An Interview with

BRUCE D. SHRIVER

Conducted by Jeffrey R. Yost

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Bruce D. Shriver Interview

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Oral History

Abstract

Computer scientist and past Computer Society President discusses his education, his career as an academic computer scientist at various universities (University of Aarhus, Denmark, University of Louisiana Lafayette, University of Hawaii), research laboratories NASA-Moffett Field, and Thomas J. Watson Research Laboratory), his consulting, and his work to build an influential foundation (The Liddy Shriver Foundation) to aid patients, families, physicians in Sarcoma (through an influential and rigorous publishing and an extensive grants program). He relates his various roles in service and leadership to the IEEE Computer Society, which includes his serving as editor in chief of *IEEE Software* and *IEEE Computer*, and his tenure as CS President (in 1992), commenting on issues and initiatives with publications, conferences, and international-focused efforts.

Yost: My name is Jeffrey Yost, from the University of Minnesota, and I'm here today in Anaheim, California. It's Friday, January 31, 2017 and I'm here with Bruce Shriver, former president of the Computer Society. This oral history is part of the Computer Society Leaders Oral History Project. Bruce, can you begin by telling me when and where you were born?

Shriver: I was born October 18, 1940 in Buffalo, New York.

Yost: Did you grow up there as well?

Shriver: Grew up there for about the first thirteen years of my life before our family moved to Chicago. We lived there for five, six years, until I was just entering college, and then we moved to California.

Yost: Can you talk a little bit about your interests growing up, educational and otherwise, pre-college?

Shriver: Pre-college, I can't say there was anything exceptional about my grammar school education. In those years we went grades one to eight and we called that grammar school; and we went to four years of high school. I understand there now is a different stratification of names for those twelve years. I went to Loyola Academy in Chicago, which was a Jesuit prep school for college. I was in the science component of their curriculum. I was among the nerds of the group and so I was headed for science and

engineering. Initially I thought I'd want to specialize in chemical engineering but when I enrolled in California State Polytechnic University — [or] as it's called, Cal Poly Pomona — I became very much interested in aerospace engineering and mathematics. I liked aerospace engineering because the various courses allowed for a good degree of analysis to be done. It was engineering, but not necessarily practical engineering, going out and building things. It was trying to understand mathematical models of things and I enjoyed that in particular.

Yost: And you completed your B.S. in both fields in 1963, correct?

Shriver: Right, I had two separate degrees; one in mathematics and one in aerospace engineering.

Yost: While there did you have any introduction to computing?

Shriver: As a matter of fact**,** I did. It was there that I programmed the; before the 1620 there was a machine that we programmed — the 650 — which had a magnetic drum as the memory, so that instructions weren't fetched sequentially from the very next address location. You didn't increment the address counter by one, you actually put the address of the next instruction in the current instruction, where you knew the length of instruction. [The "length of instruction" refers to the "the amount of time it took the particular instruction to execute"] It would take the time and you would compute the rotation of the drum and so you would place your next instruction around the drum so that when it

would spin past the read/write heads you could read the next instruction. So it was quite interesting. Of course, you were just programming in assembly until we got the very first assembler maybe a year or two into that, with I think it was called SOAP, Symbolic Operation Assembly Program or something on that order. Then I went to the 1620 and there I was introduced to FORTRAN.

Yost: And you taught for a number of years as an instructor, after you graduated. Is that correct?

Shriver: Yes. I joined NASA at Moffett Field in northern California and it turned out that my dad fell quite ill. He had a business in southern California, which had to do with safety relief control values and process control systems and automation. He asked if I couldn't come back and help him during his time of recovery. Certainly**,** you would go and help your father. So when I went to go join his company, I came back down to southern California. I wanted to see if the university would allow me to teach part time. Of course, as an undergraduate only having a bachelor's degree in math and a bachelor's degree in engineering, the faculty had to make special arrangement for me to come in and teach. I had persuaded my dad to allow me to buy a computer to do the computations. This was 1961-2 time frame, so there weren't really a lot of computers around. The computer that we bought was from a firm called Pacific Data Systems and there was paper tape, so you keyed your program onto a paper tape, and then you fed them through a paper tape reader. But what was interesting was that it also had a different kind of memory. It had a magnetostrictive delay line memory. And so the memory was the pulses

being stored, going back and forth on the delay line. In the morning when you would come in to start the machine up, if the delay line wasn't the exact length required for the clocking the machine wouldn't boot. So you would take a screwdriver and either shorten or lengthen the line [laughs] 'til you got the machine to boot. In those days, I was obviously introduced to a lot of underlying computer technology, in terms of magnetic drums and delay lines and this type of thing. But the Pacific Data Systems machine was three-quarters the size of an office desk. I had a van and I would roll the computer in from the office into the van when I was going to the university, and then I'd roll it to my office when I was doing my teaching, because I would run computations and have students run equations and things like that on the computer. So I was one of the first people in the university to have a computer, but it was my own computer. [Laughs.]

Yost: You wouldn't think of that being portable but you made it so. Then you went on for a master's degree at West Coast University in Los Angeles?

Shriver: Right. What I liked particularly about it was I needed evening courses and a lot of the instructors at Cal Tech were teaching at West Coast and so I went for both their master's component in mathematics and systems analysis side; so I wound up getting a couple of master's [degrees] there.

Yost: In 1967 [for those degrees] and then on to SUNY Buffalo in computer science.

Shriver: Right. There I was attracted by the fact that they were building one of the original computer science departments and they had attracted in some real quality people. Tony Ralston, who they brought in as department head, was in numerical analysis and I was kind of doing those things. And Art Stroud, who was also well known; and Pat Eberlein; people like that who were in the numerical math side of things. And they brought in Bob Rosen and Gideon Frieder, people that were experienced in the software and systems design kind of things. So they were trying to build up a department of real energy and focus. I went there I think in 1968, and just really enjoyed it thoroughly. We had four children at the time [laughs]. I went back to get a Ph.D. with my four kids and my wife. And I was fortunate enough. In that era, NSF fellowships were rare and they had a very small number of them. I had applied for one and it turns out that two of the folks in the department — there were like 12 awarded in computer science that year and two of us, Mike Manthey and I got an NSF fellowship, which was $3,000 and it looked like a pot of gold in 1968-69. So that supported me with four kids. I wanted to finish grad school as quickly as possible and so in just a little under three years I was able to finish the degree and be on my way.

Yost: Who was your primary advisor?

Shriver: My primary advisor was Art Stroud. Tony Ralston, Pat Eberlein, and Bob Rosen were on my committee. In that era, we also had to take a couple of foreign languages for your Ph.D. degree, and I had taken Russian and French and had to translate articles, mathematical articles, and so on.

Yost: Your dissertation was entitled “Interpolation Formulas of Gauss Type for Approximate Solution of the N-dimensional Heat Equation.”

Shriver: Right, it was a numerical analysis Ph.D. but it turns out that during the time that I was writing that, I took a seminar from Bob Rosen on microprogramming and that basically changed my whole direction. I continued to do a number of numerical analysis things for 10, 12 — maybe longer — 15 years or something; but I became so enraptured by computer architecture that I put a significant amount of my focus and energy to learn and understand, and work in computer architectures. In fact, when I got my degree, in talking with Tony Ralston and Bob Rosen about possibilities and what should I do, I told them that I was interested in going abroad for a post-doc. Both Tony and Bob recommended the University of Aarhus in Denmark. Tony had been invited to go there. He knew Brian Mayoh, in addition, who was a very interesting fellow. Tony asked, “Would you like me to talk to Brian about you going to Aarhus”? I said certainly, and that all worked out that I was invited to go to the University of Aarhus. And just within weeks of arriving I was intimately involved with writing a grant application to the Danish Research Council to design and construct a couple of micro programmable computers. The team was just wonderful — people I was working with — we became colleagues for life, we still communicate with one another. We designed and built a couple of machines that had several interesting characteristics. They were horizontally programmed, dynamically micro programmable architectures, and that got us into the heat of the issues of code optimization for horizontal machines; which now the whole issue of parallelism

was part of what we were thinking about and having to deal with back in the early 1970s era. ‘Course it’s now raising its head over the last decade with multicore systems and so on, but we basically were integrally involved with these issues of writing compilers for generating code for horizontal parallel micro programmable environments. I didn’t even look back; I spent my next 25 years easy just in computer architecture at that level. Just an awful lot of fun.

Yost: And you were there from 1971-73, correct?

Shriver: We were there a couple of years but then just before I left, I applied for some NATO research funds so that I could continue to come back and work with my colleagues, and to invite them over to the United States. And so for the next several years, I was over there in the summer and they would come to the United States during the academic year. And I had some students of mine from Aarhus come and work with me, visiting professors and that type of thing. So I really had that relationship with Aarhus for another three, four years under NATO sponsorship.

Yost: And when you came back, you came to join the computer science faculty at the University of Louisiana Lafayette.

Shriver: That’s correct. I might interject a personal, non-scientific note here at this point. You have us in the middle of the Vietnam era, okay? I was not a supporter of the war and coming back to the United States was personally a difficult decision for me. My wife,

everybody said no, we're Americans, we're going back. I was offered a professorship at Aarhus and this was a difficult thing, too, to turn down, okay? So I said okay, let's go back; and I applied to the obvious places where I would want to go and I got offers from those places. But then I said to Bev as a compromise, let's go someplace where we wouldn't necessarily think of ourselves being and that will help us make a decision as to whether or not we're going to stay. Alright. And she said okay, and so I applied to this little university in the South that I never had heard about, okay, so I think this is a rather bold and unusual step. And lo and behold they made me an offer. But what followed the offer was particularly interesting. The president of the university, Dr. Ray Authement, called me over in Denmark and he said you know, we're in the process of wanting to build a substantial computer science department, that's why we made you this offer. And we're in the process of getting some computer equipment for our main computer facility; what would you recommend we get? This was 1973. I said well, Dr. Authement, one of the finest machines that I know of right now, which could really support all kinds of interesting research, and has a really incredible operating system of the future is called Multics. They have it at M.I.T.; they designed this machine. He says okay, we'll look into that. Indeed, by the time I arrived they had purchased this machine; I think it was only the second one purchased outside of M.I.T. So I went to where I had this incredible resource for the main facility and then I was given support for developing a microprogramming lab. And at the same time I had accepted the position, Ted Lewis had accepted a position at the university and Ted's a well-known computer scientist who's intimately involved in compilers and software, and just has a wide spectrum of insights and vision. He was working in microprogramming at the time, so we were really interested in collaborating.

At the same time, within a year we attracted Subrata Dasgupta, and TRN Rao, and other people with real good experience and competency in computer architecture and related issues [such as] error codes, and mathematical models of architecture and so on. We built up a nice little aggregate of people working in this area, all due to the support of Dr. Authement. So I wound up at this university that we didn't know much about, in a place in the deep South; and I must admit Bev and I were both biased against this. We thought of all the issues of race and so on that go on when you contemplate the South, but it turns out that Lafayette was this wonderful place to bring up your children. It was where the Cajun French, the Acadians, had settled in Louisiana, in this area where we lived and they had this wonderful ethic of work — work hard — and play — play hard — and it was a familial setting. The grandparents were at the same parties that the grandchildren were at, so it was multigenerational, paternal culture, you have of this old structure of families from Europe and America. And it was just a wonderful place to bring up our four children.

Yost: Sounds terrific.

Shriver: Yes.

Yost: Were you teaching in the area of computer architecture?

Shriver: Yes. I was running conferences, and writing and publishing articles, doing research, Ph.D. students, getting grants in the area, that type of thing. What you must do to survive in an academic environment. [Laughs.]

Yost: Right, and you quickly progressed from an associate to a full professor.

Shriver: Right.

Yost: Can you tell me about your decision to leave in late 1984 and join T.J. Watson Research Center?

Shriver: As you're publishing, and you're running conferences, and you're giving talks at various universities, and you're building an international set of colleagues; and the work that Peter Kornerup and I had done on the Mathilda machines, and that Ole Brun Madsen and Peter and I had done on the Rikke machines in Denmark; as that gained recognition for what we had done and so on, I had been approached by a couple of people from Yorktown to come up and visit and think about a possibility of joining them. This was right at the time of the Fifth Gen Project of Japan. I think if you remember all the hoopla that had arisen at that time about this; and the Japanese had targeted a particular type of approach. My recollection is that it was a Data Flow architecture with some parallel compiler kind of technology and logic programming as the base for undertaking the development of their models. Andy Heller and John Cocke and Ralph Gomory were basically the people at the lab I visited. And as I left the visit — I was there two days —

they made me an offer, which was interesting and attractive in terms of how and where I could spend my time. And now when I got back to Lafayette I said honey, I think this really a wonderful opportunity; this is a world class laboratory; I'm going to have the resources to do the kinds of things I want to pursue. She, of course, had visualized New York as Manhattan [laughs] and so when we made a trip up here so she could take a look/see, and you get 30 miles outside of the city and you're in beautiful rolling forested hills with lots of lakes, it was kind of transformational for her to see this. She realized well gee, this isn't Manhattan, this is a wonderful setting, a beautiful area, and so on. So she was supportive and of our four kids, one was in college, second year, and one had just started. All four kids, we obviously said, you can go, we're going to support you whatever your decision is. The high school children, of course, those two would come with us. But the other two said they'd love to come and we made a big tour of colleges; and they chose where they wanted to go; and the family just thoroughly enjoyed living there so it was a very nice time for us.

Yost: Can you discuss your research there and the managerial roles that you had?

Shriver: I can just give you some general ideas, okay? Computer architecture really has to have its basis in knowing the nature of software you're going to be supporting. It starts with understanding what compilers do, and how they emit code, and how the code is executed so you can increase the parallelism. The group there was just some world class people. John Cocke clearly is recognized as one of the greatest computer scientists that have graced not just IBM but the computer science community in general. I was fortunate

to be able to work in the area that he was working in so all of his ideas of super scaler, execution, instruction level parallelism, all the compilation techniques that are involved with that and the underlying nature of how that impacted the architecture and how you would implement that. Those are all the areas that we were looking at. But you had to keep your foot involved in software at some particular point; you couldn't just isolate yourself over in the underlying architecture related issues. That's how you are making your advances. So I had a group of software people that I was working with as well as dealing in these other issues.

Yost: And you were there for about a half decade?

Shriver: Correct.

Yost: What led to your decision to leave T.J. Watson and you went for a year or two to the University of Hawaii, is that correct?

Shriver: What happened was there was a significant downsizing happening at Yorktown because of the economy, new management within the company, and if you go and take a look at the history of T.J. Watson Research Center — which you as a historian may have already done — I would characterize it as going from a big "R" and a small "d" to a small "r" and a big "D". [R stands for research, D stands for Development] That typically Yorktown had this wonderful relationship with the labs, and that they'd get some of their funding from the labs with that small "d" component. But capital "R", big "R", was really

driving the focus of the efforts at the lab itself. It was quite clear toward the end of the 1980s — 1988, ’89, ’90 — that these were dramatically changing at the lab. So in the next couple of years, I left and I did two things. Number one, I was invited by the University of Hawaii to help start a research unit within the University of Hawaii, which would bridge the computer sciences and the business IT components. I found that very satisfying and that was a really interesting exercise to do but my wife and I felt we couldn’t stay on there because after living there for even nine months, we found to our liking that Hawaii was too far distant from our extended family; it would take us six hours just to fly to San Francisco but our extended family was in New York, our kids and their wives and our grandchildren. So we just felt that yes, we had them come and visit us but a visit once or twice a year, you know, what’s that? So we decided to come back stateside and Dr. Authement wanted me to come back to the University of Louisiana to help start a vice president for research position there, which I did do. I came back and started it for him; formulated the entire structure of such an entity that then the university and got it all started with the research and embryo facility that you would have in such structures. And then I just wanted to take a little time to consider what I would be doing so I decided to go into consulting.

Yost: At D.H. Brown.

Shriver: Yes.

Yost: And after doing that for a short while, you started your own consulting firm called Genesis 2, Inc.

Shriver: Right.

Yost: Was that in New York?

Shriver: Yes, I functioned right out of my home, as many consulting companies did. I consulted for numbers of firms: AMD, Intel, etcetera. The area I was focusing in, and I was also involved in litigation as an expert witness for a number of cases in computer architecture, computer arithmetic, and that type of thing.

Yost: On the CV you provided, it states up to the present, you said your CV wasn't completely updated; do you still consult?

Shriver: No, I basically [stopped] about 2002, 2003; I'll tell you what happens up to that particular timeframe. During that timeframe, when I was consulting extensively, I also wanted to keep my foot in teaching so I took on a number of adjunct professorships. I deliberately took them on abroad because I knew what my colleagues were doing here in the States. I had been constantly involved with them on projects and so on. So I took an adjunct professorship at the Technical University of Berlin, and then I took an adjunct professorship at the University of Hong Kong, and then one at the University of Tromsø in Norway. These were basically to come and teach computer science courses in

computer architecture at the graduate school level. So, as an example, in Tromsø I would fly over once a week during the month of the semester. So in the spring semester — January, February, March, April, May — I would go over once each month and teach for a week, and then my research assistant would teach the labs while I was gone. So I would come back the next month and teach the course that way. So up until 2002-3 era, I was consulting and teaching in that model. Then one of our four children, our daughter, contracted a rare cancer; a family of cancers called sarcoma. The particular sub-type of sarcoma that she contracted is called Ewing's sarcoma. Adults with Ewing's sarcoma rarely last longer than 18 months after diagnosis. She actually lived for 21 months. She passed away in 2004 and what basically during her time with the disease. We learned that there was no information outside of the medical journals for patients and families. Furthermore, since this was a rare cancer, many oncologists never encounter this particular cancer so there was a desperate need for information; not just by the patients and their families but also for physicians. When our daughter was just a couple of months she passed away — her 37th birthday — we all recognized that she wasn't going to be with us much longer and I told her that I was going to start a foundation that would basically do something to help these families [dealing] with sarcoma. There are so many things you can do for families that are experiencing cancer, and a rare cancer in particular. You can help them with expenses, you can help them with second opinions, the list is infinite. My attitude was you should only do a couple of things but do them well. What did I know from my academic environment within the professional society, within the IEEE Computer Society? Well I was editor-in-chief of a couple of these journals. I was an editor; I knew how to do journals, okay? And I knew there was a

desperate need for information. And as a university professor, you live and die by the grant, okay? You provide for your students and so on, and you were on NSF review panels, and so on; you knew what the grant processing machinery was all about. So I decided that I would focus on doing a periodical on this rare cancer; and doing grants, quality grants. What we learned very quickly, in terms of how sarcoma research is funded, is that families were so overwhelmed by the death of a loved one that they would go and raise you know a couple hundred thousand dollars and they would give it to the hospital where the physician was that treated their husband, wife, daughter, son, okay? And there was no analysis of what was going to be done with that funding. So I knew that I was going to do a peer review process research grant application methodology. With respect to the publishing, one of the models that I had developed when I was editor-in-chief of *Software* and editor-in-chief of *Computer* was if an article was going to be focused; if it were written for the more technical audience, then I wanted it peppered with a lot of sidebars that would allow the layperson to follow through, and just give insights into what all these underlying equations and graphs were about. And I thought well gee, what if I got some oncologist to write articles for their peers because their peers don't know about this disease, but we'll pepper it with enough hyperlinks and sidebars and figures so that the families, the layperson, can follow through and get a sense of this. And there has to be the pragmatics of treatment somewhere in these articles, whatever is known at the time. And just by blind luck, the first couple of articles that I published were written by physicians that I had met who were completely taken by this concept. They produced absolutely stellar articles that immediately their colleagues recognized as what a way to convey information! So in an incredibly real sense, my work in the

Computer Society, what I had done as a volunteer, benefitted me to the extent that I was able to use those methodologies in this rare cancer community. So once I had the first couple of these review articles out, people saw the real benefit; the downloads were amazing! Thousands of people within a month; physicians and patients would grab this article and take it. We would get calls from patients saying you know, our daughter or son was just diagnosed with osteosarcoma and the physician gave us this paper from your website to read; the physician gave the patient to read. And then we had physicians calling us saying patient "x" just came to see me and they gave me this article that was from your website. Wow, how did you do this kind of thing? So in these years when we became intimately involved in the sarcoma advocacy community, from 2004 up to like 2015 we were working 70-hour weeks just doing these research grants. Our research program stood out as stellar; people recognized the quality of the work that we had done. Our rejection rate — this is something interesting that I just related to our editor-in-chief of *Computer* magazine, Sumi — our rejection rate for articles was 85 percent [and] our rejection rate for grant applications was 85 percent. We funded 60 grant applications, over $6.5 million so far. You can tell me how many applications I received, because I supported 60 of them. Similarly, with articles, we published almost 200 articles. There were only 2 grants out of 60 that were funded without resubmission. I wanted our reputation to be staked on the fact that whenever you came to us with something, we would give back to you significant scientific feedback that would tell you why we rejected your grant if we did and how to improve your work, if we did. And similarly, if we were going to accept it, how to improve it, and so on. After our first two years of funding grants we were inundated because of the quality of feedback we gave, and

similarly on the publication side. The articles that we published, this enormous number of articles in that time period, they were so good and so useful that sarcoma oncologists from various countries — Spain, Portugal, Italy, France, Japan, China — came to us to ask permission to translate them. You're only going to go to that kind of effort for something of real value and substance. And during this time I was continuing to act as an adjunct to Tromsø but the load was just so much that in 2014 I really cut back from those adjuncts. And there's another important issue here; is that in order to get involved as much as I could in understanding why a set of referees were rejecting our grant applications — whatnot — I had to start learning a lot of microbiology; learning microbiology so that I could talk with them about the issues. I maintain that you had to learn the equivalent of writing a master's degree in microbiology, so how was I going to continue what I was really interested in in computer science, with all of a sudden this demand to learn this other material? I said wait a minute, why am I doing two different things? There has to be some issues about biologically inspired computer systems so I then began reading the literature on nature-inspired computation and that type of thing. And where a lot of work had been done on algorithms; now the timeline I'm talking is 2005-6-7, if you go back and look historically at the way literature growth curve goes, nothing had really been done in underlying architecture except for a very few papers, once of which just had a tremendous impact on me. Shapiro, over in Israel, had actually designed a finite automaton in DNA, in a tube. And I thought, my God, isn't that parallelism? I've got a drop in a test tube and I've got a million computers working at the same time. So I started studying the area, because I wasn't all of a sudden spending this enormous amount of time in two different areas, I could combine my architecture with

microbiology. So I then starting preparing lectures in computational hardware inspired by nature. I remember my first set of lectures I gave in Aalborg and in Budapest at the John von Neumann Society, and in Tromsø. I laid out these issues and the basic response was gee, Shriver, I think you've drunk one too many aquavits. Why don't you come back to microarchitecture where we know and love you and we can talk in circuits, okay? So half of the audience would basically politely smile, but a few people would come up and say where do we learn more about this? And I would say well, if you don't have the biological background you really have to study microbiology. So two thirds of them would leave, then there would be a couple people standing, and they said yeah, but what are the papers? So I knew there was a spark of interest. That was the first year I was giving that set of lectures. The last set of lectures I gave in Tromsø — I think it was like 2014 — totally different. There was 40-50 people, professors and students from departments all over campus taking me to their labs after the lectures were done kind of thing and then coming back for the next day's lecture. Significant degree of additional interest because now we were on the curve where people could see this was a new computing modality, a new way to think about things. So that was quite exciting.

But all the hours that you put in; I'm not a spring chicken [laughs] so the hours take their toll, so in 2015 my wife and I decided we would gently shut down the Liddy Shriver Sarcoma Initiative. When we did that, I thought that by the middle of 2016 we would be done with it, but it's taken a bit longer to do in a responsible way because I still have a couple of grant applications in the process; we just awarded a $1 million grant in Leiomyosarcoma. We just awarded a three-quarter million dollar grant in immunotherapy and sarcomas; and I have a couple more that are in the bin that will take me another six

months to go through this whole process. I hope that wasn't too long a response to your question.

Yost: Oh no, that's great. With all the success of this initiative and in using the rigorous peer review infrastructure as well as what we learn as an editor, to really transform the dissemination of knowledge and resources for victims and families. Did it influence other efforts in other areas of cancer disease [research]? Did anyone come to you to advice as a model?

Shriver: Absolutely. There's a couple of lessons here. In the Computer Society — and because I did my post-doc abroad and when I came home I continued those relationships through NATO grants — I had always had an international kind of focus. Whenever I was invited to give a lecture abroad, I would go and build relationships with that group and then bring them back to my group, somehow. When I was president of the Computer Society, one of the last things — in fact the very last meeting that I chaired of the Board of Governors, at that meeting, which was in the early 1990s, remember the wall had basically come down in the Soviet Union through the late 1980s, early 1990s. The whole area of former Soviet Union nations was emerging. I had made an impassioned speech. I had to hand over the gavel because the president can't, you know; so you have to hand over; so I gave this speech about why we should [have] an initiative to welcome these engineers and scientists from former Soviet Union countries into the Computer Society because they'd been isolated all these decades. Right after I made that speech to the board I was called out of the meeting for a personal family issue. Somebody tapped me on the

shoulder, said your wife is on the phone, some emergency. So I handed the gavel to Jim Aylor, who was the next president, he took over the meeting, handled whatever it was. I came back into the meeting, the meeting's going on; there was a space next to Michael Elliott, and I went over and sat next to him. I leaned over and I said, "What's going on since I left?" He said listen — Jim hesitated — he says that's it, the vote is that Shriver will be the chairman of the Central and Eastern European Initiatives Committee. [Laughs.] Michael leans over and tells me, "Didn't I tell you never to leave the room when you made a good suggestion?" [Laughter.] I thought that was cute.

But that international focus of visiting the people and bringing them in, I carried that to the Liddy Shriver Sarcoma Initiative. One of things that we did with our grant applications was that I wanted to spur collaboration. Much of academic research — not just in medicine but in all of our fields, unfortunately — is built on the Fifedom model. A strong professor gathers people around him, you don't publish results until you've got them, the professor's name is on the top of the list, everything is quiet. Nobody is sharing results; collaboration is not done that way. So in our grant model, I wanted to fund international collaborative grants. There have to be at least three investigators from three different countries, no more than five, no less than three. I don't want to fund all of you coming from the same medical specialties; in other words, I don't want to fund five pathologists or five surgeons, I different viewpoints; so that in computer science I want software, I want hardware, I want algorithm, I want different insights being brought to the problem, whatever it is. So you have to submit this application, show me that in any of the projects that you define there's at least two of you intimately involved. Not just oh, I'll do the results and you can kind of check them in your lab. I want at least two of you

on each of these projects. You get three to five of you, you come internationally, and you come together. Those grants, two years each, also we required if you're really committed to this and the money's tight, we want you to match us dollar for dollar. So if I tell you I've got a half a million dollars, you've got to have half a million dollars. I don't know where you'll get it; from your dean, your whatever, your med school. We wanted this real financial commitment so we were leveraging money.

You had to meet twice a year, face-to-face. Electronics are nice but sitting down across from you is quite different. And we're not going to pay for any travel, any publication costs, any conference fees. Money's tight in rare cancers, we can't waste it on those things. Ask your dean for those, you're a med school, you've got lots of money, my mindset, okay? The side effects of those grants, which now everybody's doing; so did we impact sarcoma? Absolutely. I did those for seven, eight years before people really started [saying] wait a minute, look what they're doing, why don't we do something similar to see if we have the same effect? Here are some of the side effects: in the face-to-face meeting, each of the physicians that was one of the co-principal investigators would variously bring the research assistants and whatnot that were going to be in the lab doing the work. So three to four people would all of a sudden become 15 people at this meeting. And at the meeting, Dr. A would say this is why I think we should use these particular cell lines. And Dr. B said those particular cell lines are worthless, why would you use those? They're not going to give you the kinds of insights you want to get. So the students would be hearing a dialog with their mentor that they've never heard before, and they realize that collaboration is much better; look how more quickly we're learning all of these things that can do this. So the most important thing was you're educating the

next level of physician scientists for a different algorithm, a different way to collaborate. And over these years we've seen that. One of the proofs of the pudding is that of the groups that we initially funded seven years ago, they are still meeting today and they've gained in size. We funded three or four of them, they're now up to 15 people that meet regularly on that particular subtype of sarcoma. When we decided to shut down the Liddy Shriver Sarcoma Initiative, we obviously got scores of letters from both families and physicians. All of the physicians lamented the fact that the publication *ESUN*, and that the grant program would no longer be there. So we've taken a lot of time to work with groups, teaching them what we did, how to use it, so if they want to use that model they can. One group in particular, we actually had little training sessions in our home for several days for four people that were going to do the processing of the grant applications that are administrated, and then interact with the submitters, so we're trying to pass that as well. Obviously I'm indebted to the Society for the opportunities it gave to me to learn these things, to do them, to exercise them, and to influence what I was doing in the Society at the time and take that over to this cancer [initiative].

Yost: What a wonderful thing you and your wife did and the impact it's had on that area of cancer, and beyond. I'd like to turn to some specific questions about the Computer Society and the various roles you've had. Did you join the Computer Society as a graduate student or early in your career?

Shriver: No I didn't. In fact, how and when I joined the Society is interesting. I was a member of ACM and I was on their lectureship tour and having a great time. People had

invited me a number of places and whatnot, and I got a call from Oscar Garcia saying first, we're starting a new magazine and I'd like you to consider becoming editor-in-chief of it. I said Oscar, thanks very much for the invitation, it sounds exciting — he told me what it was all about — I said but I'm not a member of the Computer Society. [Laughs.] So that's when I joined the Computer Society, when I became editor-in-chief of IEEE *Software*.

Yost: That was when the journal started in 1983?

Shriver: Yes.

Yost: I interviewed Oscar and he's a fascinating guy.

Shriver: Yes, he is indeed.

Yost: Tell me about how you went about launching the publication and what were your primary goals, and what resources did you draw on to make it successful.

Shriver: What was interesting was I was editor-in-chief of *Computer* and editor-in-chief of *Software*. But they were two very different environments. *Software* was a clean slate, tabula rasa. You were there, you have to define scope, you have to have an editorial board, how are we going to do, what kinds of departments are we going to have, you had to become familiar with the staff on the West Coast who was going to actually do the

production of the magazine — Marilyn Potes, True Seaborn, just absolutely wonderful people to work with. But I first called a number of the obvious respected computer scientists in software at the time and said look, this is a startup magazine, I'd like you to become a member of my editorial board. This is what I foresee as responsibilities and duties, don't sign up if you're not willing to sign up for these. I was really a stickler on quality reviewing. I just hated to receive feedback on my own work or one of my students' work which was superficial review, giving no insight into how to improve, whether or not it was accepted or rejected. That just was so irritating to me. I wanted *Software*'s initial reputation to be built on quality of feedback that you gave as an author, okay? And I said we can scope out all the initial topics that are obviously of interest in software right now; optimizing compilers and semantics and software engineering — which may be an oxymoron — but software engineering concepts and so on. I gathered a group of people together in my home. We sat down, six or seven of us, and said we'd take this on. We kind of designed the first three, four issues — it was quarterly initially — and got some really stellar articles out of the shooting gate, which is what you've got to do; and that then got the submissions coming in. And then the next year, added a few more people because a smaller group of us are more overworked than a slightly larger group of people. But I never was one for enormously large boards. I would rather attract a member to an editorial board by saying look, I expect you to do four substantial reviews a year; that's one every three months. I know you're busy doing research, or writing articles, or teaching courses, etcetera, but you can slip in one good review every three months. And furthermore, I'm going to ask you from time to time on topics, coverage, and so on. I gave them a specific set of responsibilities including you've got to write an

article but it may not be accepted. You got that? And in fact, they were rejected for many of them, okay? Simple as that. I tried to drive up the rejection rate as high as possible to get respect right out the door kind of thing. True [Seaborn] and Marilyn [Potes], I just can't say enough about them. We never would've got to where we were with the magazine without them pouring in enormous amounts of personal energy themselves. The Computer Society itself has typically been blessed by high quality, dedicated, focused staff; just really wonderful people. Then I tried to experiment with some ideas in terms of articles and departments, as normally every editor would do. You win some, you lose some; some ideas didn't work out and you pushed them to the side and based on the response, we then grew into six times a year and ultimately to a monthly. I wanted to do a companion issue with *Computer*, that's what I called it. I think it was Mike Mulder or someone before him that was EIC that agreed to do this, and I carried that idea over into *Computer*. What I meant by that was that I'd pick a topic [e.g.] compiler construction. There's both hardware and software issues related to this, and there's a bunch of different disciplines that come into effect. Why not have a series of articles appear in *Computer* which was kind of a main thrust that you wanted the general membership to see about these issues in compilers. And over here, in *Software*, do the detail articles about all the algorithms that are involved in this and that. And then when you publish the issue there in the very same month, people got two different things on the very same topic in varied levels of detail, okay? So when I got to *Computer* I actually tried one with three. I remember I did one with AI *Software* and *Computer*, tying together on artificial intelligence kinds of issues. They're obviously more difficult to put together. You're trying to coordinate three editorials, special issue editors together; [but] I think the payoff

is just enormous to the Society and the readership to have that coordinated attempt. So those are the kinds of things I played with as the editor. Now something also, you have to remember when I took over this editorship; we're talking the mid-1980s. The mid-1980s, how much e-mail did you send out and receive? A mind-boggling thing. I had some of the finest authors and board members who said send me the stuff by express mail; I don't do e-mail. This sounds like prehistory of some sort. The second year, I said if you're going to be affiliated with this, everything will be done electronically. I think we lead the Computer Society in the electronic management of the reviewing process and that type of thing. I remember that I kept both for *Software* and *Computer*, my own spreadsheet of reviewers. Remember, I have a hang-up about quality reviews.

Yost: Did *Computer* keep records on a reviewer database and rating the quality of reviews prior, or were you the first in doing that at *Software*?

Shriver: I think we were probably the first. And basically, I would rank the review, I would rate it, and many letters went out from our office for reviews that had little or no substance, saying to the reviewer, "Would this review have helped you if you submitted the article?" I didn't feel ashamed at all at sending the letter because I got something back that was of no value. Now, if a reviewer, over the course of a year, year-and-a-half, two years, got a review of eight or seven, I would write them a letter saying look, can I tell your department head or your manager what a great job you're doing for the professional society by the quality of the reviews? Of course, they would say immediately yes, here's the address, write the letter. It was a thank you to them for doing

a professional job, as requested. But I carried that practice over into *Computer*, which didn't have a good — from my perspective — did not have a good review process at the time; sometimes getting in nine or 10 reviews until I had four that had something of substance to give feedback to the authors on why I was accepting or rejecting the manuscript. And I encouraged the authors to work with the copy-editing staff to think of sidebars to include the multiple audiences of *Computer* that would be reading the article. But the article wasn't hashtagged or whatnot. Not everybody was going to be drawn into that stuff but the sidebars, you'd have enough examples and whatnot, they'll begin to understand well gee, this does impact somewhere some of the things they're dealing with.

Yost: Tell me what were your primary goals in running for first president elect then president of the Society?

Shriver: I guess I had just finished up my four years as EIC of *Computer*. Mike Evangelist, Yale Patt, Ted Lewis, and Harold Stone, I think, got me in a hotel room and said Bruce, we want you to run for president. I said I don't have the time, energy, or effort to do that. They said you're not getting out of this room unless you say you're going to do that. I said no, look for another candidate. So they disappeared and did the same thing the next day. It was a governing board kind of week where you're meeting multiple times. Every year there are issues of particular concern to the volunteers that are on the governing board about the way the budget is going or the way the growth of membership is going; there's a whole plethora of issues. And they said well, you've got to deal with these issues and those issues, which are generic to me so I don't remember

the specific ones at the time, but they said you can help us solve these or whatever. I said finally I would run, okay? And as fate would have it, I won the election. This was a particularly interesting time if you look at the dates, I was an independent consultant. I had no idea how many Computer Society presidents there have been where they didn't have the infrastructure support of a secretary and a university or corporation; I was running my business, my consulting business, while president of the Society, which was interesting; but it will certainly develop a different bond between you and your wife. [Laughter.] If you're doing those two things simultaneously.

It was an era of interesting expansion for the Society, if you take a look, again, at the years again in the late 1980s, early 1990s, the Society was growing in a number of directions. Its membership was increasing. Its relationship with the Institute — as it always is — is a money issue that somehow leaves you in the red. I remember every meeting I went to budget-wise, I had my red sweater on, saying we've got to get out of this, okay? And into the black. I had encouraged the development of several additional magazines and *Transactions*. So it was a wonderful time of being able to expand these services with a growing membership. Not every president obviously has had the benefit of being in such an environment and context such as that. There were challenges with respect to some particular conferences and the independence that some of them sought at the time, and their own control over the books and so on, but that happens in every president's administration. So we had to deal with those; some less pleasant than others. But I did manage to persuade a couple of initiatives like the multimedia initiative that Tadeo Ichikawa; so I have a lot of fond memories of working with people that were supportive and took a tremendous workload on themselves to carry through with some of

these visions ahead of us doing this, that, and the other thing. But the committee that I told you about at the end, the Central and Eastern European Initiatives Committee, was one of my fondest memories of the time. It was just the right thing to do. There was an enormous amount of effort to do it. My vision was [that] these scientists and engineers under the control of the Soviet Union didn't have access to the conferences, they weren't allowed to travel outside the country typically, only under unusual circumstances. I wanted to give them instant libraries, and books, and conference proceedings, so you had to work to get funding from industry and what not to do that. I wanted to acknowledge through the Institute and the Computer Society some of the finest engineers and scientists that would have been up for our own medals and honors in the Society had they not been behind the Iron Curtain. So I had to establish committees within those countries that could talk among themselves to evaluate who was the most important Hungarian or Polish, Slavic computer engineer or scientist; and how would you have documented that this was the award that you were going for? What credentials and support could you give that had they not been behind this Iron Curtain, that they could've been a candidate for this award. The formation of these committees was particularly difficult because when you went to these former Soviet countries in that timeframe, they weren't really on stable footing. The factions within the countries that were trying to help them economically, [to] gain some sense of stability, some sense of governance, and so on. So you had some of the old Soviet hardliners that had been in the Soviet Academy of Sciences. You had some of the young Turks that were happy just to throw the shackles off their back that came our way. You had some people that suffered through the entire thing and they were in the colleges and universities but had been under terrible constraint by the Soviet Union,

etcetera. I found basically three or four factions and my algorithm was assign a couple from each, and then make a judgement call on a person who you've established a rapport with somehow, either an earlier connection in architecture or whatnot, who could chair the committee. Let them know what the awards are, let them make the recommendations. So I did that. It was a year-and-a-half to two-year process. And I ultimately got the list of recommendations and as you might suspect, a number of the people were dead. We're talking 50 years; I wanted them to look at who really made an insightful [contribution] that's affected the way you think about doing these things. So I wound up with maybe the names of eight or 10 dead scientists and engineers; and when I went to the Institute, the IEEE Awards Board said, we don't give awards post mortem, forget about that. I said forget about that?! Are you listening to me? These people were subverted for 50-60 years; give me a break. It took me a year of arguing with the IEEE Awards Board to finally say okay, we're going to give a one-time blessing. I said look at these people; look at these publications and journals. Sure, we didn't see them, but you see the quality etcetera; all the arguments that you would give, right? So finally they approved them but it was just satisfying getting that done, taking that effort. So that wasn't during my presidency, it was the following years after. One of the other things that I found very satisfying was starting the international student design competition. For years and years, I had looked at that ACM programming competition and what got me aggravated was computer science is not just programming. In fact, it's not programming; programming is something you do because you're writing simulations, or you're doing this, that, or the other thing. It doesn't define the field and I got really edgy about it when anybody would mention it to me. I said dammit, we need a design competition where our students are

designing a system. Now, I look back at this, because that was in the mid-1990s or something; I look at it as the beginning of the Maker Fairs, okay? Because what I managed to do was I went to a couple of corporations, five or six, and I put together these components for a kit and I said okay, you're going to design, and we gave them some parameters and some criteria, and they had to bring a working model back to the competition. I just found that very energizing and it was a lot of fun. I thought at the end of three years that I had it stable enough that I could hand it over to someone else. Alan Clements, I think had taken it over. I don't know if it's still alive, I hope it is. I just thought that was a lot of fun to start up.

Yost: You mentioned ACM; you were active in both ACM and of course, Computer Society. As president, did you make it a priority to try and partner and collaborate with ACM?

Shriver: Yes. There's been dialog about such collaboration ad infinitum. We made our attempts. During the time, we had a particular negotiation when I was president about one of the conferences that we were working on together. I don't recall the specific conference but they were just as we were, growing at the time so there was little that you can justify at the time to your own management to think about merging activities if indeed you're both having dynamic growth. The main collaboration continued to be in co-sponsoring workshops, and conferences, and seminars, and co-publishing proceedings of such. They often relied on us for publishing the proceedings of co-sponsored meetings and that type of thing. But I don't know now, since I'm not on the Board of Governors,

how the relationship between the two organizations is going; whether it's in a similar position or more collaborative in nature. One would hope that they are collaborating on the kinds of things that will benefit the members, too. It's not clear to me that one organization absorbing the other [whether] that kind of thing would be beneficial to the communities. They're basically different communities of people that are joining these organizations.

Yost: In your three years of leadership, 1992 through being president, can you talk about your interaction and work with senior Computer Society staff, and specifically, Michael Elliott?

Shriver: Michael was an incredibly gifted person with wonderful insights into such a range of issues that professional societies had to deal with, and he generously offered his insights, often telling me what his opinion was but not necessarily requiring you to adopt that opinion, which I find excellent. He gave you the plusses and minuses. He certainly had significant insights into the Institute, which I didn't, which allowed not just me, which allowed Helen Wood who came before me, and Jim Aylor who came after; the presidents Jim Aylor and Duncan Lawrie before, we kind of formed a bond of like four presidents working together. So we all leaned on one another, learned from one another kind of thing. Michael would quite often be with the four of us sharing insights as to why this particular dictum came down or what he had been dealing with; but Michael and Anne Marie Kelly who was head of the conferences and tutorials, and-Violet Doan, the

main administrative person in Washington. All these people were just wonderful to deal with — very understanding, very helpful — you couldn't ask for a better support staff.

Yost: Were there any things that you hoped to get accomplished that you weren't able to see through in what was a short term with the Computer Society?

Shriver: I don't believe so.

Yost: You've remained active in the Computer Society after the years as president. Are there some experiences you want to share about some of the roles you've had?

Shriver: I shared earlier some of those. The Central and Eastern European Initiatives Committee, the International Student Design Competition, I represented the Society in IFIP and in FOCUS, and these international things which I told you had a tremendous influence on the work we did with the Liddy Shriver Sarcoma Initiative. Over the course of those years I had to write letters of recommendation for scores of people for positions, for endorsements, for IEEE Fellow nominations, etcetera and those you take on more than willingly to do that. You obviously have to know the person and be able to write truthfully about the person you're representing. But I've been gratified that people have asked me to do those obviously, and I've been happy to do that. Just recently, Sumi, the editor-in-chief of *Computer* asked me to be on *Computer*'s advisory board. So I told him that I would accept the position. I don't know the value of what I'm going to be able to render him a service, but I came to the meetings this week to do that.

Yost: Before we conclude, are there any topics I haven't mentioned, questions I haven't asked that you'd like to get on record?

Shriver: I can only speak of my experience at the time because I haven't been involved in the interactions with the staff for some number of years. But during the 1980s and 1990s, the Society had one of the best structures of staff working with volunteers I think I've ever seen in a professional society. They really worked to complement one another, and what they brought to the table in terms of responsibility, duties and obligations, and services. That just doesn't happen in lots of societies. There is often a power structure issue and budget control issue, and people exerting themselves in a not particularly constructive way. I never found that during my years of involvement all through the 1980s and 1990s. I certainly hope that would still exist today because it really is a model that other societies could follow. One thing in addition to this business about my obsession with quality reviewing is also I believe about the young people becoming involved. During my 10-year tenure, you'll see that we passed a two-year term limit on positions; editorials and so on. I carried that over to Liddy Shriver Sarcoma Initiative as well in terms of editorial board appointments and so on. I think that's absolutely critical to the vitality of an organization, and you do see professional societies in so many disciplines where the old timers are the hangers-on, okay? You don't see a lot of new blood; it's the people who have been there for 30 years. You've got to give the young people an opportunity to get in and set their own directions for the society and so on, okay? So I hope that is happening and that there is a good turnover of new insights.

That's why I was a little bit reluctant to take this position with Sumi, is that I'm old school, so to speak. [Laughs.]

Yost: Anything else?

Shriver: No.

Yost: Well thank you so much, Bruce. This has been terrific.