”

BUZBEE: Actually, number one, number three, and number six.

HAIGH: Okay. Those are portability, resource sharing and proprietary restrictions. Now, as the subcommittee deliberated and came to the decision that the time was now appropriate to launch this effort, do you remember anything about your own personal contribution to the discussion, or about any points that remained contentious?

BUZBEE: No, I don’t remember any points of contention. However, I’m pretty sure by this point in time, ’77 to ’78, I had handed off primary technical responsibility to someone else.

HAIGH: Who was that?

BUZBEE: Probably Tom Jordan.

HAIGH: One of the issues that is apparent in the paper is this issue of attempting to standardize the specifications of the different sites in terms of coding and documentation practices. So do you remember how well specified those needs have been internally within Los Alamos?

BUZBEE: I think by that point in time, we were emphasizing internal standards for source code and documentation, yes.

HAIGH: Do you think that was something that had become more important in the 1970s, or had it been considered important all along?

BUZBEE: Documentation had obviously been important from the beginning. There was always, even in the '60s, a mandate that the source listing of a subroutine contain a discussion of the arguments to the routine. By the '70s, the source listing might include references to publications, etc.

HAIGH: How about standards for the code itself. For SLATEC they're explained in the appendix to the article. Here we are: naming, orderings of routine, prologue, format… Do remember whether those were rigorously specified within Los Alamos prior to this project?

BUZBEE: They were strongly encouraged, particularly our help desk people advocated this kind of stuff, though I doubt that we would have rejected a subroutine for the lack of it. By the mid - 1970's, before any subroutine was incorporated into the library, it was evaluated by two or three other people, people other than the author.

HAIGH: A kind of peer review program.

BUZBEE: And particularly, the help desk had at least one of those reviewers.

HAIGH: In this evaluation, would they have considered its adherence to coding standards?

BUZBEE: Well, we had certain standards. I don't think we were rigid in those, but we wanted them met in spirit, let's say.

HAIGH: Another interesting point in the article was the need within the library for a classification system, something analogous to the Dewey decimal system for structuring the different parts of the library. It mentions consideration of the SHARE library scheme and of another scheme I wasn't familiar with, the AMS classification system. Do you remember anything about what that was?

BUZBEE: Nope.

HAIGH: You subsequently adopted the classification system proposed by J. Bolstad, whoever that was, in volume 10 of the SIGNUM Newsletter in 1975. So that classification issue wasn't one that you…

BUZBEE: I wasn't in on it.

HAIGH: It seems that as well as setting these standards for coding and documentation, the main activity of the committee was evaluating existing pieces of software to see which one would…

BUZBEE: And coordinating development. If we had an area where we needed new capability, then that was typically apportioned to the participating labs.

HAIGH: The article lists a large number of areas that were covered, like dynamic storage allocation, special functions, and for each one it basically says what code was adopted. Do you remember any cases in which the question of which (or rather whose) code to adopt proved controversial or it was hard to come to a decision?

BUZBEE: I don't remember any specific instances, but I suspect there were several [laughs].

HAIGH: In which areas did code from Los Alamos eventually make its way into the library?

BUZBEE: I believe our much of our special functions and linear algebra made it into the library. As I recall, Sandia had some good stuff on non-linear problems.

HAIGH: It says that the special functions used were the collection of special functions developed by Wayne Fullerton.

BUZBEE: He was from Los Alamos, in fact.

HAIGH: Could you talk about Fullerton and this collection of special functions? It's not something I've come across before.

BUZBEE: Wayne was one of these very talented people who typically showed up for work about 4:00 p.m. and worked until midnight, and he pretty much worked by himself. But he was very good on special functions. I've forgotten where he got his education.

HAIGH: It says that LINPACK was adopted as the standard for linear equations.

BUZBEE: That makes sense.

HAIGH: And EISPACK for eigen analysis. And for linear least squares, it says that a package of routines by Tom Manteuffel

BUZBEE: Tom was at Sandia Albuquerque.

HAIGH: It says he was formerly there, and then it says, "Now at Los Alamos." So he had moved over at some point.

BUZBEE: Oh, that's right. He and his wife came to Los Alamos.

HAIGH: But at the time he did that work, he was at Sandia?

BUZBEE: Yes.

HAIGH: It also discusses error handling, which it sounds like was a topic of some degree of controversy.

BUZBEE: [Laughs]. Again, I remember our help desk people were very keen on this topic.
HAIGH: It says that the subcommittee considered use of the SNLA error handler and rejected it, it also rejected the Bell Labs error handler, and R.E. Jones developed an error handler package for the SLATEC library.

BUZBEE: Ron Jones of Sandia Albuquerque.

HAIGH: It lists its various properties, and then it says, “the evolution of the error handler appears to be complete; however, its current implementation is expensive in memory -- so much so that Los Alamos personnel have implemented it directly into the system library. Until sites have substantial experience with this package, its future is uncertain.… The subject of error handling should be approached with caution.” Are you aware of what the eventual fate of that package was?

BUZBEE: I’m sorry, I’m not. It’s surprising how a seemingly innocent topic such as error handling can become so complicated. It was.

HAIGH: For a project intended to share code between institutions using a variety of different machines, portability would have been a much more important topic than previously.

BUZBEE: It was an important topic within a given lab because by this point we were into distributed computing, i.e. networks of computers from various manufacturers.

HAIGH: If this was a problem that each lab would need to solve anyway, it was removing a hurdle that had previously existed to collaboration between labs, because now they’d have to worry about portability even within their own lab.

BUZBEE: Yeah, yeah.

HAIGH: Now, it mentions the use of the Bell Labs PFORT Verifier. Do you remember anything about that system?

BUZBEE: I just recall that we had it, and it was a useful tool.

HAIGH: One of the novel contributions that you do refer to is the idea that the documentation could be hidden away entirely within the source code itself and then stripped out as required. Do you know if that was unique approach at the time, or had it been tried elsewhere?

BUZBEE: I don’t think it was unique, and I don’t think it originated at Los Alamos. It may not have originated even within SLATEC, as a matter of fact.

HAIGH: Do you know if it proved as successful in practice?

BUZBEE: I don’t recall any failures or inadequacies in it. Well, as we noted earlier, mathematics is a two dimensional language, and so there are situations where it’s very difficult to describe mathematics, in an easy-t-read fashion, using only one dimension, i.e. “linear text”.

HAIGH: Another thing I found interesting, reading the paper, was a discussion of licensing and distribution. I quote from page 310: “Because all software in the SLATEC common math library is available and because subcommittee members hope the library will enjoy usage by universities and laboratories engaged in scientific computation, a disclaimer for the software was developed and approved by the legal departments of all participating laboratories.” So do you remember anything about whether at that point it was considered controversial to give the code away? I know that later on universities and government labs tended to think much more in terms of licensing intellectual property and trying to recover some of the money that had been spent developing it. It seems in this case that the agreement was that you should just give the code away freely to everybody who might want it.

BUZBEE: As I recall, the concept of copyrighting software was embraced by the government at about this point in time. So the concept of intellectual property took on a new dimension, and I think it was in response to some of those things that caused us to go with a disclaimer.

HAIGH: I don’t think the disclaimer is presented that way in the article. My impression was it was more of a disclaimer, like “If our code makes your jet plane crash, we aren’t liable”, rather than it being a license that restricted their ability to redistribute the code.

BUZBEE: That was probably the intent of the disclaimer but I doubt that it was quite that literal [laughs]. It probably just said, “use at your own risk.”

HAIGH: Well, it says that it was too long to include in every routine so it must have been a little more verbose than that.

BUZBEE: Yeah, yeah. We didn’t write it, the legal department did [laughs].

HAIGH: So I mentioned the disclaimer probably stretched out a little bit. And obviously they were considering legal issues, but it doesn’t sound like the intellectual property rights were a concern.

BUZBEE: Indeed, I think it was mainly liability issues.

HAIGH: Do you know if in fact the software was used very much outside the labs?

BUZBEE: I’m not aware of any significant uses outside the labs.

HAIGH: Do you know what might have been holding back?

BUZBEE: IMSL was already in place, LINPACK, EISPACK—there were a number of other options.

HAIGH: So your opinion is that by this point the niche for a general-purpose mathematical software library was already filled by other packages in institutions outside the labs?

BUZBEE: Again, I think they had lots of options.

HAIGH: Do you have any recollection whether people outside the labs would have been able to take code from this library or from the routines produced by the other labs and incorporate it into their own products or resell it or modify it in some way?

BUZBEE: I assume they had that option, yeah.

HAIGH: So as far as you can remember, there was really no restriction about what people could do with this code?

BUZBEE: Right, right.

HAIGH: There was no copyright statement or licensing agreement as far as you're aware?

BUZBEE: I know that at some point in time we began putting copyright statements into some of that stuff, but I don't recall when it was.

HAIGH: At the end of the article, you sum up the lessons learned. One thing you mention is "ego-investment" on page 313, what version to use, the need for frequent meetings, the need for careful design, flexibility, good records, error handler…any comments on those lessons?

BUZBEE: [Long pause] No… Well, I think six is an important point.

HAIGH: That's "flexibility," which allows you to incorporate the free software even if it doesn't quite meet the exact coding standards you set.

BUZBEE: Yeah. And seven is important—good records.

HAIGH: Talking of which, you aware if records from the project would still exist?

BUZBEE: I don't know. I think error handling was a particularly good idea; it served us well. And certainly number nine: you need a critical mass of people.

HAIGH: Do you consider the project to have been a success?

BUZBEE: Yes, I do.

HAIGH: Did this new library replace the earlier internally produced code within Los Alamos?

BUZBEE: I think that code was always around, but this was the primary recommendation.

HAIGH: So in that sense, it was successful in providing a new primary library within Los Alamos. And do you know if it was as widely used in other labs?

BUZBEE: I don't.

HAIGH: The article covers developments up to early 1981, although the book itself appeared a few years after that. Did this process of collaboration continue after the release of the library?

BUZBEE: Yes.

HAIGH: It certainly implies that work is still underway; for example, on page 311 it says that "There is a rudimentary abstract, but this is an area for future work." And then a little further down the same page, it says that "SLATEC intends to issue one edition of the common math library each year and that any bugs detected in one edition would be fixed in the next one." So it certainly implies that this will be an ongoing process leading to regular updates published in new editions.

BUZBEE: Indeed, that was the case. For example, Version 2.0 of the SLATEC Library was completed in April, '84, and, consequently, in '84 the Los Alamos Mathematical Software Library consisted of "850 routines in the SLATEC Common Math Library as well as 150 other routines of equal quality"; (ref. Los Alamos Report LA-10354-MS, pa. 4-8). By '85, the SLATEC library had nearly 900 routines (ref LA-10631-MS, pa. 9-3).

[Tape 3 of 4, Side A]

Session Two continues.

HAIGH: That will wrap things up for the SLATEC library, then. We had talked through your own career at Los Alamos through the position as a leader of the group responsible for mathematical software and as it developed this programmer supply pool and performance evaluation. I know in 1980 you changed positions again to become Assistant Leader of the Computing and Communications division. So was this a job that had existed prior to your assuming it?

BUZBEE: I was responsible for research and, indeed, it was a new position. It came about with a change in leadership in the division. Bob Ewald became leader of the division and offered me this position, and I took it.

HAIGH: How did your responsibly for this job differ from those that you had previously?

BUZBEE: In this position, I was primarily involved in helping people within the division write proposals, compete for money, and – to first order -- oversee the execution of whatever proposals ~~he~~ we had won funding for.

HAIGH: That wasn't just in addition to your previous responsibilities then. It was a completely different job.

BUZBEE: This was a new job, and it's also the case that I was still very much involved in the advocacy of parallel computing and so forth.

HAIGH: Was this a job in which a large number of people would have reported to you directly, or was it more of a staff responsibility?

BUZBEE: No, I didn't have many people. It was not a line management job. It was more of a program management job. The group leaders reported directly to the division leader, and I reported directly to the division leader. My job was to help group leaders and their people write proposals and win money and what have you.

HAIGH: What kinds of challenges was the lab facing at this point?

BUZBEE: The milestone event during this period occurred in January, 1982 Ewald, Worlton and I along with three colleagues from Livermore toured supercomputer manufacturers in Japan and discovered that they were putting a lot of energy into high performance computing. So we came home and wrote a lab report on the subject, which was subsequently solicited for publication by the American Association for the Advancement of Science. It was widely read by management of U.S. computer firms and also by program managers within the government. It really gave us a lot of visibility. It was primarily because of that that I ended up serving on the White House Science Council Committee with regard to high-performance computing and performance measurement.

HAIGH: I’m just looking at your résumé. That would be this…

BUZBEE: It was published in 1982. December of ’82, I believe.

HAIGH: Oh, yes. [Buzbee, B. L., R. H. Ewald, and W. J. Worlton, "Japanese Supercomputer Technology," Science, AAAS, December 17, 1982, pp. 1189-1193]. And actually then three years later in 1985 there’s an article in the same venue you wrote with D.H. Sharp called “Perspectives on Supercomputing.” [Buzbee, B. L., and D. H. Sharp, "Perspectives on Supercomputing," Science, AAAS, February 8, 1985, pp. 591-597].

BUZBEE: Right. . But at any rate, by 1982, we were really trying to crank up the van, so to speak, to get more money into high-performance computing and to get the country’s leadership to become more aware of it and more attentive to it.

HAIGH: In that sense were you looking for more money for U.S. research institutions to buy machines with, or were you attempting to get more money to U.S. supercomputer manufacturers to help them compete against the Japanese?

BUZBEE: Both [laughs]. One of the outgrowths of this was the Lax Report, which led to the establishment of the NSF Supercomputer Centers, and that increased the market. DARPA was funding… what was that project in the late ‘70s? It led to Thinking Machines.

HAIGH: Strategic Computing Initiative?

BUZBEE: Yeah, something like that. In the ‘80s, there was a lot of research, and I make note of this. On one of my slides, you see a list of four prominent parallel architecture projects within the country, three of them located at universities. Here’s a list of machines that came to NCAR in the ‘80s and ‘90s, and the CM2 [Connection Machine 2, from Thinking Machines] showed up in the late ‘80s, CM5 in the early ‘90s. In some sense, that was the outcome of this research that was going on earlier in the decade.

HAIGH: You mentioned earlier that the vector machines were only a partial solution to the performance problems and that you and the rest of the community came to realize that parallel computers would be the way forward. What was the first parallel machine to be installed at Los Alamos?

BUZBEE: It was a Denelcor HEP. Right over there, in I believe 1982.

HAIGH: What did you say it was?

BUZBEE: Denelcor HEP -- Heterogenous Element Processor.

HAIGH: I’m not familiar with that machine. Was it produced by a small startup company?

BUZBEE: It was actually manufactured by a small company here in Denver. They sold maybe about a half a dozen and then crashed. It was a wonderful design. Burton Smith, who currently is the chief scientist for Cray, Inc., was the designer of the Denelcor HEP. Again, an ingenuous design, it just had two problems: it was slow and expensive [laughs].

HAIGH: So that first machine didn’t quite live up to the promise, then?

BUZBEE: No…. That was the first machine at Los Alamos that was specifically designed to do parallel computing. The Cray X-MP-4 also arrived at almost the same time—I think ’82 or ’83—and it was capable of parallel computation. We didn’t do much of it at Los Alamos—the operating system and the codes and what have you weren’t ready yet. In 1986, an X-MP-4 was installed at NCAR, and when I arrived there in the Spring of ’87, the machine still wasn’t fully loaded. It was a factor of eight more powerful than what they had before, and it takes it takes a while to absorb that kind of computing increase. So we created a queue for parallel codes and by 1989 it was in frequent use. In fact, Bob Chervin, NCAR, and Bert Semtner, Naval Postgrad School, were recognized at the 1989 Supercomuter Conference for the performance that they were able to achieve with a global ocean model formulated for parallel processing.

HAIGH: Here you also mention in 1986 an Intel Hypercube.

BUZBEE: That was at Los Alamos. Both the Denelcor HEP and the Intel machine were basically experimental machines. They weren’t production machines, i.e. machines used for major scientific computations.

HAIGH: You were still at Los Alamos at that point. Did the Intel machine perform well in practice?

BUZBEE: Yeah, I think there was considerable satisfaction with that machine. Microprocessors were not particularly fast at that point in time, so even though it had 64 of them, it still couldn’t match a Cray.

HAIGH: Now, you’ve discussed the ways in which the SLATEC library was a response to the arrival of minicomputers and the need to support software across a variety of platforms. Now, presumably, the arrival of vector and then of parallel machines would have produced some new and much more serious problems in terms of library support.

BUZBEE: Certainly, when the Cray-1 hit the floor at Los Alamos, Tom Jordan and others went through and vectorized a lot of the math software that was in use at that time. Up until the time I left Los Alamos, I don’t think that the Crays, were ever used to any great extent in parallel mode. Even at NCAR, I don’t recall that we had to make much modification to our math libraries. Generally the granule of work one devised at that point in time for parallel computing was relatively large, so whatever subroutines you wrote for a single processor would typically work fine within a granule.

HAIGH: Can you remember if there was anything specific that first began to get you really interested in parallel computing?

BUZBEE: Even the mid-seventies, Worlton and I and others realized that we ultimately had to go there. By ’78, we had funding from Jim Pool to begin research, and we began acquiring experimental computers.

HAIGH: So your funding to evaluate parallel computers for the lab predated the actual commercial availability of any parallel computers?

BUZBEE: Yes.

HAIGH: One of the things on your résumé is your involvement with the *Parallel Computing* journal that was published in 1984. So by that point were you already recognized as an expert in this area?

BUZBEE: I think so,. We were advocating it and had been for several years.

HAIGH: Can you tell the story of your involvement with the journal?

BUZBEE: Starting sometime in the '80s, a NATO Supercomputing Workshop was held every year or two. I got involved in that, gave presentations to it and what have you. I think it was through that that I was invited to serve as the North American editor.

HAIGH: Was this the first journal to be published on the topic of parallel computing?

BUZBEE: Parallel computing is an old idea, but I can't think of any others prior to this one, no

HAIGH: How important do you think that journal was in the growth of academic work in this area?

BUZBEE: I think it was helpful. It's a topic that was a bit esoteric. You couldn't just shuffle off to ACM or whoever and expect it to be given serious consideration. But since this was focused on parallel computing, it gave us a forum.

HAIGH: As Regional Editor for the Americas, what was your personal responsibility?

BUZBEE: Solicit papers from people in North America.

HAIGH: How did you go about doing that?

BUZBEE: Well, I was attending conferences, and I had a pretty good network, so I just kept looking for things I thought would be appropriate. When I saw them, I encouraged the author to submit a paper.

HAIGH: Were you encouraging people to conduct new research in the area, or were you just encouraging them to consider this particular journal as the place where they would publish?

BUZBEE: Publishing results. Yes.

HAIGH: Now at this point, would you describe the people interested in parallel computing as being a subset of the community of people working on scientific computing?

BUZBEE: Absolutely.

HAIGH: So there was no significant interest in parallel computing from other areas of computer science?

BUZBEE: I don't think so. Not that I can recall.

HAIGH: From what you’ve said, it seems that Los Alamos was particularly early and influential in terms of trying to attract resources and interest in this area. Are you aware of any other institutions that played a similarly important part in the very early days of parallel computing?

BUZBEE: Livermore, for sure. In fact, Livermore provided a lot encouragement to Arvind at MIT for the Dataflow project. Both labs encouraged Jack Schwartz at NYU on the Ultracomputer project. Livermore obtained a HEP shortly after we did. Livermore built a parallel computer, in fact, one of Edward Teller’s protégés built one.. George Michael became leader of the computer research group at Livermore in 1980, and shortly thereafter after, he and I agreed that we would work closely together in this advocacy.

HAIGH: So that was also an area of collaboration. It sounds like the national labs were driving the interest in the topic.

BUZBEE: The universities were working on it before we began beating the drum, but certainly we joined in the fray.

HAIGH: So you’ve talked a little bit about your work on the different committees and publications during the period ’83 to ’87 to do with supercomputing. So you mentioned the White House Science Council Committee on Research in Very High-Performance Computing as one of the groups you were involved with. What was the mission of that body, and what was its contribution?

BUZBEE: A national academy report came out of it, strongly advocating increased investment in developing the technology.

HAIGH: Was that also in respect to the potential threat from the Japanese?

BUZBEE: Certainly, we used that at every opportunity [laughs].

HAIGH: Was that tied to one of your publications shown on your resume?

BUZBEE: I think that council was created as a consequence of our trip to Japan and the report we wrote.

HAIGH: And then the Committee on Supercomputer Performance and Development at the National Research Council in 1986.

BUZBEE: That was headed by Jim Brown at the University of Texas, Austin. Jim was a consultant to Los Alamos, and his expertise is in performance measurement. And this again was an effort to enhance funding for work in this area, and it is a very important and complex area.

HAIGH: You’ve mentioned the establishment of these national supercomputing centers as a general result of the work of these committees you were involved in. Do you think that these two particular committees that you just discussed—the White House Science Council and the Committee on Supercomputer Performance and Development—were influential in accomplishing that goal?

BUZBEE: Well, I think for sure that the '84 paper was helpful to Jim Pool and other program managers in Washington in advocating budget increases within their particular agencies.HAIGH: I suspect this issue may be resurfacing when we talk about your time with NCAR, but while we're on the topic of these committees: clearly they were successful in as much as significant amounts of funding were provided to the area and the supercomputer centers were established. In a long-term perspective, do you feel that the broader goals you had in mind were accomplished?

BUZBEE: Certainly, by the early '90s. When I arrived at NCAR, the X-MP was on the floor and capable of doing parallel processing. And as I noted earlier, fortuitously, a couple of NCAR users, Bert Semtner and Bob Chervin had just completed development of a global ocean model, and they had formulated it such that they could use all four processors in parallel. Because the machine wasn't fully loaded, it made it easy for me to just say, "Okay. We'll create a queue for you guys or anyone else who's doing what you're doing, and you can use the idle time." And they did. The result was the first eddy-resolving global ocean model ever produced. Scientifically, it was a significant result, and because of the performance that they achieved with it, it was an honorable mention for the Gordon Bell award at the 1989 Supercomputer conference. We were among the first, but if you look within the atmospheric and science community, many of the centers were going parallel on Cray equipment by then. For example, by 1990, we had an eight processor Y-MP. By that point in time the NCAR community climate model had been formulated for parallel computation and performed very nicely on the Y-MP, and then in '94, we got a 16 processor C-9. Well, once again, it performed nicely using all 16 processors.

Producing forecasts is a time critical process, you can't wait very long to produce a forecast, once you get the data. And they had to go on parallel. So within the atmospheric sciences' community parallel computing was well established by the early '90s - various forecast centers around the world were doing this.

HAIGH: I was thinking also of these goals of improving American competitiveness in the area of supercomputing. I think you had mentioned that as a motivation behind the original report.

BUZBEE: Certainly in the White House report, we strongly advocated continued leadership by this country. In about 1988, Seymour Cray left Cray Research and went to Colorado Springs and created Cray Computer Corporation, CCC we called it. He set out to build the Cray-3. NCAR had a long collaboration with Seymour, so we actually showcased the Cray-3. The only Cray-3 ever shipped out of Colorado Springs came to NCAR. And he was designing the Cray-4 and was pretty far along at about the time we issued our RFP and went out on procurement in 1995. At that point in time, the next generation of the NCAR community climate model was nearing completion, and we needed a machine that could sustain around 20 gigaflops to do a 100-year simulation within a month or so. Had Seymour's company survived, I think they would have won that procurement. Unfortunately, they went into Chapter 11 just before the bids were due. Before that – after we issued the RFP --, the Japanese knocked on our door, asking if they could compete . So we checked it out with NSF; NSF said, "Fine. Let them do so." So we did. And we benchmarked both Fujitsu and NEC in the summer of '95, we weren't terribly impressed by what we got on those runs. Then in January of '96, we made a second pass through all of the vendors, which was basically Fujitsu, NEC, and Cray Research. NEC had the SX-4 up and running with 32 processors, and it was blazingly fast; we were able to run 25 gigaflops on it. Cray couldn't get the T-90 to run with all the processors at the time, but we were able to run with a few processors.

We already had a Cray C-90 on the floor, which was the best thing Cray had to offer other than the T-90, which was in development. And we didn't think it was going to work well anyway. So I said, "We will go for the SX4, the NEC." We knew when we did that that we would probably raise a lot of eyebrows, and we did. The U.S. intelligence community was particularly concerned about bringing foreign-made supercomputers into this country, so we ended up in a big dumping investigation. The final outcome was that the U.S. government imposed severe import restrictions which prevented acquisition of the SX-4. So NCAR was left empty-handed --- there was nothing in this country available that would do the kinds of things NCAR wanted to do. And it took five or six years before NCAR got back to the point where we would have been with the SX-4 in '96. A lot of science would have been done during that time had we gotten the SXs. Further, if the import restrictions not had been imposed, then -- almost certainly -- some U.S. organization(s), private or public, would have configured an ensemble of SXs in the U.S. that would have been competitive with the Japanese Earth Simulator So it was -- in my opinion -- a heck of a loss, a terrible loss to U.S. science .

HAIGH: Do you think there's a certain kind of irony there that in 1982 you had been involved in raising the alarm about the Japanese challenge, and then later on you became the center of this big controversy because you were now wanting to buy Japanese yourself?

BUZBEE: Yep. The Japanese had done two things: First, they had gone to CMOS as the basic technology -- fast and much cheaper than older technology. Second, they put a huge effort into the interconnect network connecting processors with memory and done an excellent job on it. In fact, they won some IEEE awards for that. I don't know to what extent the Japanese government may have helped fund those developments. At any rate, they build good cars and they build good computers.

HAIGH: As you had said before, one of the objectives of your work on these various committees in the '80s had been to channel the resources to make the American supercomputer industry remain more competitive. Now, clearly at least in the area of this kind of extremely high-performance machine that NCAR needed by 1996, this was not accomplished. So do you think that it's simply that resources weren't made available on the scale that you had hoped? Or was it that the resources were there, but people made the wrong decisions about what to use them on and that the Japanese made superior technical decisions?

BUZBEE: I think the Japanese made superior technical decisions. By the year 2000, American massively parallel systems were competitive with the best that the Japanese had to offer. Now, it's only been in the past few months that we have finally surpassed the Earth Simulator And even today, American scientists who have huge computations to do, look for ways to get to the Earth Simulator and get them done.

HAIGH: I understand from what you said that the American push to produce these massively parallel machines, particularly famously with companies like Thinking Machines and KSR, did not provide the kinds of capabilities you needed at NCAR. During your time at Los Alamos and at NCAR, you mentioned that NCAR had installed a number of these machines. Were you successful in getting them to work effectively with your applications?

BUZBEE: Yes, but until around the year 2000, they could not match the performance of the best parallel vector machines available. It just took a long time to bring massively parallel technology to fruition.

HAIGH: The developments that made the difference, were those in terms of software technology or hardware?

BUZBEE: I think both. And certainly model formulation on the side of the parallel systems. Because you know, it's usually a combination of message passing and interprocessor communication.

HAIGH: I want to go back to Los Alamos and talk more about NCAR tomorrow. While you were at Los Alamos, were you active in encouraging research on these techniques to make massively parallel machines work effectively?

BUZBEE: We certainly were aware of some of the critical issues associated with parallel computing. Of course, the reason for getting experimental machines was to get even more knowledge about them. I don't think anyone appreciated until the '90s the amount of effort that would be required to reformulate models so that they would perform reasonably well on these machines. Microprocessors had to get to a certain level of capability, interconnection between groups of microprocessors and so forth. It's just a very complex subject. The Japanese, because of their good parallel vector machines, and because they had made some good decisions in that technology, got ahead of us in the mid-'90s. Today the IBM Blue Gene is back at number one in performance.

HAIGH: During this period from 1980 to '84, when you were serving as assistant leader of the computing communications division, it is clear from what you said that you see your biggest contribution as having been in this area of raising the alarm over the Japanese threat and encouraging national investments in supercomputing. Were there any other important initiatives that you were involved with inside the lab or outside? Any other significant challenges while you held the job?

BUZBEE: No, I don't think so. UNIX had already become the operating system of choice; we had good mass storage capability by that point in time; networking was well on its way. We didn't have the Internet yet in terms of Web technology, but we certainly had networking.

HAIGH: When you described the job, you mentioned responsibilities including development and coordination of research. So presumably you were heavily pushing this parallel computing and supercomputing agenda. And it says also "the coordination of the division's advisory panel " What was the advisory panel?

BUZBEE: We had what we called a TIP, a Technology Information Panel. People like Dick Hamming and Jim Brown were on it and others of that caliber. We'd just simply bring them to Los Alamos once a year and basically tell them everything we were doing, how we saw the future going, and asked them for their opinion. I was the person who coordinated all of that.

HAIGH: So I imagine this issue of parallel computing and supercomputing effectiveness would also have been involved in that theme?

BUZBEE: They endorsed it.

HAIGH: And finally, I'm interested in why it was the "Computing and Communications Division." Were communications becoming more of a priority?

BUZBEE: Indeed. I've forgotten now when we changed the name of the division, I think around 1978. It started out to be just C Division, and then it became Computing and Communications due to the growth of distributed computing within the lab, i.e. a lab network connecting minicomputers in the various divisions with the central computing facility. Then we became a node on the ARPAnet, again, thanks to Jim Pool. So the name change was appropriate. HAIGH: Do you remember what kind of networking technologies would have been involved in that?

BUZBEE: I'm sorry, I don't.

HAIGH: So you developed a significant internal network before connecting to what became the Internet.

BUZBEE: Yes. Actually, the DOE Magnetic Fusion Energy center in California, had a satellite network by the mid-80s, and Los Alamos was a node on it. That node was not in the computing division. I think it was in the fusion energy organizations and it only served them. NCAR had something similar when I arrived there. It was only when NSF took on and created NSFNET that things really began to roll networking-wise, and that was in 1989 or '90.

HAIGH: Let's conclude Session Two then. And tomorrow, we can discuss NCAR and also your final position at Los Alamos.

[Tape 3 of 4, Side B]

HAIGH: Start of Session Three on the morning of Saturday, April 9, 2005, at Dr. Buzbee's home in Westminster, Colorado.

Before we move on to consider your final job at Los Alamos and your service at NCAR, I understand that you have a few additional points that you'd like to add to the topics we discussed yesterday.

BUZBEE: Indeed. In particular, the topic of the SLATEC math library. At both Los Alamos and NCAR, I encouraged the writing each year of a document that reviewed the previous year's accomplishments and laid out a plan for the next two years. So after our discussion yesterday, I went back and looked at a couple of these at NCAR. For example, the 1987 review, in regard to the SLATEC library reads "Installed and documented, a new release of the SLATEC library." That implies that it was already in place before I arrived here; I can't say for sure. Then in the 1990 review, likewise, "We expanded to include the following large libraries: NAG, IMSL, and SLATEC." You asked yesterday about who was the author of the CRAY FISHPACK, and, indeed, there's a photograph here of Roland Sweet and Dick Valent, and the caption to the photograph attributes it to them; it says, "CRAY FISHPACK developer, Roland Sweet." Another package at NCAR that I think is significant was MUDPACK, which was developed by John Adams along with Paul Swarztrauber and others.

HAIGH: Well, let me ask you about that a little bit later when we're talking about NCAR in general. Were there any additional points with respect to Los Alamos?

BUZBEE: No, I think we've covered that.

HAIGH: Well, I have one follow-up question. Talking to some of the people that were involved with computing at other large federally funded laboratories, one of the things that has been suggested in the case of JPL and in the case of Livermore is that by the 1980s (with the shift to minicomputers) it was becoming increasing difficult to obtain funding for a central mathematical software group within a lab. Are you aware of whether the Los Alamos central mathematical software team would have come under increasing funding pressures in the 1980s?

BUZBEE: I don't recall that they did; in fact, I think that group grew slightly during the '80s, so I don't believe it was a problem at Los Alamos. Of course, you never have as much money as you want, but we were able to do what we needed to do.

HAIGH: You've already mentioned that one of the motivations behind the SLATEC project was to improve library coverage on minicomputers. Are you aware of any other implications that the shift first to minicomputers, then to work stations and PCs would have had for the work of mathematical software teams within the labs?

BUZBEE: The computing division's primary responsibility was the central computing facility: the supercomputers and the machines therein. One of the payoffs from SLATEC was that the ensemble of people working on it included minicomputers in the implementation space. We got the benefit of that, but I don't think we put a lot of effort into it in Los Alamos.

HAIGH: So you think that most of the computing resources at Los Alamos would have remained with the central computing center and the big supercomputers?

BUZBEE: Even before the VAX there were a number of PDPs throughout the lab, sited in the different parts of the organization. And fortunately, by the late '70s, we were in the position to begin a local network with which to integrate all those things and use them for remote job entry and what have you.

HAIGH: So the shift to network machines didn't reduce the demand for services from the central computing organization?

BUZBEE: Not really, no. And of course we went national with networks. In the early '80s, the Air Force Weapons Lab funded a 7600 at NCAR, which was then accessed remotely by their users throughout the country.

HAIGH: Your final job at Los Alamos between the years of 1984 and 1986 was as Deputy Leader of the Computing and Communications Division. How did that job differ from your previous service as Assistant Leader of the same division?

BUZBEE: In the previous position, I had responsibility for fostering research, encouraging and helping people write proposals, helping advocated their ideas, and then tracking the progress when funding was obtained. I maintained some of that same activity as Deputy, but at the same time assumed responsibility for the annual review and report and plan. Whenever the Division Leader was away, I was the one who made decisions and so forth. So literally, I was the backup to the Division Director.

HAIGH: During those final two years, were there any policy decisions or initiatives within the lab that you were particularly involved with, other than the work we've already talked about in terms of parallel and supercomputing?

BUZBEE: No, I don't think so. We certainly continued our advocacy of parallel computing and R&D to develop it.

HAIGH: Another topic that's come up in discussion with some of the other people who've worked within national labs was the difference that changes in presidential policies and appointees could make to people working within them. During the time from 1968 through 1986, were there any changes in terms of administration policy that filtered down in a significant way to the computing work you were doing?

BUZBEE: Probably so, but none leaps to mind, other than the dumping order at NCAR which made a huge impact in the entire community. For sure at Los Alamos, as we began to formulate a network, we were faced with the question of how to handle classified machines and data and what have you. And our general philosophy was that there should not be any electrical connection between classified machines and the outside world. Security is just too difficult. I'm sure today they put enough resources into that so that the DOD, for example, can provide remote access to their classified facilities.

HAIGH: Did the energy policies of the Carter administration have any influence on the kinds of projects that the lab was involved with on the computing side?

BUZBEE: Let's see, I've forgotten when the Limited Test Ban Treaty was signed. I think that was somewhere around 1980…? Let's see, Reagan followed Carter in '80. Certainly, that treaty had tremendous implications for computing in the DOE because it meant that computing was even more critical to the weapons program.

HAIGH: How about the push for renewable energy sources?

BUZBEE: We had unclassified supercomputers at Los Alamos, and those were used extensively by energy research (ER) and other non-weapons activity. I don't recall to what extent, but I'm sure the ER budget at DOE helped fund those in some fashion.

HAIGH: Alan Hindmarsh had mentioned that in many cases the weapons teams preferred to do their own computing and were relatively reluctant to use routines provide by the central computing division. Are you aware of any such split at Los Alamos?

BUZBEE: I don't recall that was ever an issue.

HAIGH: So as far as you can remember, the weapons division would have been just as involved in using the standard mathematical library routines as any of the other parts of the lab?

BUZBEE: In thinking back over the people who came to me with questions, the energy-related people were probably more likely to do so than the weapons people, but I never encountered any problems with the weapons people, no.

HAIGH: So before we turn to address NCAR, I just would like to talk a little bit about your more general involvement with the mathematical and scientific computing community. So we already discussed your service in the various committees and your involvement with the *Parallel Computing Journal*. I wonder if you could maybe step back a little bit in time and talk more generally about how and when you would have become involved with groups, such as ACM SIGNUM and SIAM. Also whether you were involved in things like John Rice's series of mathematical software conferences?

BUZBEE: One of the strong points at Los Alamos was that every staff member was encouraged to attend at least one professional society meeting each year. The first year I was there, I went to Denver to an ACM conference and typically thereafter went to at least one professional society meeting, either in the mathematics or computing arena. Even in the early '80s, the high-speed computing community was relatively small. And I think it might have had one journal, the *Journal of Computational Physics* or something of that sort. Again, the community was not networked through professional societies and publications and journals and what have you. So throughout that period, we worked to improve that situation, and as I recall, we had a local chapter of ACM in New Mexico. In fact, one of the first talks I gave at Los Alamos was over at Santa Fe at an ACM chapter meeting.

HAIGH: Were you personally involved in the ACM's SIGNUM special interest group?

BUZBEE: I was a member of ACM for many years. I attended various ACM events and so forth, but I was not actively involved in SIGNUM.

HAIGH: I see that you served as a trustee of SIAM from 1986 to 1987. Had you previously been active within that association?

BUZBEE: My co-authors and I had published several articles in SIAM journals, and I had attended various SIAM national conferences, here in Denver and other places. The way I ended up on the board was, once again, Gene Golub. A vacancy occurred. Gene knew the DOE labs were supportive of professional societies, so he nominated me to fill the remainder of that term, and so I did. I don't recall whether I ever stood for national election or not.

HAIGH: You served also as chair of the 1992 IEEE/ACM Supercomputing Conference.

BUZBEE: Right. That conference was actually created by a small number of us who headed supercomputer centers. George Michael who headed the Livermore Computer Research Group, was the initiator of it. In the Fall of 1987, I think it was, he invited a number of people to meet with him near Stanford. I was there, Ron Bailey from NASA Ames was there, Norm Morris from Los Alamos, John Ranelletti from Livermore and others. That group became the steering committee for this conference. The first conference was held in Orlando in 1988, and the second one was held in Reno in '89, and the third one was held in New York City in 1990. By that point in time, it had become a pretty successful and mainline event within the community. I served on the steering committee up until 1996 or 1997, and typically the chair of a conference was selected two or three years in advance. So when I took on the job to chair the ’92 conference, that commitment was probably made in 1989, early on in the history of that conference. But it turned out well. We had a good conference.

HAIGH: Are there any aspects of that conference series you would take personal responsibility for in terms of having thought of or supported?

BUZBEE: Of course, I was at NCAR by the time this thing existed. We at NCAR put a lot of effort into the ’92 conference. Other years, many our people served on program committees in various capacities, made presentations and what have you. With the ’89 conference, we decided for NCAR to have a booth in the exhibition that would show off NCAR graphics, which at that point in time we were charging a small fee for, and that later grew to become an annual event. NCAR has had a booth in the exhibition ever since. It was a good thing for NCAR to have that kind of PR activity at the annual conference.

HAIGH: So that would lead us back to your time at NCAR. Now, why did you leave Los Alamos and go to NCAR in 1987?

BUZBEE: Three reasons: One, it was an opportunity to head a major computing organization. I had been an unsuccessful candidate for division leader at Los Alamos for two previous openings, so when this one came up, I went for it. Secondly, the atmospheric sciences had and still have enormous requirements for computation and data handling and networking. Thirdly, it was an opportunity to live in Boulder. Los Alamos was a wonderful place to work. But the other half of your life gets a little sparse down there because it’s fairly remote.

HAIGH: Can you describe NCAR at the time of your arrival? For example, how large it was, what its main programs were, what challenges it was facing?

BUZBEE: When I joined NCAR (and I think this was true throughout my stay there), the lab was organizationally divided into two groups: science divisions and facility divisions. The science divisions were focused on atmospheric science --- one was focused on weather forecasting, enhancing that technology. Another science division was focused on climate modeling; another was focused on atmospheric chemistry -- the ozone hole and related problems,; another was focused on high-altitude phenomena. On the facilities side, there were two divisions: one, the computing division and the “atmospheric technology division” which was the experimental science division. They had airplanes for observing, radars, all that stuff.

As I recall, NCAR’s budget was around $100 million when I arrived, and the computing division consumed at that point in time about 1/10 of that. NCAR had about 2,000 employees total. NCAR has very close ties to academe; in fact, it’s managed by a consortium of universities and has been since its inception. So while at Los Alamos everyone wore the same title, at NCAR there was a hierarchy of titles for scientists. That was a significant change. It wasn’t a problem in any fashion, but I sort of felt like I had joined academe.

HAIGH: Were there any other significant differences in the culture or atmosphere at NCAR from those that you were used to at Los Alamos?

BUZBEE: One of the main differences was that NCAR did not classify anything, so we didn't have to wear badges and all that nonsense, and that was really a pleasant change. Also, at NCAR, the computing division was chartered by NSF to provide computing both to NCAR scientists and to atmospheric scientists who were NSF grantees in academe. So we had a total of about 1,300 users at any point in time; typically half of those were NCAR scientists and the other half were university researchers and their students. The resources were typically allocated equally between them. That was a significant difference from Los Alamos.

HAIGH: So you were serving a much more distributed user community?

BUZBEE: A more diverse community.

HAIGH: What did that mean in terms of the way that you had to go about doing your job?

BUZBEE: In particular, it meant that we had to be proactive in reaching out to university users and understanding their degree of satisfaction with respect to NCAR. We did that in a couple of ways. We had a user conference the first few years, typically once a year. But we would also take what we called "jaunts," take maybe a week and each day we'd be at a different university, talking to our users there. And again, there's nothing like seeing people at work in their habitat. They particularly appreciated us coming. We used to refer to these as "work by day and fly by night" outings. We did have an advisory panel that met twice a year with a number of university people on it, and they provided us with a lot of good input as well. Also by the time I left Los Alamos, we had become increasingly user-focused at Los Alamos. And when I arrived at NCAR, I brought that same focus with me. That was something of a change, and I think it was a healthy change.

HAIGH: What had the previous situation been?

BUZBEE: I think the emphasis was to just get the capabilities and make them available, as opposed to really making a considerable effort to talk to the users to find out how well we were doing . But certainly there was a user conference prior to my arrival at the lab.

HAIGH: So you think the managerial challenges when you arrived at NCAR, improving relations with the users was the most pressing thing?

BUZBEE: The computing organization was somewhat "anxious" when I arrived --- as is often the case when a new manager joins an organization from outside of it. We, the rest of the division management and I, managed to turn that around within a few months. That was a nice accomplishment.

NCAR was spending a significant fraction of its budget on computing. One of the major challenges was getting more money, and we did so through a variety of processes. That was one of the nice accomplishments at NCAR. We had good support from NSF; that's so important. Bob Corell in particular was the associate director over geosciences at NSF. And the liaison with NCAR, a very effective and very neat person, was Cliff Jacobs. But anyway, in 1990, a research consortium led by EPRI, the Electrical Power Research Institute, entered the picture. Thanks to an initiative of the UCAR president, Rick Anthes who was very supportive of computing, NCAR and EPRI joined together to create the MECCA Project, and it was focused on climate modeling.

In particular, MECCA funded the acquisition of a two processor Y-MP, which was installed at NCAR and was totally dedicated to long running, multi-year climate simulations. That was so successful, that by ’94 Bob Corell won NSF funding to create a climate simulation laboratory at NCAR, which was to be a monster supercomputer totally dedicated to a small number of computation intensive climate simulations. It was that lab that played a major role in the procurement that we initiated in ’95. Had we gotten the NEC machines, two of them would have been dedicated to that laboratory. Those were very exciting times, both scientifically and computationally.

HAIGH: For those kinds of initiatives, I presume you would have been working with scientists in other parts of the lab to prepare proposals in which a large chunk of them would be earmarked for the computing facilities?

BUZBEE: Yes, certainly the climate division at NCAR was going to be a major beneficiary, but keep in mind that this climate simulation lab was going to be open to all comers, including NSF grantees. So by no means was it exclusively for NCAR’s benefit; it was for the community’s benefit.

HAIGH: Would those grants have been seen as grants to the computing division to provide facilities, or as scientific initiatives in which the provision of computing facilities was incidental?

BUZBEE: They were primarily to NCAR for computing. The funding of the scientists would be in parallel, but somewhat independent of that.

HAIGH: You said that one of the shifts had been that NCAR didn’t do any classified research, and you said that that was a pleasant change. So why did you personally find this pleasant.

BUZBEE: There’s so much bureaucratic trivia, in my opinion, that accompanies classified “anything.” When you’re hiring people, you have this clearance process that they have to go through, and they may or not make it. And it’s just a tremendous overhead, non-productive overhead. So I really was pleased to put it behind me and to deal with scientists on the basis of their science.

HAIGH: You’d already mentioned that NCAR was more hierarchical in terms of the scientists. Other than that, did you notice any particular differences between the mindset and culture of the scientific users at NCAR from the people you had been dealing with up in Los Alamos?

BUZBEE: The university users, typically professors and two or three grad students, --- that was a major difference from anything that I’d seen at Los Alamos. There are other DOE labs that do that. For example, NERSC probably serves a number of academics. But we weren’t serving many at Los Alamos.

HAIGH: Did the presence of the graduate students make a difference in the way you had to deal with the users?

BUZBEE: Not in a negative fashion at all; they really knew how to compute, and they did.

HAIGH: Now, you've already talked about the controversy regarding procurement of the Japanese supercomputer. Just listening to the description of the work that NCAR did, particularly in terms of the ozone hole and climate control, I would imagine that there would be potential for some controversy in those areas, too. Did you ever find that the work at the computing division was getting caught up in these broader debates?

BUZBEE:. We were not directly involved with the scientific controversies, but indirectly we were. Computer models are an approximation – even a simplification – to reality. Thus the extent to which one can trust the output of a model is paced by the extent to which the model has been used – both the number of users and the number of validated results. But to do a century long simulation took months of calendar time in the early '90s which could be frustrating to the scientists. One of the things we learned at Los Alamos was that if you really want to maximize people's productivity, then minimize the time that they spend waiting for results from a computer. . And if you have people who are doing a long simulation, you want them to have results every day, to make some significant amount of progress daily. . It might be one simulated year out of a hundred, but you need to give them some output every day from their model so they stay busy and stay focused on the problem. So you must provide them with "capability" – computers that are fast and an environment that enables steady progress. That's one of the great challenges in managing a supercomputer center. .

HAIGH: Did anything in the transition from the Bush administration to the Clinton administration, in terms of policies or priorities, have any impact on the work that you were doing in the computer division?

BUZBEE: Well, in fact, funding for the climate simulation lab, I think, was obtained when Clinton came to office. I don't know that it would not have been if Bush stayed in there, but I know the Clinton administration was certainly environmentally friendly.

[Tape 4 of 4, Side A]

HAIGH: I also imagine that the Internet must have had quite an impact on the work you were doing in that period.

BUZBEE: Absolutely. When I arrived at NCAR, it already had in place a satellite-based network called USAN that provided connectivity to some of the major universities around the country. Also, at that point in time, Paul Rotar of NCAR was in Washington on temporary assignment to the Computer Information, Science and Engineering (CISE) Directorate of NSF. He and others helped advocate and put in place the NSFNET around '89. We were also involved with ARPA. ARPA created a consortium to foster parallel computing, and the NSF supercomputer centers were key players. In the early '90s we had a meeting at the University of Illinois in which the first web browser was demonstrated. It was developed at Urbana and I think it later became Internet Explorer.

HAIGH: Mosaic was the software developed there, and the key programmer went off to co-found Netscape. Though the NCSA code was licensed to a firm called Spyglass and that in turn provided Microsoft with the basis for the first version of Internet Explorer.

BUZBEE: Okay. I was there along with a couple of other people from NCAR, and that was just such a delight. So we immediately came back to NCAR and implemented the same sort of thing here. Also, we urged NCAR to do the same thing on a lab-wide basis, and it did. So we were early users and beneficiaries of the Web. It's a tremendous asset.

HAIGH: Were you able to use the Internet to give researchers remote access to the computational facilities?

BUZBEE: Indeed, one of the first things that happened after I arrived is that we were wrestling with how to use the Internet. And one of our software engineers proposed what we called the IRJE, the Internet Remote Job Entry system. We got that up and running by 1989, and it was used extensively for the next four or five years.

HAIGH: You also mentioned work on graphics as being a visible part of the lab's work.

BUZBEE: The NCAR graphics package was a national resource. We had it at Los Alamos back in 1980, if not before.

HAIGH: Do you know who had been responsible for developing it?

BUZBEE: A number of people. We did have a small group in the division whose responsibility was NCAR graphics. Bob Lackman headed that group in the early years at NCAR, and shortly after 1989 and '90, visualization emerged as an option and, fortunately, Bob had hired the fellow – Don Middleton -- who now heads the Visualization and Enabling Technologies Section of the division. So yeah, we put a lot of energy into that. Visualization is not something a typical scientist can do on their own; they need professionals to help with it. At NCAR, it was fairly routine for scientists to bring their data in to the guys in the viz lab and after the vis lab folks had massaged it, the scientists would see behaviors that they not seen before. Sometimes the behaviors told them that their program might not be working the way they thought, and oftentimes visualizing experimental data showed a phenomenon not previously seen—new scientific insights. So we put a high priority on it.

HAIGH: Was the scientific computing division also responsible for setting policies for, or doing support of, people's desktop and personal computers?

BUZBEE: Initially, we were. That eventually became such a broad and demanding effort that it was spun off into another organization, which was fine with us, we needed to be focused on high-speed computing. But networking stayed within the division.

HAIGH: During this period, do you think an appreciable amount of the computation being done within the lab would have shifted onto smaller systems, or were most of the scientists dealing with such demanding problems that this wasn't practical?

BUZBEE: Every division, and groups within divisions, had their own local desk-side systems that they used for code development and debugging and checking and what have you. But when it came time to do computation, generally speaking, they had to go to the supercomputers.

HAIGH: Since about 1974, you've steered your career in more of a managerial direction. What would you say your strengths and weaknesses were as a manager?

BUZBEE: Fairness and openness were the strengths. And I've always welcomed debate and respected differing opinions. I think that stimulates people. Also, I encouraged people to tell me what they think: "Don't waste my time. Tell me what you think." Weaknesses… I probably should have been more aggressive in many situations, not to the point of being offensive, but being more aggressive than I was. I'm a laid-back, easygoing Texan.

HAIGH: Aggressive in terms of dealing with the people who were working for you or in terms of pushing for things from outside and above?

BUZBEE: Pushing for funding and decisions.

HAIGH: So more aggressive in dealing with the outside world?

BUZBEE: Upper [laughs]. Upper and outer.

HAIGH: Do you think anything changed in terms of your managerial style over that 25-year period?

BUZBEE: Certainly I hope that I grew and matured. I learned fairly early in management the importance of dealing with people positively, if you possibly can, to avoid letdowns. And I learned fairly early in management that every story has at least two sides, and be sure you hear both before you make decisions.

HAIGH: You already talked about the background in development of the controversy over the SX-4 supercomputer procurement. How did you personally react to being at the center of that kind of controversy? This was a quite high-profile story, widely reported in national press.

BUZBEE: It brought with it a lot of responsibility, and any time you add more responsibility that includes stress. At the same time, it helped us present our side of the story. In fact, last night I rummaged through some related materials. Here's an article from *Business Week* by John Carey, "A Trade Fight that Doesn't Compute," Business Week commentary, September 22, 1997. Here's another article from the *New York Times* by John Markov, "Congressmen Going to Bat For U.S. Computer Maker," April 29, 1996. So it was high responsibility—when you're dealing with the press, you want to get your words right, and you don't want to embarrass or in any way complicate an already difficult situation.

HAIGH: Do you think the case was generally reported in a fair manner?

BUZBEE: I think in the press it was, yes. Within the Beltway, we were really outgunned, no question about it. NSF never had much experience with this kind of situation. The DOD was hell-bent to keep the Japanese supercomputers out of this country. They were very skilled in dealing with the administration and Congress and what have you, whereas we were amateurs. . So we were on a steep learning curve, whereas our opponents were well-oiled and skilled.

HAIGH: Were you happy with the support you received from NSF?

BUZBEE: Indeed, I was.

HAIGH: Are there any other issues that you think were important during your time at NCAR that we haven't dealt with?

BUZBEE: I don't think so. The decision to go to NCAR was an excellent decision. I always felt fortunate to have been at Los Alamos, but I was particularly fortunate to come to Boulder.

HAIGH: You mentioned a couple of packages other than the graphics ones: FISHPACK and MUDPACK. Can you say a little bit about those two?

BUZBEE: Yeah. MUDPACK was developed in the early '90s. It was a package of portable vectorized Fortran subroutines for automatically discretizing and solving two and three dimensional real and complex linear elliptic partial differential equations using multi-grid techniques, which was, of course, the follow-on to direct fast Poison solver techniques.

HAIGH: What platforms were these intended for?

BUZBEE: It was developed for use within NCAR on our supercomputers, but it was, again, portable. There's an entire community, as you probably know, associated with multi-grid techniques. They have an annual meeting here in Colorado at one of the ski areas. Craig Douglas, son of Jim Douglas, coordinates that community. I can't give any statistics about how broadly used MUDPACK was.

HAIGH: Was that something developed with internal funds or was it part of a grant or broader project?

BUZBEE: I think we developed it with internal funds. Again, it was a natural follow-on to fast Poisson technology.

HAIGH: Did the nature of the simulation work being done at NCAR mean that you had a particular need for a package of this kind?

BUZBEE: Absolutely. Elliptical equations appear at every time step of climate models and weather models and what have you.

HAIGH: Other than the points that you've already made about the enormous size of these simulations and the difficulties in running them efficiently on a massively parallel machine, was there anything else that was distinctive about the computational needs of the NCAR community that played in the directions that you took?

BUZBEE: Another big difference between Los Alamos and NCAR is data. The atmospheric sciences are very data-intensive sciences. And consequently, NCAR had been putting heavy emphasis on mass-storage technology dating back to the early 1980s. Just prior to my arrival at NCAR, they had completed development of a new system called: NCAR Mass Storage System, which was hierarchical with long-term storage on tape and active files being kept on disc. And shortly after I arrived, within a year or so, fortunately, StorageTek introduced its robotic tape library technology, which allowed us to keep several thousand cartridges on line. And we immediately anted up the money to get one of those because it's a tremendous resource for us. If you talk to people in atmospheric sciences, you'll find that they value the data, archiving and access services that NCAR provides, probably as much as they valued the computing. Some of them value it higher than computing. And in fact, we had a group within the computing division whose sole task was to monitor the community and identify data sets that would be valuable to the community and acquire them and clean them up and make them available.

HAIGH: Was FISHPACK still under active development when you were at NCAR?

BUZBEE: I think it was in a constant state of enhancement, but the development was complete by the time I arrived.

HAIGH: That was a more of a low-profile maintenance activity. What led to your retirement?

BUZBEE: Once the dumping order was issued, we at NCAR were left empty-handed. There was just nothing available. That was tremendously frustrating. Also the political hype and what have you that we had just gone through… I was concerned I might be a liability to NCAR. I was old enough to retire and there were opportunities for consulting after I retired. I just felt it was best for both me and the institution to move on, so I did.

HAIGH: Do you know if there were any major changes of direction following your departure?

BUZBEE: The only choice they had was to go to massively parallel microprocessor technology, and they've done quite well in that regard.

HAIGH: That would have been quite a difficult transition.

BUZBEE: Absolutely. The models themselves…there was a probably a five-year period required to get them reformulated and rewritten so they would perform well. A lot of science was either deferred or accomplished offshore, science that would have done at NCAR , had we gotten the SX-4s.

HAIGH: How did your consulting career develop after your departure from NCAR?

BUZBEE: I did work with several organizations. I was certainly busy the first couple of years. Then we had the tech wreck at the end of the '90s, so business decreased a bit after that. But I've been busy enough, and I also substitute teach math and science in middle and high schools --- which is almost an exercise in charity, , but it's a valuable effort. Teaching is like swimming -- if you haven't done it, then you don't know about it.

HAIGH: What specific kinds of projects were you undertaking as a consultant?

BUZBEE: First, with Alaska, I chaired their advisory panel. I did some work with the NEC subsidiary here in the US, and in that case, we were attempting show that European and Japanese access to Japanese supercomputers might have some far-reaching consequences on American industry --- the ability to design automobiles, airplanes and what have you. And indeed, today Airbus has the dominant position in the commercial aircraft business. In the automobile industry, Toyota is now number three in this country. I can't quantify to what extent numerical simulation capability may have been a factor in all of that, but if I had to bet, I'd bet that it is a significant factor.

HAIGH: So in that case your expertise would have been in advising on trends in simulation work. Were you involved in helping groups obtain grants or advising them on possible opportunities?

BUZBEE: No.

HAIGH: What was your specific involvement with the Arctic Regional Supercomputer Center?

BUZBEE: In the beginning, when we made that deal, they didn't have an advisory panel of any sort, and they wanted one. So I took the action to recruit panel members, lay out plans, chair an annual advisory panel meeting in Alaska, and prepare a subsequent report. Jim Pool, for example, was a member of the panel for the first couple of years, and even today, we have members from Livermore and several universities. The Alaska center management has done an excellent job of taking our recommendations and using them as a part of their advocacy --- both to the university and the DOD. For example, the panel has been urging them to offer some sort of open system and be like other universities. And they pulled it off this last year. And I think they're the only DOD center right now that offers open systems.

HAIGH: Open in the non-classified sense rather the free-software sense?

BUZBEE: That's right. Also, in the year 2000 to 2001, Cray, Inc. entered into a contract with NEC whereby they could market the SX in this country, and shortly thereafter the idea was posed as to whether Alaska should try to get one of those machines. We on the panel cautioned them, having been through that procurement. We urged them to not surprise anybody in Washington; nevertheless, they did get a four-processor SX-4 and had it on site for two or three years --- the only site in this country that's ever had an SX-4.

HAIGH: What do you think had changed in the intervening period that allowed them to achieve that?

BUZBEE: The machine was used by a number of organizations for benchmarking. Also, by that time, most supercomputer centers were far along in switching over to massively parallel.
HAIGH: In terms of your work as a mathematics and science substitute teacher, can you say something about the kinds of challenges and satisfactions you've found with that?

BUZBEE: [Laughter]. Well, the biggest surprise I had… and of course I took a couple of classes in how to manage a classroom … but the biggest surprise I had was the assertion that in the classroom, 80% of communication between students and teacher is non-verbal … which to a computer geek was just staggering [laughs]. But I've come to believe that yes, probably so. Your tone of voice, your facial expressions, everything is important when you're teaching.

HAIGH: Did you learn any lessons there that would have been helpful earlier in your career?

BUZBEE: That one probably would have been [laughs]. And of course, the other thing about teaching, when you walk into that classroom, you've got to be prepared. You've got to have a plan … Few people comprehend the implications of preparing for a class. I've enjoyed it and I've learned a lot. Typically, substitutes don't have to do this unless they're subbing for several days.

HAIGH: You prepared a list of suggestions for the future. Would you like to discuss that?

BUZBEE: Indeed, I would. First and foremost is that this country has got to keep its people competitive with the rest of the world. The rest of the world has come to understand the value of education, particularly the Chinese and the Indians, and they're just as smart as we are. Given the right education, they can compete with us without difficulty, and they are doing so. I'm concerned because here in Colorado, and I think elsewhere in the country, education has really taken a funding hit. And then of course, we've got more of this security nonsense of not letting talented foreign students come in like we used to and shipping out some of those who we've previously granted visas to. I'm particularly concerned that our political leadership at both the state and national levels just doesn't get it. They don't see it. I think it's extremely important that we put a lot of priority on education—K through Ph.D..

HAIGH: I saw that you have prepared an article on this with George Michael and David Kahaner. Is that intended for publication soon?

BUZBEE: I regret that we didn't submit that for publication. We wrote that about a year ago. And we've handed it out to a lot of colleagues. Unfortunately George's health has deteriorated substantially, but anyhow we didn't submit it anywhere. In hindsight, I wish we had.

The second thing on my list of futures is the point that we saw in the procurement: American scientists and engineers must have access to the best tools available. I don't care where they're manufactured. If that means the government needs to ante up the money to get them manufactured in this country, so be it. Science and the advancement of knowledge and technology is so vital to our overall health, -- economically and militarily. Those with the best tools win. We just simply cannot afford to hamstring our people with second best tools.

HAIGH: I imagine that would be a belief you associate with your experiences with the procurement controversy.

BUZBEE: Came right out of that. I saw it first hand. I didn't appreciate it as keenly when until I went through that as I do today. Again, if we had gotten that SX-4 at NCAR, a lot of science would have been done that probably hasn't been done today.

HAIGH: Do you believe that there are many examples of similar examples around at the moment?

BUZBEE: I have not made it a study, but certainly any technology that Washington views as strategic, vital to the strategic well-being of the country is at risk because they're liable to veto it. One of the nice things about the editorial in *Business Week* in 1997 was the concluding paragraph—I always cheer when I read this: "If we really want to ensure that the nation's military and intelligence community stay on the cutting edge, Uncle Sam should simply and directly pay US companies to develop the machines we need."

HAIGH: Your third point is about reliance on foreign energy.

BUZBEE: Yes, I'm tired of going to war every ten years, killing lots of people, spending billions of dollars for energy. It is vital to this country that we become more energy independent. We'll probably never be 100% independent, but we have got to reduce our dependency on foreign supplies. If that means we go back to nuclear, go. I would rather have that problem than to have Iraq.

HAIGH: So despite your proud Texan roots, you're not aligned with the petroleum industry on that point?

BUZBEE: There was an article just a couple of days ago about an oil shale in western Colorado. There is a lot of oil there, and indeed, the petroleum industry is working on technology to extract it, and they think within ten years they'll have something. I don't see myself as being in opposition to the people in the petroleum industry.

HAIGH: To conclude, looking back over your career, what thing do you think you regret most?

BUZBEE: I would love to relive that procurement [laughs]. I don't know what exactly I'd do differently, but I'd put a lot more energy into it. HAIGH: So you're regret wouldn't be that you had gone into it in the first place. It would be that you didn't fight more effectively?

BUZBEE: That's right. Again, we were on a very steep learning curve, and I would love to relive that experience, knowing what I know today.

HAIGH: On a more positive note, what single accomplishment over the course of your whole career would you say that you're most proud of?

BUZBEE: That's hard. Certainly, the Fast Poisson Paper was a proud accomplishment. Also, I'm proud of the decision to come to NCAR and of the things that were accomplished when I got there.

HAIGH: That concludes the topics that I planned to discussed. If there is anything else that you would like to say, then now is the opportunity.

BUZBEE: No, I think we've covered the bases.

HAIGH: Okay. Well, excellent. Thank you very much for agreeing to participate in the interview.

BUZBEE: My pleasure. Thank you for the opportunity.

Appendix A: BILL BUZBEE Curriculum Vitae

January, 2000

EDUCATION:

Ph.D., Mathematics, University of New Mexico, Albuquerque, New Mexico, August 1972

M.A., Mathematics, The University of Texas at Austin, June 1962

B.A., Mathematics with Physics minor, The University of Texas at Austin, January 1961

PROFESSIONAL HIGHLIGHTS:

1987- 1998

I came to NCAR in the spring of 1987, to help make the NCAR Supercomputing Facility an effective and leading edge facility.

Each NCAR Division is externally reviewed every five years. Following is an excerpt from the 1992 Review. "The panel determined that the NCAR Scientific Computing Division (SCD) has made great strides over the last few years in improving the quality and quantity of the computer resources that it manages. NCAR can be proud of having such an effective organization. The management and staff of the division are very good. The quality and commitment of the staff both in their dedication in delivering a high-quality service and their grasp of the major operational and technical issues confronting them is impressive. They are in touch with their user community and are networked into both their peers at other supercomputing centers and the major vendors. This has been accomplished to a large degree by the leadership provided by division management and the culture in which the staff has been able to operate."

Following is an excerpt from the 1997 review. "The SCD has done an exceptional job of meeting its service responsibilities in an environment of rapidly evolving technologies, uncertain budgets, and constrained resources. By this measure (user surveys), SCD is doing very well and the user community appreciates the service efforts put forth by the management and staff".

<u>1995-98</u>

I led the NCAR procurement team that was the first U.S. group to evaluate the NEC SX-4, one of the most powerful supercomputers in the world. After NCAR announced that the SX-4 had won the procurement, the U.S. Department of Commerce initiated a dumping investigation. This attracted a lot of media coverage -- e.g. [1], [2], & [3] -- and I was one of a few people at NCAR who interacted with the media. Also, I led the team that testified before the International Trade Commission [4]. We knew from the beginning that chances of success were small, but the superiority of the SX-4 relative to other systems that were offered, was dramatic. Had we been

successful, the SX-4 would have made possible simulations in 1997 that even today are not possible at any U.S.
organization.

1. R. T. King Jr. and H. Cooper, "NEC Beats Cray in Bid to Build Weather-Forecasting Computer," Wall Street Journal, May 20, 1996.
2. Evelyn Iritani, "U.S. Supercomputer Maker Wins Anti-Dumping Ruling Los Angles Times, September 27, 1997.
3. John Carey, "A Trade Fight that Doesn't Computer," Business Week commentary, September 22, 1997.
4. "Bill Buzbee Comments on Cray/NEC Dumping Controversy" HPCwire, August 21, 1997.

1995-96

I served as Program Chair for the 1996 IEEE/ACM Supercomputing Conference in Pittsburgh.

1991-92

I served as general chair for the 1992 IEEE/ACM Supercomputing Conference in Minneapolis. This conference is a premier event in high performance computing and the 1992 conference was widely viewed as outstanding.

1979-87

As Leader of the Computer Research and Applications Group, Computing Division, Los Alamos National Laboratory, I directed and encouraged research in parallel processing as an alternative mode of high-speed computation.

After coming to NCAR in 1987, I continued to encourage research in parallel computing. Onsite parallel computers during 1987-98 have included Thinking machines CM-2 and CM-5, IBM SP-1, Cray T3D, HP SPP 2000, and the SGI Origin 2000.

1983-89

The Journal Parallel Computing was initiated in 1983, its first issue being published in 1984. "The success of Parallel Computing, as measured by the number and quality of the submitted and published articles, is to an essential degree also a success of our Regional Editors. To Bill Buzbee, who has acted as Regional Editor for North, Central, and South America from 1984 until May 1989, goes the merit of having convinced the most eminent scientists in the biggest Parallel/Supercomputer market of the world of the importance of Parallel Computing. Editors and Publisher thank him cordially for this excellent achievement." Parallel Computing, Vol. 12, 1989.

1980-84

As Assistant Leader of the Computing and Communications Division at Los Alamos, I assisted various national initiatives to ensure that the United States continued to enjoy world leadership in the manufacture and application of super-computers. In 1982 I contributed to the "Report of the Panel on Large-Scale Computing in Science and Engineering," by Peter Lax of the National Science Foundation. Two associated publications appeared in Science AAAS.

1968-70

At Los Alamos National Laboratory, my colleagues and I provided a mathematical analysis of a new technique for solving Poisson's equation introduced by Professor Oscar Buneman of Stanford University. This analysis proved the technique to be numerically stable and extended it to other boundary conditions and other coordinate frames. The initial publication of this work was entitled "On Direct Methods for Solving Poisson's Equation," by B. L. Buzbee, G. H. Golub and C. W. Nielson, and published in Society for Industrial and Applied Mathematics (SIAM), Journal Numer. Anal. 7, 1970, pp. 627-656.

In 1982, the Journal Citation Index surveyed 58 mathematical journals in an effort to identify classical papers. (A classical paper was defined as one that at least 50 other papers had cited as a reference during the period 1961 to 1980.) The aforementioned paper by Buzbee, Golub, and Nielson was one of six classical papers appearing in the SIAM Journal for Numerical Analysis, and it was the most frequently cited paper from 1961 to 1980. This paper was also recognized as a Citation Classic by Current Contents: Engineering, Technology and Applied Sciences, Vol. 23, #36, Sept.7, 1992. To qualify as a Citation Classic, a paper must be referenced more than 400 times. My primary contribution to the paper was to prove the numerical stability of Bunemann's modification to the well-known odd-even reduction scheme.

PROFESSIONAL ACTIVITIES:

Program Chair, 1996 ACM/IEEE Supercomputing Conference.

1998 –Chair of the Minnesota Supercomputing Institute's National Advisory Board.

Served on a committee to review the activities of the National Center for Environmental Protection (NCEP), Central Operations (NCO), 1997.

Served on computing technology review committees for Fermi National Accelerator Laboratory, Goddard Space Flight Center, the DOD Laboratory Infrastructure Capabilities Study, Center for Research in Parallel Computing, and the NASA COSMO program.

Listed in Who's Who In Science and Engineering, 1991.

General Chair, 1992 ACM/IEEE Supercomputing Conference.

Chair, Conference on Computing in the Atmospheric Sciences, 1992.

Member, Steering Committee for the IEEE/ACM Supercomputer Conference, 1988-97.

Member, IEEE Computer Society Scientific Supercomputing Subcommittee, 1989-98.

Member, Argonne National Laboratory Advisory Board for Computing, 1993-94.

Chair, Space Data and Computing Division Review Committee, Goddard Space Flight Center, 1995.

Chair, North American Editor, Parallel Computing, North Holland Publishers, 1983-1989.

Advisory Board, Journal of Supercomputer Applications, MIT Press.

Member, Editorial Board, Computing Systems in Engineering, Pergamon Press, 1995-97.

Member of The White House Science Council Committee on Research in Very High Performance Computing, 1984.

Member of the Committee on Supercomputer Performance and Development, National Research Council, 1986.

Member, Board of Trustees, Society for Industrial and Applied Mathematics, 1986-87.

Observer and contributor to the "Report of the Panel on Large-Scale Computing in Science and Engineering," Peter D. Lax, Chairman, NSF, December 26, 1982.

Member, Cornell University Theory Center Executive Committee, and Chairman of the Cornell Computer Policy Committee, 1983-88.

Member of Postdoctoral Committee, Los Alamos National Laboratory, 1980-83.

Member of numerous conference program committees.

PROFESSIONAL AFFILIATIONS:

American Meteorology Society

Association for Computing Machinery

IEEE Computer Society

Society for Industrial and Applied Mathematics

EXPERIENCE:

1999 – present

Consultant to various organizations that are involved with high performance computing. Also, a Fellow of the Arctic Region Supercomputer Center.

1987 - 1998

Director, Scientific Computing Division, National Center for Atmospheric Research, Boulder, Colorado, beginning April 1987. This Division, with an annual budget of over $20M and 100 employees, provides the atmospheric and oceanographic sciences community with a reliable, high-performance, productive supercomputing environment for the development and execution of large numerical simulations, and for the archiving and manipulation of large datasets. Network and communications capabilities are also provided for a national user community to access SCD computational and data resources.

1984-1986

Deputy Leader of the Computing and Communications Division of the Los Alamos National Laboratory, Los Alamos, New Mexico. This Division, with an annual budget of about $75M and 350 employees, had responsibility for a state- of-the-art computing network offering a variety of hardware, software, communication facilities, and computing services for the 8,000 persons at Los Alamos engaged in research and development associated with national security and energy programs.

1980-1984

Assistant Leader of the Computing and Communications Division of the Los Alamos National Laboratory, Los Alamos, New Mexico. Primary responsibilities included development and coordination of research, coordination of the Division's Technology Information Panel, and coordination of a project to develop a distributed processing network.

1974-1980

Leader of the Computer Research and Applications Group, Computing Division, Los Alamos National Laboratory. The Group's responsibilities included numerical analysis research, research on vector and multiprocessor algorithms, development and maintenance of math software, applications programming and benchmarking.

1973-1974

Visiting Professor of Computer Science at Chalmers University of Technology, Gothenburg, Sweden.

1968-1973

Staff member, Los Alamos Scientific Laboratory. Performed research in numerical analysis and assisted scientists in solving a variety of problems involving partial differential equations, systems of algebraic equations, and matrix eigenanalysis.

1967-1968

Programmer/Analyst, ESSO Production Research, Houston, Texas. Maintained reservoir simulators for one- and two-phase flow in three space dimensions.

1961-1967

Programmer/Analyst, Los Alamos Scientific Laboratory.

TEACHING EXPERIENCE:

1987-93 Adjunct Professor, Computer Science Department, University of Colorado

1983-84 Adjunct Instructor in Computer Science, Los Alamos Branch of the University of New Mexico

1973-74 Visiting Professor of Computer Science at Chalmers University of Technology, Gothenburg, Sweden

1972-73 Adjunct Professor of Mathematics, The Los Alamos Graduate Center of the University of New Mexico

SELECTED LIST OF PUBLICATIONS:

Bill Buzbee, James Hack, and Steve Hammond, “High Performance Computing and the NCAR Procurement – Before, During, and After”, Proceedings from the Conference on High Speed Computing, Los Alamos Report LA-13474-C, 1998.

Buzbee, B. L., "The NCAR Climate Simulation Laboratory and Trends in Performance per Unit of Cost of High Speed Computers", invited presentation to International Symposium on Parallel Computing in Engineering and Science (ISPCES’97), Tokyo, Japan, January 27-28, 1997.

Buzbee, B. L., "Comments on PVPs, MPPs, and NOWs and Future Computer Architectures," invited presentation to the CUIC Workshop in Marseilles, France, June 1995.

Buzbee, Bill, and IEEE Scientific Supercomputing Subcommittee, "Workstation Clusters, Rise and Shine," Science, Vol. 261, August 13, 1993.

Warner, Thomas T. and Bill Buzbee, "The Future of Weather and Climate Prediction," Visions of the Future, Art, Technology and Computing in the Twenty-first Century, Ed. Clifford A. Pickover, St. Martin's Press, New York, December 1992.

Buzbee, Bill, "Examples of Scaleable Performance," Proceedings, NATO Advanced Research Workshop on Software for Parallel Computation, Ed. J. Kowalik, Cetraro, Italy, June 1992.

Buzbee, Bill, "Supercomputing and the Environment," presented at Supercomputing Europe '90, London, January 1990, Supercomputer, Summer 1990.

Buzbee, Bill, "Supercomputing Facilities for the 1990s," 1989 NATO ASI Series, Supercomputing, Ed. Janus S. Kowalik, June 1989.

Buzbee, Bill, Editorial, "Report from Trondheim: Trends and Needs In Supercomputing," The International Journal of Supercomputer Applications, Vol. 3.4, winter, 1989 NATO ASI Series.

Buzbee, Bill, "The Philosophy of Supercomputing: The Triad of Theory, Experiment, and Prediction," Interdisciplinary Science Reviews, J. W. Arrowsmith, Ltd., Spring 1989.

Wienke, B. R., and Buzbee, B. L., "Supercomputing Applications," The Physics Teacher, University of Maryland, No. 1, Vol. 27, January 1989, pp. 10-21.

Buzbee, B., "Supercomputers: Value and Trends," The International Journal of Supercomputer Applications, Vol. 1, No. 2, Summer 1987, pp. 100-103.

Buzbee, Bill, "A Strategy for Vectorization," Parallel Computing, Vol. 3, No. 3, July 1986, pp. 187-192.

Buzbee, Bill, "Parallel Processing Makes Tough Demands," Computer Design, September 1984, pp. 137-140.

Buzbee, Bill, "Application of MIMD Machine," invited paper, Vector and Parallel Processors in Computational Science II Conference, Oxford, England, August 23-31, 1984.

Buzbee, B. L., and D. H. Sharp, "Perspectives on Supercomputing," Science, AAAS, February 8, 1985, pp. 591-597.

Buzbee, B. L., "Gaining Insights From Supercomputing," Proceedings of the IEEE, 72, 1983, pp. 19-23.

Buzbee, B. L., and C. A. Slocomb, "Distributed Processors and Intelligent Workstations at Los Alamos," invited presentation to the International Conference on Tools, Methods, and Languages for Scientific and Engineering Computation, Paris, France, May 1983.

Buzbee, B. L., R. H. Ewald, and W. J. Worlton, "Japanese Supercomputer Technology," Science, AAAS, December 17, 1982, pp. 1189-1193.

Buzbee, B. L., "The SLATEC Common Math Library," Sources and Development of Mathematical Software, W. Cowell, Editor, Prentice-Hall Series in Computational Mathematics.

Buzbee, B. L., J. A. Howell, and G. H. Golub, "Vectorization for the CRAY-1 of Some Methods for Solving Elliptic Difference Equations," High-Speed Computer and Algorithm Organization, Academic Press, 1977.

Buzbee, B. L., "Application of Fast Poisson Solvers to A-Stable Marching Procedures for Parabolic Problems," SIAM J. Numer. Anal., 14, 1977, pp. 205-217.

Buzbee, B. L., and F. W. Dorr, "The Direct Solution of the Biharmonic Equation in Rectangular Regions and the Poisson Equation on Irregular Regions," SIAM J. Numer. Anal., 11, 1974, pp. 753-763.

Buzbee, B. L., "A Fast Poisson Solver Amenable to Parallel Computation," IEEE Trans. on Computers, C22, 1973, pp. 793-796.

Buzbee, B. L., and A. Carasso, "On the Numerical Computation of Parabolic Problems for Preceding Times," Mathematics of Computation, 27, 1973, pp. 237-266.

Buzbee, B. L., and F. W. Dorr, "Computing Thermal Conductivity from Experimental Data," Instruments and Control Systems, September 1971, pp. 117-119.

Buzbee, B. L., F. W. Dorr, J. A. George, and G. H. Golub, "The Direct Solution of the Discrete Poisson Equation on Irregular Regions," SIAM J. Numer. Anal., 8, 1971, pp. 722-736.

Buzbee, B. L., G. H. Golub, and C. W. Nielson, "On Direct Methods for Solving Poisson's Equations," SIAM J. Numer. Anal., 7, 1970, pp. 627-656.