Oral History of Robert (Bob) W. Taylor

Interviewed by:
Paul McJones

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McJones: My name is Paul McJones. It's October 10th, 2008. We're going to conduct an oral history interview with Bob Taylor, and we have, behind the camera, Ken Beckman. Hello, Bob.

Taylor: Hello, Paul. Hello, Ken [Bob's friend and colleague at Xerox and DEC].

McJones: Bob, I thought it would be good to start at the beginning and have you tell us a little bit about growing up, your parents and early school and other experiences like that.

Taylor: The very beginning? All right. This may be a little complicated. I was adopted when I was 28 days old in San Antonio, Texas. I was born in Dallas, so somehow or another, I don't remember how, I moved, during my first 28 days of life, from Dallas to San Antonio. I was adopted from an orphanage, in effect, by a Methodist minister and his wife. They could not have any children of their own. They didn't have RH negative things figured out in those days, so they adopted me. And during the first few years of my life I lived in small Texas towns where my father was a Methodist preacher. And then when I was about four, I guess, something like that, my father became a professor in a small Methodist college in San Antonio and we lived there for ten or eleven years. During World War II, he was in the Chaplain's Corp. and was overseas. And when he came back, he went back into the ministry and was located in South Texas where I finished high school. I then went to college.

Taylor: So I was born in 1932 and lived in various Texas towns. I might as well mention them: Uvalde, Ozona, Victoria, and then San Antonio, which is not such a small town. In 1946, we moved to Mercedes, Texas, which is down in the Rio Grande Valley on the Mexican border. In 1948 at the age of 16, I graduated from high school there and went off to college in Dallas at SMU [Southern Methodist University]. Three years later, I was in the Naval Air Reserve and the Korean War occurred, and I was put on active duty at the Dallas Naval Air Station, referred to affectionately as the U.S.S. Neverfloat. And after 22 months of active service, the Korean War was over so they released us two months earlier than they would have normally and I went back to college, this time at the University of Texas and this time as a more serious student than previously.

McJones: I understand your mother was a teacher?

Taylor: Well, she did do some teaching, but mostly she was, when I was there, after I came into her life, why she was mostly just my mother. My dad was a minister and then for a period of a few years was a professor in a small college, and then was in the Army as a chaplain during World War II and then went back into the ministry after that.

McJones: So education was a definite theme in your family?

Taylor: Yes. My dad has a Bachelor's degree and a Divinity degree, and then he's got a Master's of divinity from Yale. His other college was in Texas, at the University of Texas and SMU, but he got a Master's at Yale before he came back to Texas and went into the ministry. And then he and my mother, before I was born, were teaching missionaries in Brazil for five years, and then they came back to the states, and that's when I was adopted. So yes, education was important to them.

McJones: And you were able to graduate from high school at the age of 16 so I assume you got good grades?

Taylor: Well, what happened was <laughs> let's see. When I was four or five years old, my parents sent me to a special school that was an experimental school run by this college that my dad taught at. This was an unusual school. The school didn't last for very long, a few years, I guess. And it was pre-public school so I went there, I think, from the age of four and five or something like that. And at this school, among other innovations, they would conduct for a one-week period, they would conduct everything, all the classes and their playground activity and their lunchtime activity in English. And the next week they would do it in Spanish. The next week they would do it in French. The next week they would do it in German and then they would go back to English, which was rather amazing. Now, today I don't know any of those languages, but I gather at the time I had a five-year-old, or maybe a three-year-old's vocabulary in some of them. And years later, when I would be in Germany or France, if I was in those countries for as long as a week or two, phrases would come back to me. I mean, I would utter some phrase and then I would ask myself, well, how did I know what that meant? It's a very strange phenomenon. So by the time I started to public school at age six, I knew how to read, from my previous schooling. And furthermore, in this small college where my dad taught, there was a Psych professor whose field was intelligence testing. And he was lecturing to his class one particular period of time about intelligence tests, and he asked my dad if he could use me as a Guinea pig in front of his class and give me an intelligence test in front of the class so he could show the class an actual test underway. So my dad said, "Oh, sure." And so they did that, and the professor was surprised at the score. There's no point in me mentioning it except that it comes in later when I'm going to public school. The results of the test made the front page of the San Antonio Express, which is their local newspaper, and with a picture and all. And so my mother <laughs> takes me on the first day of public school and she goes in and shows this teacher, kindergarten teacher, I guess. "Now, my son already knows how to read and I just wanted to let you know. And there's this thing in the paper." Sounds typical of a mother, right? And so the kindergarten teacher handed me this kindergarten book that they were learning to read out of, "Run, Spot, Run," or something like that. And I read it and so the kindergarten teacher said, "Well, you better take him to the first grade." So my mother, after about an hour in kindergarten, why we go to first grade. And this story repeats itself. And now I'm in the second grade, but I'm only six years old and the people in second grade are eight years old. And the teacher says, "Well, you could take him into third grade, but I could give him special work if you want to keep him in the second grade because he's already two years younger than everybody else." And so they kept me in the second grade and that's why I graduated from college two years ahead of my time.

McJones: Then you spent several years at SMU before the Korean War and several years after?

Taylor: And at SMU I was not a serious student, but I had a good time.

McJones: You were taking a lot of classes but maybe not as much focus?

Taylor: No, I didn't take very many classes. I mean, I played. I was 16 and away from home for the first time, 500 miles away from home and I loved athletics so I played a lot of sports. I was too small and young and light to make the college teams, but I played lots of intramural sports; football, basketball, softball, stuff like that, and had a good time. But then I went in the Navy and, as Herman Wouk says,

"The Navy is a master plan designed by geniuses for execution by morons. You want to get along in the Navy, throttle your mind down to a crawl." You're not smiling. He actually did say that in "The Caine Mutiny." No, some other book. Maybe it was "The Caine Mutiny." And it's true. So while I was in the Navy I did a lot of reading and got interested in philosophy and science. And so when the Navy period was over and I went back to school, I was a little more serious as a student. I still didn't know what I wanted to do. I didn't even have a plan for a career major or anything like that. I was on the GI Bill and I just took courses that I was interested in. I had a job in a research lab as a research assistant as well as the GI Bill, and I could have just stayed there forever as a professional student.

McJones: Was your research position in experimental psychology?

Taylor: Sort of. It was in acoustics and especially psychoacoustics, but it was in a defense research lab and so we were doing research on hearing and how the human nervous system can localize sound and things like that, and masking of signal by noise. And we were also doing some work on developing sonar, which leads to a story as to an event when I was more afraid than I've ever been in my life. Since we're there, I might as well tell that story. In our sonar work, we had a floating lab sort of, out on a large lake outside of Austin, Lake Travis, and it's a lake that's created by a large dam. The lake is about 200 feet deep at its maximum, and one of the researchers who worked on this lake station, so to speak, was a former Navy scuba diver and he trained me in scuba diving. We would go down into this lake and plant simulated mine fields, large canisters of metal of various sizes and shapes, and put them in the bottom of the lake in order to test various sonar devices that we were experimenting with. Now, when you're down about 100 feet or 150 feet or 200 feet, which is about as far as we could go with scuba equipment in those days, it's very dark. You always go down with someone else. I was always down with this guy who taught me how to scuba dive. So you can't see anything, really. I mean, you can put your hand in front of your face like this and see that, and you kind of know where he is. Sometimes you can hear his bubbles going up. You know he's not far away. And so I was down doing whatever it was I was trying to do and I felt sort of a tap on the side of my head. I had a mask on so I didn't have any peripheral vision. And I figured it was him so I turned to look at him and there was a huge carp sitting right here. I have never been so frightened in all my life. First of all, I didn't realize instantly what it was. Carp are among the ugliest of fish. And I think about it today, I'm still <laughs> now, the carp didn't intend any harm, you know, he was just curious. But that was really scary.

Taylor: So I had to contain myself because you can't go up faster than your bubbles or you'll get the bends. But I knew I needed to get out of there and I couldn't find my buddy, so I just gradually sort of froze. And I looked around again and the carp wasn't there anymore and so eventually I composed myself and came up.

Taylor: In contrast, I'll tell you another story, just because it comes to mind. The time when I should have been really scared out of my wits and wasn't, and I don't know why, just stupidity is the reason I wasn't. It was a job I had in the summertime when I was in college or this may have been my last year in high school. I worked for a neighboring rancher. I lived in a small town and there were ranches outside of town. This rancher had a small herd of cattle, like maybe a dozen cows and a couple of bulls. And he needed someone to take them in the morning and herd them out a few miles to where there was a water supply, across some other people's land to get to the water supply, and let them water and feed and so on, and then bring them back in the afternoon, on horseback. So this is sort of a cowboy's job in a minor way. No branding or roping or anything like that, just taking care of a small group of cattle. But in this

herd there was a Brahma bull, a large Brahma bull, and he was a very passive, lethargic beast, and moved along just very ploddingly and slow. And so the herd could only move as fast as he was moving actually. My horse did all the work. I mean, if an animal would roam out off the path where we were supposed to go, well, the horse would just go and get it back. I didn't have to do anything. We would come to a gate, the horse would come up to the gate and turn sideways to the gate so that I would reach over and unlatch the gate <laughs> and the horse would move sideways out of the way, the gate would open and the cows would go through and the horse would come back and I would close the gate without ever having to get off the horse. An amazing animal. So one day we were on our way either to or from, and we were in a citrus grove. This is in the Rio Grande Valley, which is the citrus belt of Texas. This citrus grove was a mature citrus grove and had very large citrus trees. I don't remember if they were grapefruit or orange trees, but they were large. Citrus trees don't grow very tall, maybe 20 feet, something like that, but they can grow very wide and very low. In this case, the trees had not been trimmed excessively above ground, so the branches came down very low to the ground and very wide and so while we were in this citrus grove, just by happen stance, not because we were in the grove, it just happened to occur to me, that I thought I'd try to ride this bull. And so <laughs> because the bull just was so passive, so lethargic. So I thought it might be fun so I got off my horse and I got on this bull. And he just stood there, didn't do anything. And so I kicked him and he still didn't do anything. I just wanted him to walk, you know? So I kicked him again about as hard as I could and the bull exploded. He just went "boom." And I went flying off and landed on the ground near one of these citrus trees, and the bull came after me. If a horse throws you, he'll avoid stepping on you. He'll try not to step on you. If a bull throws you, he'll come after you with everything he's got; his hooves, his horns, anything he can. You've probably seen bull riding in the rodeo. They're serious. And Brahma bulls are huge. And I knew this bull was coming after me and so I just rolled on the ground instinctively and I rolled underneath the citrus tree up against the trunk of the tree. And the branches coming down out of the tree kept the bull from getting to me. I just lay there. I wasn't scared, I just thought, "Well, okay. I guess finally we woke him up." I was kind of pleased about that <laughs> and I recovered, the bull recovered. I got back up on my horse and we went on our way. Much later, I realized <laughs> that should have been my last moment, you know? Because it was a stupid thing to do <laughs> because there was nobody within miles. So the contrast of being afraid when there is no need, and not being fearful when there was a serious need, it's something I've thought about from time to time.

McJones: You were describing that after the Navy you were a much more disciplined student.

Taylor: More serious, anyway. I'm not sure about disciplined.

McJones: Were there specific professors that were a strong influence during that period?

Taylor: Yes. While at Texas I finished a Bachelor's degree and went on and got a Master's degree in psychology. I had a teaching assistantship in the department, and they were urging me to get a PhD, but to get a PhD in psychology in those days, maybe still today, you have to qualify and take courses in abnormal psychology, social psychology, clinical psychology, child psychology, none of which I was interested in. Those are all sort of in the softer regions of psychology. They're not very scientific, they're not very rigorous. I was interested in physiological psychology, in psychoacoustics or the portion of psychology which deals with science, the nervous system, things that are more like applied physics and biology, really, than they are what normally people think of when they think of psychology. So I didn't want to waste time taking courses in those other areas and so I said I'm not going to get a PhD. So I

completed a minor in math and a minor in philosophy. I already had a minor in English because of my other schooling, and finished my Masters and did a thesis in psychoacoustics on sound localization. And my thesis adviser, to get to your question, was a pretty well-known figure in acoustics research by that time. He was a good friend of a younger man named J.C.R. Licklider, who played a key role later in my life, but whom I had not yet met. I knew of him because he was very famous in psychoacoustics. My thesis adviser was named Lloyd Jeffress[1] and he was influential. And some of the research projects I worked on were in the lab where he was head of this group. There was another professor named Hugh Blodgett[2] who was famous for research that he had done in the learning area years ago, and he was a strong influence. These were both really not only very smart men, but they were fun to be around and had good senses of humor. They were just a good influence. I had some other good teachers too, but those two stand out.

McJones: At that point you were married?

Taylor: Yes. Yes, I got married while I was at the University of Texas, maybe a year or two before I actually finished my Bachelor's. And in the fullness of time, we had three boys.

McJones: That was part of your motivation to stop being a perpetual student?

Taylor: That's right. I finally had a reason to no longer be a professional student. The father of a good college friend of my wife was head of a prep school in Florida and asked me if I would come and teach at this school. So that was my first regular job out of school, apart from the jobs I had while I was in school. I taught in this prep school for a year, including a summer, I guess. I taught their math courses; introduction to calculus, algebra, trig. A course I really enjoyed was spherical trig; a course I had never taken, but I got the textbook and that was a lot of fun. You could work lots of interesting problems with spherical trigonometry. I taught an introductory course in philosophy using Will Durant's "Story of Philosophy", and taught an introductory course in sensory psychology using a book by D.O. [Donald Olding] Hebb, who was a famous Canadian experimental psychologist. And I coached the basketball team. That was fun. Oh, and we were dorm parents.

McJones: Was that an average teaching load?

Taylor: Well, it was sort of a full day, I guess. Yes, I suppose.

McJones: But in a few of those cases you were definitely needing to go home and prepare lectures--

Taylor: Well, you don't really lecture in high school. You try to engage the students in a way rather than lecture. I guess some of those courses were just single-semester courses because they wouldn't all fit into one day. But the math courses were continuous both semesters. Then at that time we got free room

[1] In Memoriam: Lloyd Alexander Jeffress. http://www.utexas.edu/faculty/council/2000-2001/memorials/SCANNED/jeffress.pdf
[2] In Memoriam: Hugh Carlton Blodgett. http://www.utexas.edu/faculty/council/2000-2001/memorials/SCANNED/blodgett.pdf

and board because were dorm parents. This was a coed school. We were parents of the boys' dorm. But the salary was $3600 a year and I already had one son and we had two more on the way—but we didn't know that there were going to be two of them at that time—so I thought I'd better look around for another job. I found a job as a systems engineer at The Martin Company in Orlando,—Florida—and more than doubled my salary overnight. I worked there for a year on the Pershing missile system design. I had some interesting experiences doing that. I was a systems engineer, and, one of the things I was responsible for was putting some of the support equipment for the Pershing through cold tests. Pershing had to work at minus 65 degrees Fahrenheit. At that temperature, your eyeballs can freeze if you don't blink your eyes frequently. There was a cold test chamber north of Philadelphia, at I think a Navy installation. So we shipped some of these parts of the missile system like the turbine that would get it running and so on, on the ground. Some of the equipment had to stay on the ground and had to be operable. We shipped them up to this cold test lab, and then I would put on all of this cold gear and go into this lab and run this equipment, start it up and make sure it worked in the cold weather and make sure that I could get my hands with heavy gloves on various things, switches that I had to throw or knobs that I had to turn, things like that. So it was a cold test for the humans as well as for the equipment. That was interesting. I'd never done anything like that before.

McJones: That was around 1959?

Taylor: That would be probably 1960.

McJones: Did you work for two different companies between teaching and going to NASA or just one company?

Taylor: Two. I was at Martin for about a year, I guess, Martin Orlando. And one of my colleagues there had gone to work for a flight simulation firm in Riverdale, Maryland just outside of D.C. They were called ACF Electronics. They built a wider variety of flight simulators than any other company in the country. They made me an attractive job offer and we moved up there and I went to work for them. While I was there, McNamara, who was with Kennedy, who had just been elected president, was Secretary of Defense, standardized or attempted to standardize many of the aircraft across the Navy and the Air Force. Consequently, a company that based its reputation on building a wide variety of flight simulators was no longer quite so important because we were not going to have as wide a variety of airplanes. So ACF had been looking for other things to do and NASA was just opening up. I was encouraged to write a research proposal to NASA using flight simulators, but in a research context rather than in a teaching context. For example, one of the flight simulators that the company built was an anti-submarine warfare airplane flight simulator where you could simulate all of the stations in the airplane that were monitoring various sensor devices that were looking for submarines. There was a lot of display technology to fool around with and things that NASA was interested in. So I wrote this research proposal and sent it in. NASA called me and asked me to come down and talk to them. I thought they wanted to talk about the research proposal. And instead, they offered me a job in their newly-formed office of advanced research and technology. The job of this part of NASA was to fund the NASA research centers and also fund limited research in limited areas in private industry, and sometimes universities. Then they put me in charge of two research areas to manage the funding that went into these areas, whether it was the NASA center or in the private world. The areas were manned flight control systems and flight displays. While I was there I created another area called simulation technology. So I managed research in those three areas while I was at NASA. One of the unsolicited proposals that came in was from a guy named

[Douglas C.] Engelbart at SRI.[3] I thought it was an interesting proposal and he came into D.C. on his round of looking for money, and we talked. I funded his proposal and the mouse was created by NASA funding. Most people don't know that. Remember when NASA was advertising Tang as its big contribution to the civilized world? Well, there was a better example, but they didn't know about it.

McJones: Was that the beginning of Doug Engelbart's major funding?

Taylor: He had some smaller amount of funding I think from the Air Force, but the mouse work was actually done under a NASA project. And then when I went to ARPA, I funded it more, but Licklider had funded him as well, as a result partly of my funding.

McJones: Maybe this would be a good place to stop briefly and talk about computers and computer technology entering your consciousness and life. At this point, clearly, some of the displays and things like that are related to things that will be involved later. I gather up until this point you had minimal impact with computers, *per se*?

Taylor: In the 1950s when I was working on my thesis in acoustics, I had to submit my data to a statistical tool called "Factor Analysis." There's a lot of computation involved, depending on how many dimensions there are in your factor analysis and how large each dimension is. So I went to the computer center at the university to explore ways in which I could put this data into a factor analysis program and run it on the computer, because it does require lots of computations. They introduced me to the card punch machine, which I didn't know about before. I said, "You mean I have to sit down and punch holes in these cards to get my data in, and then I have to take the cards over to the computer and I give the cards to a guy who runs the computer and I go away and come back and get the results on a long printout of paper?" And they said, "Yes." I said, "I'm not going to do that." In fact, the whole notion of people sitting in key punch rooms just really, I don't know why, just irritated me. There's got to be a better way to use those things, but I didn't know what it was. So I went back to my lab and I keyed in all my data on a Monroe calculator. But I didn't have to deal with card decks and long reams of printout and so I get a number, I write it down and I put it in my thesis and it's done. About a year later, and by this time I was working in Orlando for Martin after my thesis was done, I read an article in *IEEE Transactions on Human Factors and Electronics*. In this journal there was an article that is now very famous written by J.C.R. Licklider entitled, "Man-Computer Symbiosis."[4] And in this article he outlines how a human being and a computer can form an interactive partnership. When I read it, I just lit up. I said, "Yes. That's the answer to my key punch dilemma. That's worth working on."

McJones: And this is the second time you were hearing about Licklider?

Taylor: Yes, right. But in a totally different context. He discovered computers maybe two years before he wrote that article. Wes Clark and Licklider were both working at Lincoln Lab in the late 1950s, 1958, I

[3] Doug Engelbart. The augmented knowledge workshop. In *Proceedings of the ACM Conference on the History of Personal Workstations* (Palo Alto, California, United States, January 09 - 10, 1986). J. R. White and K. Anderson, Eds. ACM, New York, NY, 73-83. DOI= http://doi.acm.org/10.1145/12178.12184

[4] J. C. R. Licklider. Man-Computer Symbiosis. *IRE Transactions on Human Factors in Electronics*, volume HFE-1, pages 4-11, March 1960

think this might have been, and in different departments. Wes had designed and was working with the TX-2[5], which was a famous computer built at Lincoln on which Sketchpad was built by Ivan Sutherland, and Lick was working down the hall doing his psychoacoustics work. Wes passed by his office one day and Lick was in there poring over some figures. Wes walked in the office and introduced himself and asked Lick what he was doing. Lick explained to him that he was analyzing some acoustics data, and Wes said, "Well, you ought to be using the TX-2." Lick said, "What's that?" Wes took him down the hall and introduced him to computing. That's how Lick got hooked on computing. Wes later became a mentor and good friend of mine and consulted with us at Xerox and at DEC.

McJones: We'll try to weave that topic in later.

Taylor: Anyway, Wes had been a principal designer of the TX-2 and is a relatively unknown but a crucial and strongly contributing pioneer to computing.

McJones: So for example, you talked about the difficulty of doing the batch-style computing a few years before. Can you say a word or two about what Lick faced when he learned to program the TX-2? It was still pretty primitive, as I understand it.

Taylor: Yes, but he was sitting at a display and a keyboard, and it was interactive. There were no cards punched for the TX-2, as far as I know.

McJones: Do you know about the kinds of software; did he have a higher-level programming language?

Taylor: That's a good question. I probably did know the answer to that once upon a time, but I don't remember now. It wasn't an assembly language, I think, but I don't know what it was. It probably wasn't Fortran either, but it might have been, I don't know. But there was some other point I was leading up to. Now I can't remember what it was.

McJones: So after you did read the Man-Computer Symbiosis paper-

Taylor: Oh, right, that's where I was. So yes, I was enthusiastic about that objective, making computers interactive. But the first opportunity I had to do anything about that was, I guess, a year later, yes. It must have been a year later when I was at NASA and ran into Doug Engelbart, because that's sort of what his--

McJones: His program matched very closely…?

Taylor: That's right, that's right. That was sort of closely related to what he was trying to do and I funded him. So at that point, then I could sort of be involved in this long-term objective that I thought was worth working on.

[5] http://www.bitsavers.org/pdf/mit/tx-2/

Taylor: In terms of what I was going to do with the rest of my life, reading that paper by Lick pretty much determined it.

McJones: So during your NASA period as I understand it, you did meet meet Lick [Licklider]?

Taylor: ARPA was formed as a result of Sputnik. We were surprised by Sputnik in October of 1957. In 1958, Eisenhower asked Secretary of Defense, whose name I can't remember [Neil H. McElroy], to create an agency in the Defense Department that would be a quick-reaction agency that would fund long-term research so that we would not get, hopefully, surprised again the way Sputnik surprised us. We would have some research investments out there in various promising areas that would militate against that surprise. And so ARPA, as a result, was formed and created the space program. There was no NASA in 1958. So the first space launches and so on were sponsored by ARPA. In 1960 or 1961 maybe, somewhere in there, NASA was ready to take on some programs of its own. [Actually NASA was established in 1958 by Eisenhower.] And those ARPA programs were transferred from ARPA to NASA. Now ARPA was looking around for other things to do and they decided to hire Licklider, who was at Bolt, Beranek and Newman by this time, to come down and start two research programs, one in behavioral science and one in what then was called Command and Control, but which later became Information Processing Techniques, and which fundamentally was computer research with an emphasis on interactive computing; creating and developing interactive computing. So Lick did that. And when he got to ARPA, shortly after, he looked around at what else was going on by way of government funding and having to do with computing, and he discovered various people in the government who were funding computing research, and I was one of those people. He invited us all to come to his office so we could meet each other and we could talk about what each of us was doing, so we shared some information in that way. And so I, of course, knew who he was but I don't think I knew that he had come to Washington until I got this invitation. So I went over with great anticipation, and as soon as I walked into his office he introduced himself and started talking to me about my thesis <laughs> which blew me away, of course. And so we became really good friends from then on until he died in 1990.

So that's how I met Licklider. And then Licklider, after a year or two at ARPA, decided to go back to New England. Well, actually, I guess IBM hired him next, away at Yorktown Heights. He didn't stay there very long, understandably <laughs> and ARPA hired, at his recommendation, Ivan Sutherland to take his place. He and Ivan then recruited me to be Ivan Sutherland's deputy. I think that was probably because Ivan had trouble finding other people whom he knew. He didn't know me. There was no reason why he would look to hire me first, but I think Licklider must have had some influence on him. And so that's how I wound up at ARPA. After about a year or so after I was there–I went there in 1965–a year or even less, Ivan left and went to Harvard as a professor. Actually, he was spending a lot of time out at NSA with a research group even while he was still at ARPA, so I was kind of running the ARPA thing at Ivan's request while he was out at NSA doing other things. And then I became director of that office officially, I guess, in 1966. In February of 1966 I went in to see the head of ARPA, Charles Herzfeld, and said I'd like to build a network that would connect these timesharing systems that Lick had set up in various places around the country. And Herzfeld, who kept up with what Lick had been doing and what Ivan had been doing and what I had inherited, understood right away what I was talking about and he took a million dollars out of one of his other programs and he put it in my program and we were off and running on the ARPANET.

McJones: So that was one of your first actions when you had become director and it followed an initiative that Lick had started talking about —the Intergalactic Network.

Taylor: Lick and I never talked about the intergalactic network. That phrase came out of a memo that he wrote. I didn't interpret that memo at the time to mean that he was talking about a computer network, but rather a network of his principal investigators, who were scattered all over the place, all over the United States anyway. But there was some reason to believe that he may have indeed been talking about a computer network as well, albeit, it would not be a network of personal computers. It would have been a network of timesharing systems, which is all we could do at that time. We couldn't build a network with personal computers until after we did the work at Xerox, which you were involved in, and Ken Beckman.

<Approximately 3 minutes of silence>

END OF TAPE 1

[While Beckman changed the tape in the video camera, Taylor and McJones chatted about early personal computers…]

Taylor: … and compared to an Alto they were pitiful.

McJones: Right, although there was some crossover point I think kind of in the late 1970s where VLSI [Very Large Scale Integration] was starting to get enough horse power. So clearly your world had this tremendous impact on what came to pass. But in some sense there was this other-

Taylor: What was their contribution?

McJones: A zeal to get it out as a mass product for one thing. If you think about it, all those people were building both hardware and software and driving the price down starting in about 1975, so while the work at Xerox was going on, they were actually bootstrapping an industry, so by the time the IBM PC actually came out-

Taylor: And what year was that?

McJones: Well the PC didn't come out until 1981.

Taylor: That's what I thought.

McJones: But there had been machines, Apple IIs, and CP/M machines, for several years that would have a hard drive that you could run small business on, even pre-PC.

Taylor: I thought about the only thing you could do with an Apple II was a spreadsheet.

McJones: That was the main thing, but for example you could get-

Taylor: Did it have an editor?

McJones: Oh yes, there were simple little-

Taylor: A word processor?

McJones: Very simple word processors and everything.

Taylor: Yes?

McJones: We can come back to it later.

Taylor: No, that's fine.

[At this point McJones and Taylor returned to the official oral history.]

McJones: Okay, so we're all back again and we were just talking about the early ARPA days and how ARPANET first got its funding then the intergalactic network idea. Your sense of it was more the people networking side.

Taylor: Yes.

McJones: The people networking and the hardware networking should go hand in hand.

Taylor: Yes, my primary motive for wanting to do the ARPANET was because I had realized that at any individual single place where a time-sharing system was built, that system was instrumental to the formation of a community. So people, through this mechanism of the computer, would learn of one another and of one another's software, data or programs, that they might use; one another's interest as it was expressed by what they were doing with the computer. That was a sociological phenomenon. And so why not network these time-sharing systems nationally? Let's expand this sociological phenomenon. That was the main reason I wanted to do that.

McJones: I remember you had a series of separate terminals in your office.

Taylor: Which served to reinforce this notion, because if I was going to use one system, I had to be at this particular terminal. If I wanted to use another system I had to get up from this terminal and go to a different terminal. That brought to your consciousness right away, "Well this is silly; I should be able to access any of these systems from a single terminal."

McJones: So not only the convenience factor, but then as you had just said before, you were curious about what this would do to the nature of those communities as they interconnected?

Taylor: Right.

McJones: Okay, so at that time that was you said early 1966 when you got the [approval to initiate the ARPANET project]

Taylor: February of 1966.

McJones: And there had been up until then just some very simple point-to-point networking experiments.

Taylor: Well in 1965 I had asked Lincoln Lab and SDC to run a bit reliability experiment; to ship bits back and forth and look at error rates, so I'd get some idea of the feasibility of interconnecting time-sharing systems. The TX-2 had a time-sharing system on it by that time, and the Q32 at SDC had a time-sharing system. I was funding both of them, so it was possible for me to ask them to perform this experiment. And they did. I asked Larry Roberts to run it. He was at Lincoln Lab at the time, and he said he would. I can't say he was all that enthusiastic about it, but he said he would do it. And it turned out he subcontracted it to Thom Merrill at the Computer Corporation of America, who actually ran the experiment. And of course it required SDC's cooperation, which was given because I was funding that work. The results of that showed that, just in terms of reliability of bits being sent a long distance over telephone lines, it wasn't a show-stopper. And of course then, you know, after that there are all kinds of other techniques, there is error correction and packet switching, and so on that can apply.

McJones: But you were just looking to take the raw-

Taylor: Yes, we were looking for raw bits. There was no concept of packet switching by us. Packet switching had been invented in 1962 or 1963 independently by Paul Baran in the United States at Rand and by Donald Davies at the National Physical Laboratory in England. But nobody had built a packet switch network.

McJones: So, in fact Larry Roberts comes back into the picture again.

Taylor: Yes, and so after Herzfeld gave me the project go ahead, I began to do two things. I began to talk to various ARPA contractors about becoming members in this network, some of whom were enthusiastic, and some of whom were definitely unenthusiastic. And then I began to look around to hire someone who'd be a Program Manager, because this was going to be a unique project within our office. All other

projects up until this time had been implemented as a result of unsolicited proposals. In this case we were going out with an RFQ [Request for Quotation] to do a specific thing which we were defining, not asking the recipient to define. And our office had never worked that way before. So I needed a Program Manager to do just that, and I also needed a financial guy who was familiar with government contracting and government contractors, to ride herd on those aspects of whoever wound up building the infrastructure for the ARPANET. Turned out to be Bolt, Beranek and Newman [BBN]. So I hired a fellow from the ARPA Program Management office. All the different ARPA offices shared a single Program Management office to help them with the legal and contractual and financial aspects of whatever it was they were doing. So I hired a guy to come out of that office to come work in my office for me. His name was Al Blue and he made a big difference in making all this RFQ business go smoothly, making sure we went through the evaluation process according to government rules and regulations and all that sort of thing, which could be very troublesome or complicated if you didn't have somebody who knew how to go through it. Wait a minute; I'm getting ahead of myself.

These efforts that I just mentioned, not the RFQ yet, were started in February of 1966 and shortly thereafter, sometime in March probably, I decided to get Larry Roberts to come be a Program Manager, so I asked him to come to ARPA to do that and he was not interested. He said he didn't want to become an ARPA bureaucrat. He wanted to stay at Lincoln Lab and do his research, which had nothing to do with networking. It had to do with graphics and three-dimensional wands and that sort of thing. Incidentally his website today will have you believe otherwise, that he wanted to build a network since 1962. It's not true. All you have to do is look his publication record and you can see that. Anyway, he did a good job once he got there, but he didn't get there until Christmas of 1966, which effectively had him start to work in January of 1967, although he probably went on the payroll sometime just before Christmas because his family had moved down to our area, and had not found a house and they stayed with us during the Christmas holidays. That's how I remember that. What turned him around, because I would ask him periodically during 1966 sort of several times each month and then in sometime in the fall of 1966 I remembered something that I thought I remembered correctly and I went in to see Charlie Herzfeld my boss, and I said "Isn't it true that ARPA funds 51 percent of Lincoln Laboratory?" And he said "Yes." I said "Well there's this guy in Lincoln Lab that I've been trying to hire to be the Program Manager for this network I want to have built, and he keeps refusing me." I said "Would you call the director of Lincoln Lab, and tell him to tell Larry Roberts that it would be in Larry's best interest and Lincoln Lab's best interest if Larry Roberts would come and take this job?" He said "Sure." He picked up the phone while I was in his office, and he called Gerry [Gerald P.] Dinneen, who was the Director of Lincoln Lab, and he told him that, and a few weeks later Larry accepted the job. And today he would have you believe that it was something he wanted to do forever.[6]

McJones: Well, you convinced him of what he really wanted to do.

Taylor: Yes, right. I like to tell people I blackmailed him into fame.

[6] Roberts, L. 1986. The Arpanet and computer networks. In *Proceedings of the ACM Conference on the History of Personal Workstations* (Palo Alto, California, United States, January 09 - 10, 1986). J. R. White and K. Anderson, Eds. ACM, New York, NY, 51-58. DOI= http://doi.acm.org/10.1145/12178.12182. See especially the Editor's Note on page 145 of the final version of the proceedings published by ACM Press.

McJones: Right. I see that what you had really done here was a first for ARPA. They had been funding research, suggesting perhaps directions, but more or less looking for people that had a similar way of thinking.

Taylor: That wasn't true of ARPA in its entirety. That was only true of IPTO and Behavioral Science, and probably Material Science, which were the basic research parts of ARPA, but a lot of other parts of ARPA were more-

McJones: Program-oriented.

Taylor: Yes, were development, more development inclined than research inclined.

McJones: Okay, but in this case, it's sort of a hybrid. You're doing something brand new. There's a strong research component and yet, also it was you folks who were out driving the RFQ and-

Taylor: Yes, right.

McJones: What the system was going to look like.

Taylor: Right, and so after Larry got there and we got the RFQ out, and we begin to have replies to the RFQ, proposals would come in, we formed a proposal evaluation group to look over all these proposals, and it met sometime in late 1967 or early 1968. I don't remember exactly when, and it went through an evaluation process, and Sylvania or Raytheon—I can never keep those two companies straight—one of them led the voting in this evaluation, and BBN came in second. And the results came back to me, and I said "No, we're going with BBN. We're not going with this other company because BBN's culture and the people in it, and the people who would be involved in carrying out this work are very similar to the people in the ARPA culture and the other places that we fund from IPTO. They know one another in many cases they come from the same-

McJones: They had come from places like Lincoln Labs and so on.

Taylor: Yes, the same places. They did not have an industrial culture at all, they had a research culture. And I could just imagine people with a corporate culture going into Stanford for example, or any of the places we were funding, typically, and trying to work with the people who were from, fundamentally, an academic culture. It was just oil and water; it would not mix. So we went with BBN, and Frank Heart led this project. He's relatively unknown, which is too bad, because he made a huge contribution. The senior hardware designer on this project was Severo Ornstein, whom you know very well. And the senior software guy on this project was Will Crowther, whom you also know. And there were probably three or four other chaps who were a member of Frank Heart's team, the most junior of which was Robert Kahn, Bob Kahn. Bob Kahn was a theoretician. The rest of these people were fundamentally systems people, and this was a systems problem. A systems design was called for and a system had to be designed and built. So in system design meetings Kahn would be in attendance, and he would ask question after

question because he didn't understand what the rest of them were talking about much of the time. It wasn't his background. And they finally had to say "Look, you're slowing us down," you know? "Just back off and we'll take care of it." Now the reason I mention this is because some few years later, Kahn claimed and has claimed ever since that he was responsible for the system design of the ARPANET. This is not true and somebody ought to say so.

McJones: And you've talked to the other five? You've talked to all of those people?

Taylor: Yes, I asked Frank Heart, I've asked Severo Ornstein. They've asked the other members of the group, and they all say it's not true. He's not responsible for the system design. And I said to some of them,"Well, why didn't you speak up when you first heard that he was making this claim?" Oh, they said they didn't want the hassle. That's too bad, you know.

McJones: Yes, I hope they get their place in history.

Taylor: Right.

McJones: Well that's what we're trying to do is get things on record.

Taylor: Yes.

McJones: So maybe we should back up briefly because chronologically by the time the ARPANET was starting to come online you were actually starting to move on to other things also." How about if we just cycle back a little bit earlier in the ARPA period, but clearly this networking was a key thing for you, something that you initiated the instant you had the chance. There was time-sharing, graphics, other things. Could you give me kind of a sense of how much of your effort and thought went into other kinds of things that you were funding?

Taylor: Well in terms of other things that I initiated, Dave Evans, who had been at UC Berkeley, as I'm sure you know, was invited to come back home to Salt Lake City, his original home, and head up a Computer Science department they were building at the University of Utah. And so he came to see me about ARPA funding and I said, "Why don't you build at the University of Utah a Center of Excellence of Graphics?" ARPA had this notion of Centers of Excellence. I think it was a notion that was probably started in their Material Science Program, where they would find people in a particular university who were strong in a particular area of material science and give that group money to do research in material science to create what they call the "Center of Excellence" in that particular domain. I got that idea from the ARPA Material Science Program and said "Let's build a Center of Excellence at Utah," and David said "Yes, great." And so he went there and he then recruited Ivan Sutherland to come from ARPA and he and Ivan also founded Evans and Sutherland Computer Corporation in Salt Lake City. So that was something that I initiated.

McJones: The time-sharing work was going on?

Taylor: It was continuing. It was continuing. It began to taper off by the time I was ready to leave. When I knew that the ARPANET would work, when the testing had been done at BBN, where a full simulation had been run and we'd had our first installation that sent bits back and forth between SRI and UCLA, then I thought it was time for me to leave. I don't think anyone should be in a job like that for very long. Power corrupts and absolute power corrupts absolutely. You need other people to come in when you have that much money with that much leverage. ARPA wasn't like NSF. If I wanted to start a project in ARPA, the only person I had to convince was my boss. I didn't have to have a board come in and do an evaluation. NSF calls it advisory boards? I can't remember. Anyway, if you wanted to get NSF funding, your proposal had to pass through a committee. ARPA was designed to be a quick-reaction agency. There was none of that committee approval process

McJones: This reflected its origins and the urgency for the space program.

Taylor: Yes, to be responsive. So I left and Dave Evans made a job for me at the University of Utah to pull together disparate research projects they were conducting at the University of Utah under different fundings into one laboratory organization. I did that for a year, but it wasn't much fun. Then George Pake called me and asked me to come talk with him in Palo Alto, because Xerox was setting up a new research center in Palo Alto. He didn't say he wanted to talk to me about a job. He just wanted some advice about his research center, because it was supposed to include something having to do with computing. He had been a provost or a chancellor at Washington University in St. Louis, and had been instrumental in Wes Clark and his group moving from MIT to Washington University, St. Louis. And Wes Clark had told him about me when he had asked Wes Clark for help about this new lab that Xerox was setting up. So there's no evidence that he wanted to talk to me to offer me a job, but I came out to visit him because I was not extremely happy with being at Salt Lake. He told me about this new lab, so I said, "Well what's it going to do?" And he said "Well it's going to do research in support of SDS [Scientific Data Systems]." The people who had given us the Sigma 5 and the Sigma 7, and in spite of themselves had given us the 940. I'll tell you that story in a minute.

McJones: Okay. Xerox had just recently acquired SDS?

Taylor: That's right, Xerox had recently acquired SDS and eventually it was called XDS but I think at the time I was talking to Pake the first time it was still called SDS. And he said they're going to do research or some part of this new lab that we're going to set up called PARC, Palo Alto Research Center, Xerox PARC, will do research in support of SDS. I said "That's too bad." And he said "Why is that?" I said "Because no one who's any good will go to work there." "Why is that?" "Because SDS doesn't have any respect or regard or history of investment in computer research." "Oh. Hm. Okay." He paused and said, "Well, what do you think we ought to do if you don't think we should do research in support of SDS?" And I said "Well you ought to automate the office. You ought to bring the computer into the office." I said "You're already in the office in the big time, so you already have a position there, and a computer can do lots of stuff that's done today in the office manually." And it was to that that he just said "Oh." I mean he didn't light up or want to further discuss it. He just said "Oh." And I don't know, we changed the subject and we talked about other things. I went back home and then I got a call, I don't know, a month later or a few weeks later maybe, to come back again, and he offered me a job. And I said "Well, yes, I'll come and build the research group."

McJones: A low reactor.

Taylor: So apparently he thought about it and a few weeks later he called me up and offered me a job. And I said, "Well okay, I'll come build your group, but I don't want to be the official head of it, because I don't want to have to spend my time with Xerox corporate people. I want to spend my time getting the right people here, and getting working on the right things." And he said "Okay." He liked that because I didn't have a PhD and he thought that the head of the group ought to have a PhD. He said "Well okay, I'll let you recruit the head of the group," and so I said "Okay, fine." So I recruited Jerry [Jerome I.] Elkind and he lasted for a few years. I don't remember how long. Maybe not terribly long. But he annoyed some of the people in the lab, and without my knowing it, some years later after that, this would be in 1970 when I <audio skips> hired so this must have been like 1975 maybe, or 1976? He annoyed some people in CSL and they went to see Pake at a time when Jerry was off doing something else temporarily and asked Pake not to bring him back, and Pake resisted and they resisted and Pake finally gave in. So that's when I was officially made head of the lab.

McJones: I see; then Elkind went and did ASD[7]?

Taylor: Yes, that's right. But as people in the lab have told me, when they came back to give me this news of what they said to Pake, was, "Well, you're head of the lab anyway, so you might as well take the title." I had not thought of it that way, but it was true. So I said fine, let it go. And to this day Elkind probably thinks that I choreographed that, but I didn't. There are a lot of people who could verify that story I just told you. Severo [Ornstein] was one of them. He was one of the people who complained to Pake.

[Doorbell rings; camera stopped to change batteries.]

McJones: We're back after a break. Okay, we're right there at the beginning of PARC. So Bob, tell me about this new job, you already told George Pake what Xerox ought to be doing, so what sort of things went through your mind? What were your first steps to get the lab going? You had a vision yourself, I suspect of where that might go, what computers in the office might be like. Were you looking for people who matched your vision? Or were you looking for people who had a strong vision of their own?

Taylor: I was looking for people that would endorse my vision. That thought it made sense, but more important than that, I was looking for people who I knew to be really good, so it turned out for example that early on Berkeley Computer Corporation was folding, and there were a number of good people there whom I knew.

McJones: They were people that you had funded?

[7] Advanced Systems Development, which did customer trials of office automation systems based on PARC's Alto personal computer.

Taylor: Yes, people that I'd funded from ARPA at UC Berkeley in Project Genie, and so I went up to see them or invited them to come down, I can't remember which, several of them. Butler [Lampson], Jim Mitchell, Peter Deutsch, Dick Shoup.

McJones: Chuck Thacker?

Taylor: Chuck Thacker. And I said there's this new lab starting up, and so on, and there's a big opportunity to put interactive computing in the office. I guess they looked around at the various alternatives; living in Palo Alto is not all that bad. And they said, "Okay," so they came and other people that I talked with along the same lines came, and the first thing that we needed if you're going to do computer research, you need a computer. And so Pake and Bill Gunning, his right-hand man who had been a big advocate of SDS, said "Well, of course you'll get a Sigma 7." And I said, "No, we won't get a Sigma 7. We'll probably get a PDP-10." Well, DEC made the PDP-10. DEC and SDS were bitter competitors. And the PDP-10 especially with an operating system called TENEX[8], which had been built under ARPA contract by Bolt, Beranek and Newman, if you put that operating system on a PDP-10 you've got a nice environment for computer research. If you put the DEC operating system on it, it's not so good. And the SDS operating system on a Sigma 7 is not so good. So one of the reasons why the SDS Sigma 7 wasn't so good, takes me to an earlier point in our story that's now appropriate to tell you. When Project Genie brought up their time-sharing system-

McJones: At Berkeley.

Taylor: At Berkeley, one of the earliest time-sharing systems built. They did it by buying an SDS Sigma 5.

McJones: Or a 930?

Taylor: Oh you're right, right. Not a Sigma at all. The 930, thank you. An SDS 930, and tearing out some of its hardware, and putting in some memory protection hardware, and they bought this 930 at commercial prices, not at education discount. SDS did not give them an educational discount. It was ARPA money. And the Project Genie people then designed and built this time-shared operating system on this rewired 930 in effect. And it was up and running and working, and I had a terminal to it, one of the terminals in my office was to this machine, so I could use it, you know, a little bit. Moreover, anyone else who wanted a demo of that system could come in and get it.

So I asked Berkeley to contact SDS. Max Palevsky was head of SDS in those days. (I'll call it XDS sometimes and SDS sometimes. It doesn't matter; it's the same company.) And I asked the Berkeley guys to tell SDS that ARPA would give them all the software that Berkeley had built, and the hardware mods, if SDS would make this rewired 930 into a product. And they said no, but we'll come talk to you about it. So eventually Palevsky and several members of his staff came to see me in the Pentagon, and I

[8] Daniel G. Bobrow, Jerry D. Burchfiel, Daniel L. Murphy, and Raymond S. Tomlinson. TENEX, a paged time sharing system for the PDP-10. In *Proceedings of the Third ACM Symposium on Operating Systems Principles* (Palo Alto, California, United States, October 18 - 20, 1971). SOSP '71. ACM, New York, NY, 1. DOI= http://doi.acm.org/10.1145/800212.806492

repeated the request, and they said “No, we don’t think so. Time-sharing is not going to work. These guys at Berkeley are just playing in a sandbox. We’re not interested in this. We’re a serious commercial company.” And he went on and on about this contrast between his seriousness and these children playing in sandboxes so I finally said “Well, we’re not getting anywhere here. Get out.” And I got pretty mad at him. Real asshole. So he left, they left. A few minutes later one of the guys with him came back into my office. And he said “You know, I think you’re right. I think this time-sharing thing is going be important. What can I do to help?” he said. His name is Rig Curry, Rigdon Curry and I said, “Well, I’ve got this terminal to this Berkeley system in my office. Why don’t you line up some possible customers and bring them up here and give them demonstrations. We’ll line up some paying customers with money in their hands outside Palevsky’s door and then we’ll see what happens.” So he did that, and he got 19 people ready to buy this system.

And so SDS made it into a product they called the 940. Now, to build this 940, they took a 930 all the way up through the production line, through the testing sequence, got it all the way accepted as a 930, took it over to another line, did the hardware modifications on this other line, and then issued it off of that line as a 940. That’s the way a 940 was created in hardware. For software they contracted to a guy whose name I can’t remember, who lived in San Francisco, to make a commercial version of the Berkeley software. So they never internalized the Genie operating system inside SDS at all. They never had a 940 line from A to Z. They sold a large number of those machines, 940s. It was their most successful product. Well, during the time that they were having it as part- BBN bought, I don’t know, several of them.

McJones: Didn’t Tymshare use them also?

Taylor: Tymshare used them. Whiteweld, the investment firm, used them. A lot of people bought them. And then SDS announced the Sigma 7. The Sigma 7’s operating system had none of the adorable features in the 940, in the Project Genie system. They didn’t learn a damn thing from any of that technology. That’s why I told George Pake we wouldn’t get a Sigma 7. When the Sigma 7 was announced, once people who had signed letters of intent saw the specs for the operating system, they canceled and ordered PDP-10s. This was the beginning of the end for SDS.

McJones: Right. So your people and you were absolutely not going to buy a Sigma 7 for your lab.

Taylor: Yes that’s right. And so Pake was insistent, and he said, “Well, you well you can’t do this. You’re going to embarrass SDS, embarrass Xerox corporate.” I said “Okay, I’ll tell you what. You invite SDS’s most knowledgeable people about the Sigma 7, the designers, anyone you want, you invite them up here, and I’ll have you come in and sit down with me and those SDS people and my people, and we’ll have a discussion in your presence. And then you tell me what you think.” And we had that discussion, and those SDS guys were ripped a new one. I mean Lampson, and Thacker, and Mitchell, my guys, just tore them up one side and down the other. They knew more about the Sigma 7 than the Sigma 7 people. And Pake, at the end of that, said, “Well, okay, I guess you people know what you’re doing.” But there was no way that they could win that argument. So we got PDP-10 and we got TENEX. We didn’t have to build a new operating system.

McJones: But you did have to build MAXC[9].

Taylor: We didn't get a PDP-10. But what we did was we built the PDP-10. We told Pake "Okay, we won't buy it from DEC. We'll just build one." And I talked to Ken Olsen DEC's CEO, on the phone, and told him what we were going to do and that we were not going to sell it. We were just going to build it for ourselves for research and he said okay. I had met Ken when I was at ARPA and new him to be reasonable.

McJones: So that was the beginning of MAXC?

Taylor: That was the beginning of MAXC, yes.

McJones: MAXC the Multi Access Xerox Computer.

Taylor: With a silent "C".

McJones: Which was also the name of the-

Taylor: Palevsky.

McJones: Yes.

Taylor: Yes, that was deliberate. Named it after Palevsky as far as the Sigma people were concerned.

McJones: So this MAXC was was the first big systems project [at PARC].

Taylor: Yes, now there's a story about that too. I didn't want to build MAXC initially. Or maybe it was MAXC II I'm thinking about. We built a MAXC II, didn't we?

McJones: Yes, I think there was a second one.

Taylor: That's right. So why did we want a MAXC II? I can't remember. I don't remember. But anyway, it was either before MAXC I or MAXC II, and I can't remember, I said "We really ought to build a display-based computer." That's the only way I could think of describing what it is I wanted. We had some Imlacs. Do you remember Imlacs?

McJones: Very vaguely.

[9] E.R. Fiala, "The Maxc Systems," Computer, vol. 11, no. 5, pp. 57-67, May 1978, http://doi.ieeecomputersociety.org/10.1109/C-M.1978.218184.

Taylor: Little computer that had a display but it was a storage tube display[10]. It wasn't a bitmap display. The central design point of a computer ought to be the display. The eyeball is where your bandwidth to the human is. And it took them a couple years, Thacker, Lampson, and the rest of them to come around to understanding what I was talking about, at least according to them. If you look at my retirement video, one of them, I think it's Lampson, tells the story. He said, "Taylor tried to get us to build the Alto but we didn't understand what he was talking about."

McJones: Well, you'd seen the importance of displays back in your ARPA and <NASA days.

Taylor: Right, right.

McJones: Whereas they had not come from such a visually oriented background.

Taylor: Right, right, right. So-

McJones: In some sense MAXC seems like a good project; it brought the team together.

Taylor: Ah, and I remember one other thing. The Berkeley Computer Corporation did not make it financially. And their machine I guess ultimately ran, but it had some problems. It got shipped to the University of Hawaii eventually, so I presume it ran. And so there was something called "the second system syndrome". Do you remember what that means?

McJones: Yes it's when the designers get grandly inflated expectations based on early modest success.

Taylor: I think this was about MAXC I. The first machine that they wanted to build was a time-sharing machine because they just finished building one of those and they didn't like in all respects the way it turned out I believe. And so that was sort of more familiar ground to them than to start off with something they'd never conceived of like the Alto. It was a good thing they did it because what building that machine did for us is first of all was that it was a lot easier design problem because we just copied the PDP-10. Secondly, in getting the hardware parts built for that machine, we had to create a local chain of suppliers which came in really handy when we were ready to build the Alto. So it was a big advantage, big plus. It helped Thacker win a bet with some one of the SDS guys that we could build the Alto in four months. We did it, and he had said "You can't do that."

McJones: He had confidence in the supply line and so on.-

Taylor: Yes, we already had the suppliers and we knew their response time, and we had good working relationships with them, so.

[10] The Imlac PDS-1 was an early graphics terminal; actually it used vector graphics rather than a storage tube or bitmap.

McJones: I believe that MAXC also was one of the first machines to use dynamic RAMs instead of core.

Taylor: It was the first machine to use 1103s as memory.[11]

McJones: So you were an important early customer for Intel.

Taylor: Right.

McJones: So you were a little edgy that it wasn't going in this graphical direction, but MAXC happened, but then you continued to urge-

Taylor: Yes, we needed a machine and we needed it soon, and we could build this thing, the MAXC I, pretty quickly. I still don't remember the case for MAXC II.

McJones: Perhaps just so many people wanting to run big LISP and other programs with large address spaces.

Taylor: Oh, that's it. Time-share computers run faster at night.

McJones: Right.

Taylor: That's the reason. So I think I tried it again with that to get them to do the Alto or what was later called the Alto. And it wasn't until after that was done I think that they were willing to turn their attention to the Alto. Did you know the Computer History Museum is having a celebration for the Dynabook?

McJones: I heard something about that. This is the celebration for a computer that never existed? [Taylor nods.] Maybe we should talk about some of those people. I think you encouraged or recruited some of these people to come to PARC, right?

Taylor: Which people?

McJones: Alan Kay?

Taylor: Yes, absolutely. Alan Kay was a big contributor to PARC but the Dynabook was not. Dynabook never existed but Alan liked to call the Alto the Interim Dynabook. In fact Thacker claims that he thought of calling the Alto the Interim Dynabook. Now this may be because Thacker and Lampson went to Alan to get from his budget center some money to help to pay for the first Altos because they knew that from his

[11] The Intel 1103 was a 1024-bit DRAM chip.

talk about the Dynabook, that he would like a machine that was centered around a visual display. Alan contributed some of the design ideas to the Alto. I can't remember, it may have been maybe the character-

McJones: Well let's see. There was a display that looked like an Alto display but it had a character generator rather than a full bitmap and would be connected to Novas or something like that? Was that one of the earlier systems from SSL?

Taylor: No, that was- I don't know where that came from.

McJones: POLOS[12]?

Taylor: Yes, but the POLOS was not Alan's idea. It was Bill English's and a number of other people's idea based on the Nova. I'm trying to remember for designing , you could take a character glyph, enlarge it on the display, fill in pixels within the glyph, and then reduce it back down and it would become a font, right?

McJones: Is that a program that Alan had done?

Taylor: I think Alan's people wrote that program, but I'm not sure about that. But I think he made some contributions. I want to say that the bitmap display was his idea but I don't think that's right. I don't know. But he definitely made some design contributions, but he was not the designer of the Alto.

McJones: Right, but at least he was willing to-

Taylor: He paid for some of the ones that were built. He ordered some for his own lab. He didn't order enough main memory. We ordered 64K [16-bit words] for ours. He ordered 40-something K for his. He regretted that later. But you never have enough main memory. Sixty-four K wasn't enough main memory either.

McJones: Right. Six hundred and forty kilobytes wasn't enough a decade later.

Taylor: Right.

McJones: Four gigabytes isn't enough now.

Taylor: Right, right.

[12] PARC On Line Office System.

McJones: The Alto clearly for many people outside of PARC was the central invention.

Taylor: A lot of people forget that the Alto wasn't just a computer.[13] It was a system.[14] It was a complete system. It involved a local area network. It involved printers and print servers. It involved name servers. It involved email servers. It was a lot more than just a personal computer.

McJones: And yet at the very beginning it was just sort of a glimmer of that whole system in people's eyes and people like Thacker went forward and said "Well, I'll do the hardware and then."

Taylor: Yes.

McJones: What were some of the other steps? I guess networking would be a good one to talk about?

Taylor: Yes, Metcalfe and Boggs with some help from Thacker and Lampson did the Ethernet. Lampson and SwineHeart and who else did EARS? Do you remember EARS? The first laser printer, which stood for E for Ethernet, A for Alto, R for Research character generator, S for Scanning laser output terminal. Oh, pretty good Bob. <smiles>

Beckman: Was Starkweather involved in that?

Taylor: Yes, Starkweather was involved in the "S". The scanning laser output. The laser device that scanned the document.

McJones: Right. He was not in your lab.

Taylor: Yes that's right. He was in the Optical Science. He later joined my lab but at that time he was in the Optical Sciences . Before coming to PARC he was at Webster, Xerox Webster Research Center working on a laser copier. And the head of the Optical Sciences lab in PARC mentioned him to me one day and said "He's working on a laser copier." And I said "He's what? You mean there's going to be a laser that puts those marks on the paper?" He said "Yes." I said "Get him here now." He said "Why?" "Because we're going to make a laser printer that's driven by a computer." And he said "This guy doesn't know anything about computers." And I said "Fear not. We do." <laughs> So they brought him out here. His boss at Webster thought he was wasting his time. So he was brought out here and put to work on this project.

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[13] Chuck Thacker. Personal distributed computing: the Alto and Ethernet hardware. In *Proceedings of the ACM Conference on the History of Personal Workstations* (Palo Alto, California, United States, January 09 - 10, 1986). J. R. White and K. Anderson, Eds. ACM, New York, NY, 87-100. DOI= http://doi.acm.org/10.1145/12178.12185

[14] Butler Lampson. Personal distributed computing: the Alto and Ethernet software. In *Proceedings of the ACM Conference on the History of Personal Workstations* (Palo Alto, California, United States, January 09 - 10, 1986). J. R. White and K. Anderson, Eds. ACM, New York, NY, 101-131. DOI= http://doi.acm.org/10.1145/12178.12186

McJones: So Gary came pretty early on in the founding of PARC.

Taylor: Yes.

McJones: I see. And you thought there was something better, a higher purpose for him.

Taylor: Yes. That's right.

[Crew talk]

<audio ends abruptly>

END OF TAPE 2

McJones: Okay, we're back. So, just recap on the--

Taylor: So, I'd say the first laser printer was called EARS, as I said, E for Ethernet, A for Alto, R for Research Character Generator and S for the laser output part of it. And the R, the Research Character Generator, was a very complicated computer, in fact. And of course, the Alto was a computer and the Ethernet was, of course, critical to get stuff there and back. So, to give one person credit for the laser printer doesn't seem correct to me.

McJones: Like many things at PARC, they were systems that were a combination of insight and sweat from a bunch of people.

Taylor: Absolutely right. There is no single person at PARC who should be given credit for all the many things that many different people did. The overall systems architecture for the Alto I would attribute more to Lampson probably than any other single individual, but a lot of other people helped, critically helped.

McJones: Ed McCreght worked on some of the early hardware, maybe the disk controller.

Taylor: Yes, McCreight, yes, that's right, on the Alto disk controller.

McJones: And many, many people contributed to the software over the years.

Taylor: Lots of people, yes. Probably 50 people you could name who contributed significant stuff to the overall Alto system.

McJones: Through the early BCPL-based software and the Mesa-based software and into the Dorado world, this was a continuation of that. So, this takes us up to approximately mid-1970's when many of the pieces were coming together. I joined in late 1976 and there there was a very nice WYSIWYG [What You See Is What You Get] text editor and there were laser printers, Ethernets, file servers, the whole world, it seemed.

Taylor: Do you remember whether you could use email when you got there over the Alto system, right? Do you remember using email?

McJones: Well, it was possible that when I first got there we actually telnet'ed over to MAXC and used its email, but the Alto based Grapevine system with Laurel was coming online around that time.

Taylor: Not before, huh?

McJones: I don't remember the exact chronology of that.

Taylor: I was thinking about PUP. Do you remember PUP?

McJones: Oh, yeah the Public Utility Packets, the whole network stack.

Taylor: PARC Universal Protocol.

McJones: Okay, PARC Universal Protocol.

Taylor: Which was a packet system and which preceded TCP/IP by several years. Did you know that?

McJones: Yes, I've read a little about that but why don't we talk about that.

Taylor: I don't remember much about it. I just know that the Xerox lawyers kept us from making PUP public, letting it outside. Vint Cerf at Stanford, who was a faculty member at Stanford in 1975, I think, or 1976, created a design committee amongst Stanford people mainly to come up with a protocol design to put on the ARPANET. He invited some people from PARC to come to these meetings and no one outside of PARC knew that we had a working PUP. But the Xerox lawyers told the Xerox guys who went to this meeting at Stanford that they could go to the meeting but they couldn't tell the people at Stanford about PUP. So, Metcalfe and Shoch went to these meetings and Boggs and Taft might have as well. Do you see Taft ever?

McJones: Occasionally.

Taylor: Ask him. You might ask him also what year PUP became running. When were we first using PUP? I think it was 1975 but I'm not sure.

McJones: That sounds about right.

Taylor: So, the Xerox guys went to this meeting and some Stanford guy would lay a design idea out on the table and the Xerox guy would say, "Well yes, but if you do that you might have trouble with this idea over here or with this feature over here or this will get in the way of that." And this conversation went around and around for a few times and finally one of the Stanford guys sat back from the table and said, "You guys have already done this, haven't you?" And indeed, they had. So, Vint Cerf and Bob Kahn owe their fame to the Xerox attorneys. If it weren't for the Xerox attorneys nobody would have ever heard of TCP/IP. TCP/IP designs were based a lot on PUP.

McJones: They benefitted from all of this feedback from the PARC people.

Taylor: So, you know about the Farber List of Dave Farber?

McJones: Oh, right.

Taylor: Something was--

McJones: Interesting people.

Taylor: They're not very interesting but things come up on this list about history and every time Cerf and Kahn come up there, they're the fathers of the internet because of TCP/IP. So finally, I wrote back to Farber's List, which is—moderated — whatever you send doesn't get printed on it unless Farber approves it. So, I sent back to the list saying, "Cerf and Kahn ought to share some of the many awards they've received with the Xerox lawyers." And Farber didn't print it.

McJones: Didn't forward it.

Taylor: No, he didn't put it on his list.

McJones: Well, I hope eventually historians will be able to actually look at the PUP software, the design documentation and so on, and eventually get the record complete.

Taylor: Maybe, I'm not terribly optimistic. History is made by the people who write about it, not the people who truly make it.

McJones: Okay, so this is all very interesting. So, now we've got a complete internetworking stack just within Xerox, and I used that. I know there were links going to El Segundo and the East Coast, and I can't remember for sure where else, probably you can.

Taylor: There were between 4,000 and 5,000 Altos built.[15] Most of them were within Xerox, all over Xerox. Xerox's legal department in corporate had them, even though they wouldn't let any of the information out. They were in other parts of Xerox, too. The White House had some under Carter. The Senate and the House had Dover printers, or a printer at least, which was the laser printer successor to EARS. We gave Alto systems made up of Dover printers, 18 Altos in each group and Ethernet to MIT, CMU and Stanford. At Stanford, the people who founded Cisco took that Alto system technology and founded Cisco with it to build routers. The people who founded Sun took the Alto architecture with them to build the Sun workstation. Later, the people who founded Adobe took PostScript with them and built Adobe. Earlier, Charles Simonyi took Bravo with him and built Word, which was the most money-making product for Microsoft for some time and really put Microsoft on the map. So, these guys at PARC had a lot of influence on things that happened later.

McJones: And you're skipping Convergent Technologies and various other companies.

Taylor: I'm skipping lots of other things.

McJones: 3Com.

Taylor: 3Com, right. It took Ethernet to build 3Com, yes.

Beckman: Wasn't it Interpress?

Taylor: Was it Interpress they [Adobe] took? Not PostScript, PostScript they did at Adobe, Interpress, they took from Xerox. Is that the way it went?

McJones: Yes, well--

Taylor: Why didn't you stop me? You didn't remember either?

Taylor: No, I mean why didn't he stop me? He works for Adobe.

McJones: I'm off the clock right now.

[15] The previously-cited paper by Thacker estimates 60 of the original Altos were built, mostly for CSL and SSL, and about 1500 of the redesigned Altos were built.

Taylor: I see. Well, it was Interpress not Postscript, right?

McJones: Right, Interpress was the Xerox thing.

Taylor: PostScript was done at Adobe, right?

McJones: At Adobe. And of course, you know, obviously they say they didn't take any intellectual property.

Taylor: Well, in all of those cases they took the ideas.

McJones: Yes, the ideas. By the way, Brian Reid eventually wrote a very interesting technical comparison between PostScript and Interpress in the '80's after Postscript came out.[16]

Taylor: Really, and?

McJones: He was trying to just be objective and only talk about the ideas.

Taylor: Yes, and?

McJones: And he thinks they're both good.

Taylor: Are they different significantly?

McJones: Oh yes, there were a bunch of interesting differences that it--

Taylor: Improvements, and Postscript was a big improvement I hope over Inter--

McJones: In some ways it was. It had a very interesting font technology. There were other differences. It was a dynamic programming language. if You actually executed the Postscript program, and the net

[16] Brian Reid. PostScript and Interpress: a comparison. LASER-LOVERS email list, March 1, 1985. http://groups.google.com/group/fa.laser-lovers/browse_thread/thread/1f7bace21f12dca9/5d0df32a0e91f1fa?lnk=st&rnum=3#5d0df32a0e91f1fa; it resulted in this response by a Xerox consultant: Jerry Mendelson. Interpress and PostScript, A Second Comparison. LASER-LOVERS email list, March 3, 1985. http://groups.google.com/group/fa.laser-lovers/browse_thread/thread/cbff6e2e23748b10/4a5d81b4eda79b4c?lnk=st&rnum=2#4a5d81b4eda79b4c

effect was that it was very hard to do something like reverse the order of pages to be printed for a printer that stacks backwards because that would mean rewriting the program to print the pages in the opposite order. So over the years Postscript became PDF, which is much more like Interpress.

Taylor: Oh, I didn't know that there was that connection.

McJones: Yes, so that was--

Taylor: I haven't followed Adobe's products at all. I don't use them. I mean, it's not that I use something else instead of them, I just don't do that kind of thing with my computer.

McJones: Right, right, well anyway, we shouldn't talk about that, we should get back to the exciting news. So, can we talk a little bit more about other people that you hired into the lab? You created quite a ferment; it was quite an interesting batch of people. Are there any other names that jump out at you?

Taylor: Well, I mean, people--

McJones: They were all good.

Taylor: People that jump out at me that made lasting impressions include—I've already mentioned the BCC group—Howard Sturgis came from the Berkeley community but not the BCC group. I think I mentioned Severo Ornstein earlier, but I mentioned him in the BBN context probably. But he and Bob Sproull were responsible for the Dover pretty much.

McJones: And also the Dorado Project, wasn't Severo on--

Taylor: Yes, Severo was a big part of that, yes. Jim Morris, whom I think you mentioned earlier, did a lot of things for the Alto system, so did Roy Levin, Mike Schroeder, Andrew Birrell, and Jim Horning. Then in the graphics area there was Patrick Beaudelaire and Leo Guibas; there was Lyle Ramshaw, and that included some font invention stuff.

McJones: There was a small theory group at PARC. Can you say a little bit about how that <inaudible>?

Taylor: Let's see, Frances Yao was a part of that. Guibas was a part of that, so was Ramshaw, so was Greg Nelson, but they also had graphics influence as well as theoretical influence. There are names now that are flashing in front of me and I can't bring them up. Help me out. Ask me about some people. What did so and so do? Who were you thinking about?

McJones: Well actually, what I was going to do before that is ask when did you start thinking about theoretical computer science versus systems? In the '60's were you--

Taylor: Early.

McJones: Were you thinking--

Taylor: Early--

McJones: And the whole field was forming in the 1960's.

Taylor: I have a bias in favor of mathematics and I think of software as fundamentally a mathematically oriented discipline. I also have always thought that software was more complicated than hardware and more difficult to do properly than hardware. So, I have a software biased view of the world, the computing world anyway. I sometimes don't like the fact that we're called Silicon Valley for that reason because you have to have both of them. If you have a national transportation system, you have to have highways and you have to have trucks. But I think software, pardon the pun, gets short shrift too often because hardware you can feel and touch and see and pick up and the general public doesn't understand software.

McJones: And probably even less in the late 1960's or early 1970's.

Taylor: Right, right, I mean, even today you'll hear people talk about software programs. It's a redundancy. So theory has always been, to me, a part of what computer science is all about.

McJones: But it seems like it's been more the university departments that have seemed to have some of the strongest theory groups.

Taylor: That's because it's very difficult to have system strength in a university department because building hardware and building the software for that hardware is an expensive proposition and also requires a lot of teamwork. The university value system is based on individual performance, individual publish or perish, individual contract acquisition. Teamwork is not rewarded in universities so it's perfectly understandable, team--

McJones: Although, you're early—and Licklider's and Sutherland's—funding for universities was based on big block grants, right? You were actually trying to create a different environment so they could do systems work.

Taylor: Right, but there weren't very many of them.

McJones: You could only fund a few.

Taylor: Right.

McJones: Okay, well so since your focus was systems, you managed to get some good theory people but in a sense they had to be willing to forgo the academic environment. They had to be risk takers or something to come and work at your lab rather than stay in academia.

Taylor: Risk what?

McJones: Well, what I mean is--

Taylor: What were they risking?

McJones: Well, they were risking falling out of the the academic publishing system if--

Taylor: No, they still published and they went to theory conferences and they were widely highly regarded by theoreticians in universities and we had many theoreticians in universities who wanted to come and spend their sabbaticals with us. So, I think the theorists that we had were generally respected by academic theorists.

McJones: Right, I wasn't trying to cast aspersions on anybody; just to say that it was kind of a small element of the overall mix.

Taylor: Yes, it's true. But some of the people that appreciated the theorists most were our systems people who had problems they couldn't solve so they'd go see one of the theorists and they'd--

McJones: Give a solution for a hard hardware or software problem, right. I remember that happening many times later at DEC. Well, I think one thing would be interesting to talk about is the Future Day? There was a big demo of the entire Alto system to try to convince the-- well, I shouldn't say the whole story.

Taylor: Yes, do you remember what year that happened?

McJones: I'm not sure but perhaps around 1977 or 1978.

Taylor: Yes, I think that's right. Okay, so to set that up, by 1978 the Alto system had been demonstrable and working and Xerox had not committed, to my recollection, to a systems product based on the PARC technology. And the head of Xerox wanted to have a large conference of the Company's most senior

people in Boca Raton, Florida and they could bring their wives and they would make a big to do about it and have entertainment for the wives and so on and so forth, a multi-day conference, I don't know, three or four days, maybe a week, I can't remember exactly. One of those days the conference organizers dedicated to Future's Day: what a future product world of Xerox might look like. And they asked PARC to come and demonstrate its technology to this audience of high-level Xerox people and their wives. So, a bunch of people at PARC and SDD[17]-- no, did SDD exist?

McJones: Yes, it existed but I think it was really a PARC event.

Taylor: Okay, all right. A bunch of people from Palo Alto, I guess you should say, were asked to put together exhibits, in effect, that were live exhibits, that were running exhibits, of the various capabilities that the Alto system could demonstrate. And so they did that and the morning of the Future's Day in an auditorium with an audience filled with Xerox executives and their wives there was a stage filled with several different stations, I guess you'd call them, showing Xerox things, the Alto doing word processing, doing printing, doing email.

McJones: Graphics.

Taylor: Graphics, Smalltalk, probably. It had a Hollywood touch. In fact, they hired someone from Hollywood to write a script and they had a moderator up on the stage dramatically describing this or that or calling the audience's attention to this or that and they had TV cameras. I think they had a large screen showing things that were on the [computer] screen. I don't remember that. Were you there, Ken?

Beckman: I was not at Boca Raton. They had a Light Valve [video projection system].

Taylor: Okay, right. So, they could show the audience what was on an Alto screen. That happened in the morning, a program that lasted, I don't know, a couple hours, something like that. In the afternoon they set up these demonstration areas in a big room so that people could walk around from one area to another, sit down in front of the Alto, have the PARC person who was in charge of that demonstration take them through demos of various things so they could actually sit down and have direct experience. Well, one would notice by looking over the whole room in the afternoon that all the men were kind of standing back against the wall out on the periphery, and all the women were eagerly moving from one Alto to another to another engaging, sitting down, working the keyboard and so on and so forth. And someone observed that men grew up in a culture where it was degrading to know how to type because typing was a secretarial skill and the women knew how to type. Well, that should have been a foretelling of what was to come, the beginnings of understanding that Xerox was not going to understand this technology.

McJones: And arguably they would even have a challenge selling it to customers who thought that same way, but ….

[17] Systems Development Division, which designed the Xerox Star office automation system using PARC research results.

Taylor: Well, that's what Xerox thought, that the customers would think the same way. They happened to be wrong about that as Bill Gates and other people proved.

McJones: Right, as we were talking about earlier off camera,there was this additional sort of hobbyists/kids effort and so on, effort that was helping to create some of the expertise and demand also.

Taylor: I suppose that's what [John] Markoff would say and perhaps it's true, but I don't think very many people really knew about that hobbyist effort outside of the Bay Area.

McJones: Right, I think it was a much focused thing at that time.

Taylor: So, I don't think it had much influence nationally.

McJones: You could subscribe to *Byte* magazine. Apple had an amazing ad, a full-page color ad of this couple in their suburban kitchen with their Apple II and--

Taylor: But that was Apple.

McJones: Yes, but this is right around that same time.

Taylor: But that wasn't the Homebrew Computer Club.

McJones: No, but it was the same time.

Taylor: Same culture, maybe.

McJones: Yeah, the same people and someone in the form of Steve Jobs who was a little bit more aware of appearances ….

Taylor: Somewhat the same culture, you're right.

McJones: So—

Taylor: Larry Tesler even.

McJones: Right, a few PARC people went--

Taylor: Alan Kay at one point.

McJones: Right, and I think Tom Malloy was one of the early ones to go out. Well, so this must have been very frustrating because I remember that entire Future Day thing. First, it was moved to L.A. and set up on a sound stage and rehearsed and then it was moved to Boca Raton, and it must have been heartbreaking really for the people to have such a low reaction for the PARC people. Do you remember talking about this?

Taylor: I think that the recognition of the low reaction came slowly. I don't think it was--

McJones: So they didn't necessarily come back and ….

Taylor: I don't think it was instantly recognized.

McJones: Yes, so that was 30 years ago.

Taylor: Was it?

McJones: Just about, plus or minus, and you folks had invented the future.

Taylor: Isn't that right?

McJones: So, how are you feeling on time?

Taylor: I'm fine.

McJones: Okay. We might try to finish up the the Xerox era and that might be a natural boundary. There were some other interesting projects going on around that time;, for example, the Dorado project, which helped to build a whole new more capable base of hardware that would enable more powerful software.[18, 19, 20] Do you remember any interesting stories from that period?

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[18] Butler W. Lampson and Kenneth A. Pier. A processor for a high-performance personal computer. In *Proceedings of the 7th Annual Symposium on Computer Architecture* (La Baule, United States, May 06 - 08, 1980). ISCA '80. ACM, New York, NY, 146-160. DOI= http://doi.acm.org/10.1145/800053.801920

[19] D.W. Clark, B.W. Lampson, K.A. Pier, "The Memory System of a High-Performance Personal Computer," *IEEE Transactions on Computers*, vol. 30, no. 10, pp. 715-733, October, 1981. http://doi.ieeecomputersociety.org/10.1109/TC.1981.1675691

[20] Kenneth A. Pier. A retrospective on the Dorado, a high-performance personal computer. In *Proceedings of the 10th Annual international Symposium on Computer Architecture* (Stockholm, Sweden, June 13 - 17, 1983). International Symposium on Computer Architecture. IEEE Computer Society Press, Los Alamitos, CA, 252-269. URL= http://portal.acm.org/citation.cfm?id=800046.801663

Taylor: I remember a story that the Dorado reminds me of, although it's not about the Dorado, per se. Jim Morris had a couple of observations about having a computer in his office. One observation was that, which the Dorado reminds me of, is that I told him that he was going to have a computer in his office and I didn't tell him that he was going to have a heater in his office, as well. Another story that he tells is that his mother-in-law never thought he was going to amount to much until she saw that he had a computer in his office. He thanked me for that.

McJones: Interesting. I'll say she was pretty perceptive. She was one of the, you know, vanguard of noticing things like that.

Taylor: But I was never a big fan of the Dorado because it just violated a lot of things that I believed in emotionally like the idea of owning a computer that's right next to you.

McJones: Why don't you describe the Dorado for anybody who hasn't heard of it.

Taylor: The Dorado was a very fast machine, a lot faster than the Alto. I don't remember how many times faster but a lot, but it was also too big to put in the office and too hot to put in the office. And so we built a whole bunch of them and put them in a computer room, which in my view was a step backward. One of the reasons that personal computing appealed to me so much was to get out of the computer room and to get rid of these guys in white coats and so on. Well, we didn't have to have guys in white coats in the Dorado computer room but we had to have it very cool. And so you still accessed Dorado from your desk via keyboard and display and a mouse, but nevertheless, it wasn't nearby.

McJones: It was sort of cheating.

Taylor: Yes, that's right. But it ran and it ran well.

McJones: Well, the Dorado really enabled the Cedar software system[21], because of having more physical memory and a larger address space.

Taylor: Yes.

McJones: Cedar, it was interesting the way that got going. As I remember there was sort of a bake-off, there was a big discussion amongst the labs as to what was the best programming language and maybe where should we centralize--

Taylor: I don't remember much about that.

[21] Warren Teitelman. A tour through cedar. In *Proceedings of the 7th international Conference on Software Engineering* (Orlando, Florida, United States, March 26 - 29, 1984). International Conference on Software Engineering. IEEE Press, Piscataway, NJ, 181-195.

McJones: Because there had been groups that had been programming in Mesa, Smalltalk, LISP and it seems that there was a lot of debate about the best programming environment and as I remember--

Taylor: Well, wasn't Cedar a Mesa-based environment?

McJones: It was, but as I remember there was a sort of task force. You know, there was a lot of discussion, a lot of very careful debates and a final report that I think maybe Peter Deutsch and perhaps someone else co-authored to try to pull together a consensus view of the strengths and weaknesses.[22]

Taylor: Yes, I vaguely remember discussions about the pros and cons of all of us using the same programming environment.

McJones: Yes, I think we're talking about the same thing.

Taylor: There were definitely some cons. I think Peter elected to move over to Alan Kay's group because he wanted to work more in Smalltalk, but I could be wrong about that.

McJones: You know, that does sound right but maybe that was even before this period. Well, in some sense the Cedar version of Mesa, Cedar-Mesa, tried arguably fairly successfully to combine a bunch of the properties that the earlier systems had had.

Taylor: I think it's hard for me to see the impact of those kinds of efforts, because unlike the other examples we've given of companies that made use of our technologies, I don't know where the Cedar stuff led. I mean, did it lead to Java? Well, Mesa influenced Java in some ways. But I can't see that as direct a connection.

McJones: I see. That's a very interesting point. That's right, also, I think the Cedar-Mesa world wasn't really fully functioning until well into the early 1980's when you left.

Taylor: Yes, I left in 1983, yes.

McJones: Let's see, one piece of Cedar-Mesa was the graphics environment and it had a much richer graphics environment than had been possible on the Alto, color and so on. And so aspects of that graphics model were another thing that, for example, influenced Adobe.

Taylor: Oh good, I didn't know that.

L. Peter Deutsch and Edward A. Taft, editors. Requirements for an Experimental Programming Environment, Technical Report CSL 80-10, Xerox PARC, June 1980.

McJones: The Dorado and Cedar era, lasted many, many years but in some sense it was a second system. It wasn't this fundamental going from nothing to….

Taylor: Well, what else did it influence besides Adobe?

McJones: Well, that's a good question. I don't know. For example, I think it probably didn't have as much of an impact on the universities because when the universities got Altos it was just day and night, this was a total game changer for them, whereas by the time, if, I suspect, they did get some Dorados, by then they were getting Sun workstations and a lot of other things also…

Taylor: I can't remember if they got any Dorados.

McJones: Perhaps not.

Taylor: I don't think they did.

McJones: Dorados were being sold as high-end Lisp machines, InterLisp machines, so a few people got their hands it that way.

Taylor: Oh, you're right, you're right. I'd forgotten about that.

McJones: But that was a kind of a late…

Taylor: I wonder how many of those were actually sold. Did you ever know?

McJones: I never ran into one because I had left Xerox by that time. I only recently have found people at GE Research who had bought some of the Lisp machines in the early 1980's.

Trying to summarize this, the Xerox period was an amazing period in terms of inventing the future and having an incredibly smart, feisty group of people who didn't quite blow each other's heads off, and had a lot of fun. I was across the street so I could only see the roof rise every so often.[23] Just trying to think, what else should I be asking about that period, Bob?

Taylor: Well, I wish I could remember more of the names of all the people that contributed.

Beckman: Chris Jeffers, what did he do?

[23] Xerox SDD, where McJones was employed from 1976 to 1981, occupied various buildings, all within walking distance of PARC and its cafeteria.

Taylor: He was in Alan Kay's group.

Ken Beckman: And Gene McDaniel?

Taylor: Gene McDaniel, I think--

McJones: He helped bring up the Alto operating system and he worked on--

Taylor: That's right, I think he was in CSL, wasn't he?

McJones: Yes, he was in CSL. Gene and I worked hard at Berkeley as both students and on the CAL Timesharing System.

Taylor: Good, give me some more.

Beckman: Well, the thing that comes to mind is two things, one, regarding the Dorado, I had a lot of information from the other side when you had told Pake you wanted 40 Dorados and then what I heard was after you left the meeting, everybody hitting the roof and thinking, you know, "Hey, that's absolutely crazy $40,000 and he wants to build all of these things." And of course, I got that from Pake through Gloria [Warner].

Taylor: Did I want 40 Dorados?

Beckman: Some large number.

Taylor: Are you sure it wasn't Altos?

Beckman: No, it was the Dorados because they were so expensive, you know, what I remember is people signing up for Dorados 24 hours and coming at three in the morning to take their four hours because there weren't enough of them. And you said, "This is crazy. Everybody needs their own Dorado."

Taylor: I guess you're right, yes. What I remember about that is that I thought about the investment in equipment on the first floor of PARC, a hell of a lot more than $40,000 a person. That's something Pake never thought about.

Beckman: This may or may not be relevant, but D0's and things.

Taylor: I don't know what happened to them.

McJones: One thing that might be interesting to just talk about briefly was spinning out ASD and SDD, just your impressions of what made that happen.

Taylor: Well, I thought somehow or another that was connected to Boca Raton, but maybe not.

McJones: I think actually, they'd happened before. SDD must have gotten started around late 1975 or so.

Taylor: Well, I helped recruit Steve Lukasik to head one of those organizations. I don't remember which one.

McJones: I think it may have been SDD in the very beginning

Taylor: He came from ARPA and I had known him at ARPA. And I remember after a couple of years he got discouraged. And I just remember all that as seeing a series of people get discouraged.

McJones: Well then Dave Liddle took over.

Taylor: And John Shoch was there for awhile. Harold Hall, who was a cipher, was there for awhile doing that, supposedly. I don't know, from my perspective, that organization never took hold of anything. I mean, it never--

McJones: Well, it built Star. SDD built Star.

Taylor: Yes, but it couldn't--

McJones: Yes, it was not a successful product.

Taylor: Star was a failure and one of the reasons Star was a failure is because Xerox never trained a service or maintenance staff who were knowledgeable about it. They never trained a sales staff properly. To recruit the sales staff for the Star they would ask copier salesmen to move over to sell the Star. Well, the only copier salesman who would take that offer would be the ones who were failing. If you're a successful copier salesman, there's no way you're going to go join that organization.

McJones: Right, then you'll have to develop a whole new business.

Taylor: Right, so the salesmen they got were not among the best and the training program that they gave them was terrible. Some acquaintance of mine at Georgia Tech who Xerox attempted to sell Star to said he knew more about the Star than the people who were trying to sell it to him. It was just

embarrassing. So, all that speaks to the fact that Xerox corporate simply didn't know that business and they were not a computer company. The contrast between working for Xerox and working for DEC is amazing. I had a corporate executive at Xerox say to me one time, "If what you're doing there at PARC is so important, why isn't IBM doing it?" These are people who thought that IBM was a really technologically advanced computer company.

McJones: Well, IBM thought it was. <laughs>

Taylor: Everybody's entitled to their own opinion.

Beckman: When you were talking about Future Day you were asking about the head of Xero; was that C. Peter McCullough?

Taylor: No.

Beckman: Beforehand.

Taylor: After Peter McCullough.

Beckman: Kearns?

Taylor: Yes, David Kearns, that's who it was.

McJones: Did the folks at PARC, your people, the CSL people, ever talk about alternative ways to turn the Alto itself into a product?

Taylor: Yes, John Ellenby was asked to put together a proposal that would do that and--

McJones: Well, in fact, okay, so he did the Alto II, didn't he, which was a very solidly re-engineered Alto. Of those 3,000 or 4,000 you had discussed earlier they were probably mostly Alto II's from Ellenby's ….

Taylor: He did most of the work required to get El Segundo to build them. I don't think he did much of the redesign. I think Thacker did that, but yes.

McJones: Production engineering.

Taylor: Yes, you had to coordinate a lot of production.

McJones: In some sense you would say it's another one of these second system effects where the Star was a different group of people, with some overlap, and had much more grandiose plans…

Taylor: That's right. Some people will argue that the Star was too ambitious in its design and therefore, it ran too slowly and cost too much. So, you could have trimmed down its design and decreased its cost and increased its speed, some people say.

McJones: Decrease the degree of integration. For example, on the Alto, each application would run as the only thing in the machine; it would exit memory and another piece of software would be loaded and that worked quite well, whereas Star as a fully-integrated suite of applications that were all resident all the time and required vastly more RAM. You can imagine porting the Alto software to something in the late 1970's, maybe running on a Motorola 68000 chip and have a very competitive offering, in an alternate universe.

Taylor: I recall this; when you mention alternate proposals, there was a proposal that--

McJones: Are you thinking of Bob Belleville? He was over on the SDD side but he put together a very interesting little system, you know.

Taylor: Yes, and what happened to that?

McJones: It, I think, died.

Taylor: Yes, but how? Who killed it? I recall Pake responding to something that came out of SDD that laid various conditions on corporate that Pake objected to, and I can't remember what they were. I can't even remember what the proposal was but Pake thought it was outrageous that a development organization, a relatively small organization within a large and successful company would expect the company to give them this or that or the other. And I don't remember what these things were that they were expected to do. But maybe it was to return all the sales, the gross sales revenue back to the development organization or something like that. I just remember Pake being upset about it. I'm trying to remember who he was upset with. It could have been [Donald] Massaro or [David] Liddle.

McJones: Oh yes, Liddle and Massaro ran the product group. So--

Taylor: Ken's falling asleep.

McJones: Yeah, this might be perhaps a good time to wind down for today and--

Taylor: Whatever you say.

McJones: Okay, well why don't we adjourn.

Taylor: Well, I didn't realize it was so late.

McJones: Ttomorrow we could do Digital [Equipment Corporation Systems Research Center] and perhaps we would all have further things that we thought of to do as a recap.

Taylor: That's fine.

McJones: I think, for example, Wes Clark, we didn't really do justice to Wes Clark.

Taylor: No, we didn't. Yes, be sure and bring him up tomorrow.

McJones: I'll make a little list of things like that.

Taylor: Because he contributed a crucial idea to the ARPANET, which very few people know about.

McJones: The idea of using a separate little router machine to isolate the network and provide some uniformity in the midst of the heterogeneity.

Taylor: To control the communication without centralizing it.

McJones: Yes, good. Well, I'm very pleased with the amount we were able to cover. I feel like I'm not a very good interviewer but you've been a great interviewee.

Taylor: I think you're a better interviewer than I am interviewee, so there you are.

[Crew talk omitted]

[Brief discontinuity at 2:46:48 – camera momentarily turned off?]

Taylor: [Bob discusses his attendance at the Future Day event in Boca Raton.] And Xerox Travel or somebody paid for it, Pake never approved of it. It never got to Pake. He didn't approve it. He took Burt Sutherland with him and they had seats in the auditorium, side by side. I broke through security, went through a side door that nobody was watching and climbed up to where the lighting and cameras were up in what would be balcony height and watched from the balcony behind one of the spotlights.

McJones: Why were you not invited?

Taylor: It's George Pake.

McJones: But you had created all that stuff; made it possible.

Taylor: Well, George Pake didn't think so.

[Personnel matter unrelated to Taylor's history omitted]

[DVD stops here; audio transcription apparently continued]

[Crew talk omitted]

END OF TAPE 3

McJones: It is Saturday October 11th and we're going to continue where we left off yesterday. We were near the end of your tenure at Xerox. What sorts of things caused you to move on?

Taylor: Well, I'd been there for 13 years by the time I left. I had pretty much given up on Xerox trying to do anything with our technology. In the final analysis they built for themselves a laser printing business, which became a billion dollar a year business, which more than paid for the investment in PARC Research and all of their investment in Webster Research. So when people say Xerox didn't take advantage of the work we did at PARC, that's both true and false.

It's true that the stuff that they dropped on the floor they did not take advantage of, while five or six other companies did. At the end of a book called *Fumbling the Future*, written by a couple of business writers, they interviewed George Pake as to why Xerox didn't take more advantage of all this stuff. And George Pake said, "Well, it was too early." But nobody told Bill Gates or Steve Jobs or the people at Cisco and Sun and 3Com and Adobe, and a number of other companies that it was too early. They just didn't see it that way. George Pake was something of an enigma. He was a physicist, and a lot of physicists I have known are pretty arrogant. My boss in ARPA [Charles Herzfeld] was a physicist. He was the exception. He's a really good guy and easy to talk with and interested in listening to people and what they had to say, whereas Pake didn't seem to understand what was going on right under his nose. We offered to put an Alto in his office as soon as we had Altos, and he wasn't interested. Some years later while we were still there he did put an Alto in his office, but he never used it. I think he was of the "I don't type" community. Typing is for underlings. He would never say that explicitly. He was just very strange. I think in the 13 years I was there he was in my office once, maybe twice. He would never come down to see us. One of the people in my lab once said to me that he thought Pake was afraid of us. It's certainly true that Pake avoided confrontation and that may have been part of it. There are certain personalities in different people that I'm sure you've known over the years who go out of their way to avoid confrontation. Even if they think someone's ideas aren't any good they will not confront.

In any case, after 13 years or so and my dealings with Pake and pretty much the rest of the corporation I decided to leave. The thing that kept me there, mainly, for so long were the people in my lab who were really exceptional folks. And so it was a real pleasure to work there from that standpoint. But from the standpoint of the support and understanding that I got from the rest of the company, it was pretty much nil. It was a copier company. We used to say, “The selenium tail is wagging the silicon dog.” It was a copier mentality. The first time I visited the Webster Research Center, which is where the copier research went on outside Rochester, New York, the head at one of the labs at that center greeted me at the lobby as I arrived. And the first thing he said to me was, "You know, the computer will never be as important and valuable to society as the copier."

McJones: There was no concept of creating information, being able to manipulate information?

Taylor: And I just thought to myself, “Is this the way it's going to be?” That was one of the most ridiculous statements I had ever heard. The computer is a general purpose machine. A copier is very--well never mind. It's so obvious. The environment created by those views of the <doorbell, dogs barking>.

Beckman: The model that I think you’re referring to and which actually persists with a lot of people is that the secretaries print out the email that the mail manager or boss-type reads on paper. That was the original model when the computer showed up, but the one you’re describing, they got email, it was printed out, then they read it.

Taylor: We should put that on the tape. Did you just record that just now?

Beckman: Yeah, it’s off mike but it’s there.

Taylor: It will get in the transcript? Or do you want me to repeat it. Are you running the tape now? Because there are some other things I was trying to think of, of that sort. They've escaped me.

Beckman: It was typewriter thought processes..

Taylor: While we were still at Porter Drive, probably the first year or so, Gloria Warner, Pake's secretary, got a new IBM Selectric put on her desk. And she and a number of other people were clustered around it. It had just arrived, and they were admiring it, looking at it. And I passed by this group and saw what they were doing, and I walked up to the typewriter and patted it on the top and said, "Gloria, we're going to replace this." And they just looked at me like I was crazy. And I just smiled and walked on.

Beckman: It's a very slow transition actually with Xerox buying Versatech, and these print wheel companies.

McJones: Diablo.

Taylor: It's funny when you look back on it. At the time it was sort of pathetic.

McJones: Time wounds all heels.

Taylor: I guess I should try to push on. It's bothering me that these things that I wanted to tell you flash through my mind, some of them even a few minutes ago, and I lost them because I was talking about something else. Maybe they'll come back. In the process of my leaving, I had decided to leave and I didn't have any idea where I was going to go, what I was going to do. And I had always advised people never to leave a job if they don't totally have another job. But I wasn't willing to follow my own advice. But to help me leave, now I remember. Several years before I left, Pake hired Bob Spinrad, who was a senior technical person at SDS in El Segundo, to come and head PARC while Pake was made head of corporate research, replacing Jack Goldman, who I think had retired. And that meant that Pake had to spend some time back in corporate headquarters on the East Coast from time to time. And PARC needed, therefore a new head. So he recruited Bob Spinrad, who was a computer guy. I can't remember whether he was a physicist or an electrical engineer. He didn't behave like a physicist, so I'll assume he was not a physicist. But because he was from SDS I was highly suspect of how imaginative and supportive he was going to be, given XDS' previous behavior over the years. So I proceeded to get to know him as best I could, and he turned out to be a perfectly reasonable, bright fellow. And we began to have frequent discussions. And I began to ask him what he thought about PARC's investment portfolio, the fact that the computer research at PARC was less than a third of the total PARC investment, when in fact the productivity of the computer research at PARC dwarfed anything else that was going on there. Since he was head of all of PARC he took an interest in this. And he was in the middle of going through every project in every laboratory as a project review, which required a lot of time. He probably spent close to a year going through each of the labs and each of their projects. And by the end of that time he had become convinced that PARC needed to readjust its investments and invest more in computer research.

So Bob Spinrad went to Pake and said, "Over a long period of time I think we need to phase more resources into computer research here and less into the other areas." There was a material science lab, so called. No, it's called a general sciences lab. When Pake went back to corporate headquarters to give his annual report about what was going on, he talked about the science, which was the general science lab and the optics lab. And what did he call us? We weren't science. We were something else, maybe engineering. I'm not sure, but we weren't science. We were talking previously in this interview, you and I, about theoreticians. We had some mathematicians in our lab who could run circles around any physicist in PARC. If you want to talk about science, let's talk about the queen of the sciences. So Spinrad made this proposal to Pake. I wasn't there, but let's just say Pake rejected it rather strongly. Some of his fellow physicists who were in these other labs, I'm sure came and objected to any such move. The result was that Pake hired a fellow named Bill Spencer to come and take over half of PARC. He took half of PARC away from Spinrad and gave it to this guy Bill Spencer, a fellow physicist, and let Spinrad keep the other half of PARC, which included my lab. So I still continued to report to Spinrad. But it was a big demotion for Spinrad, and a real slap in the face because Spinrad's proposal was completely ignored, rejected. And he was demoted. So that had an effect on me. I felt in some sense that Pake was demoting me as well, because he knew perfectly well that Spinrad and I had reached a good agreement about the way PARC was headed and the way in which it was investing its resources. So that contributed to my wanting to leave.

Then the next thing, I guess a year or so later, somewhere in there: Spinrad was further demoted and put on Pake's staff or corporate staff. Spencer was now head of all of PARC, and Spencer was a control freak. He liked to tell people what to do. Toward the end of my stay there I had sent the annual salary proposal up to my management. I needed their approval to give people raises that I had specified. It came back that I could not give a raise that I had recommended to one of my people. That was Dave Boggs. I went up to see Spencer and said, "What's this all about?" He said, "Well, that's too much money." I said, "Do you know who Dave Boggs is and what he's done?" (co-inventor of the Ethernet.) "Yes, but it's too much money. I don't want to see you give him that kind of a raise." I was furious. In that same conversation we were talking about other people, not their raises but various things about them. And he came up with the idea that Butler Lampson wasn't all that smart. He said, "People seem to think that Butler Lampson is smart. He's not very smart." These things kind of compounded, when one idiotic opinion gets piled on top of another one, on top of another one. It's past time to leave. So someone, not in my lab, I think someone in SDD said to me shortly after this that he had learned from someone that I was unhappy and he was afraid I was going to leave, and he went to see Spencer to encourage him not to let me go. Spencer said, "Taylor's not going to leave. He doesn't have anywhere to go." Well, that got back to me. I can't remember now if I had already told him I was leaving. No, I guess not. Another element of this was when I had objected so much to his trying to run things from the top, he wrote a letter to me on paper, not email, and outlined various things I was to do. One of them was to meet with him every Monday morning at 9 o'clock or 8:30, something like that. Now, I would typically work from 10 in the morning or 11 sometimes until maybe 7 or 8 at night. And he knew that, so it was just another attempt to exert his authority.

I can't remember the sequence of events now, but at some point in there some people in my lab got together and went to see David Kearns at corporate headquarters, the head of Xerox, and voiced their concerns about what was going on and Spencer's interference and things like that. Xerox had a policy that if you had a serious disagreement with your boss you could go up a level. I decided to do that just to give Spencer a hard time, knowing that it wouldn't do any good. And so I went back to Stanford, Connecticut with Spencer. That's where corporate headquarters was. We didn't meet with Kearns; we met with one of his staff associates; I can't remember who. Spencer laid out his case that I had not trained any of my people to be my successor; that I had not supported technology transfer. I don't know – a number of other things. I had written a letter in reply to his letter, taking his points one by one and tearing them apart. Somewhere in this time, and I can't remember which came first, but the events now that I'm talking about were all within a week or two of one another. At some point in there we had our weekly meeting of the whole lab and I announced that I was leaving, and I left the building. This was 10 or 11 in the morning. Were you there Ken? It was the whole lab, and Spencer had even come to the meeting. I had said something in the meeting to the effect that Spencer had given me this letter, and I had written a letter back. Oh, and Spencer's letter said that I was prohibited from showing his letter to anyone. It's coming back slowly. So after I left the meeting, someone in the meeting, I don't know who, said, "I want to read to the rest of you Spencer's letter." He had a copy of it, and he read it. And then he said, "Now here's Bob's letter in reply to Spencer." And he read that. These letters were three or four pages long, something like that. They weren't just single paragraphs. At the end of all of that, the lab was kind of in an uproar in this meeting, and Spencer said, "Well, you heard him. He's left, what do you want me to do?" And someone in the back of the room said, "Quit." They were pretty upset. So I left and Pake then had his secretary send an email to the community, PARC and SDD, saying that I was leaving but that he wanted everyone to know that his support of computing would continue to be strong, computing research and development, and nothing was going to be any different. But what that email did was tell everybody that I was leaving when they hadn't known it except just CSL.

Well, then the word got out, and people began to send emails over ARPANET out to their friends around the country. And so I began to get phone calls from people wanting to talk to me about coming to work there. One of the phone calls I got was from DEC, Digital Equipment Corporation. And somebody came to see me from DEC; it may have been Sam Fuller, who was head of DEC research. That's who I ultimately worked for. We met somehow or another. We met at Forest Baskett's lab, which was another DEC lab in Palo Alto, at his request. I went and talked with him. This would be September of 1983, October maybe. I think I quit at the end of September. He offered me a job, but I didn't accept it. I didn't turn it down either. I just said, "Well, I want to think about it," because other phone calls were coming in. I wasn't sure what I wanted to do. He said, "We want you to build a lab for us in Palo Alto from scratch." I thought that was pretty interesting, but I didn't accept right then and there. I said, "Well, it sounds interesting and I may do it, but I'm considering other stuff." Well, the reason I'm telling you this part of the story is that a few days later two guys showed up at my front door who I had never met. And they said, "We're here from DEC, Digital Equipment Corporation at the instruction of Sam Fuller," who's a corporate VP, "and we're here to help you find a building." I said, "What?" "To find a building for you new lab." I said, "Well, I haven't accepted the job yet." And they said, "Well, we know, but let's see if we can't find a building and maybe that will help convince you." So we went out looking for a building. And we found this great site across from Palo Alto train station, a four story building [130 Lytton Avenue]. The outside was finished, but the inside was completely unfinished, which meant that we could lay it out any way we wanted to. So we got the building, and I took the job, and I began to work with an architect and the construction people. And I designed the interior. And it got built.

We began hiring people before the inside was finished. I think we had maybe five offices for the first five people or something like that. As we would finish part of it we could hire more people. And so the first part of the building was finished. The building was built in two sections. The first section was completely finished by, I think, January of 1984. And by the beginning of January in 1984 a dozen people had left CSL PARC and come over to join me. When they first learned that I was going to build a new lab for DEC they would contact me by phone or come see me at my house before they quit Xerox. And they said they wanted to come if I wanted them, whenever it was ready. And I said, "Well, in order for you to do that, you have to first write a letter to Spencer on paper instructing him that you are going to look for another job. And you don't have to tell him where you're going to go, but you need to get it established as a matter of record that you're going to look for another job." So they did that, and they showed me copies in each case so that I could know for sure that they had done it. That served to work against Xerox. Xerox later claimed that I couldn't hire these people. Not only that, Xerox claimed that they had a hold on the technology that we had created. I don't mean in detail, I mean in general. Xerox arranged a meeting with Sam Fuller and I don't know if anyone else from DEC. I don't know where the meeting was, I wasn't there. But somebody at Xerox, it might have been Jerry Elkind, told Fuller, "We understand that Taylor's built this lab. We want you to know you cannot do research in personal distributed computing." Fuller just laughed at them. And they said, "And you can't hire Xerox people." Xerox was a DEC customer, and the DEC salesman who handled the Xerox account came to me after I was already a DEC employee and said, "Hey, you're hurting my business here. You're hiring people away from Xerox and Xerox buys DEC computers from me, and that's not good for me." I said, "Well, that's too bad. But these are good people, and they want to leave. And they can go anywhere they want to. They don't have to come here. They notified Xerox before they left Xerox that it was their intent to look for another job; too bad." He got over it eventually, but he was pretty upset.

So there were a lot of forces here that people normally would not think of as something corporate America would do. But corporate America is no better than the worst of individual Americans. From then on my life, in terms of my working life, and the support that I got from people above me and around me in

addition to the people that were in the lab was just really good. I enjoyed working for DEC. I worked for DEC almost as long as I worked for Xerox, about 12 years. DEC eventually got overtaken by disruptive technologies, just as Xerox in some sense got overtaken by disruptive technology. I love that phrase. The phrase was invented by some Harvard Business School people. Disruptive technology is a technology that's available to your company, and if it succeeds it will undermine your fundamental business, and therefore you'd better invest in it.

McJones: Disrupt yourself rather than having somebody else do it.

Taylor: Right; a lot of companies have ignored that at their peril. IBM hit the skids for a long time because DEC came up with a minicomputer and put a dent in IBM. And then the personal computer put a dent in DEC and would have put a dent in IBM had they not had that Boca Raton lab, which to their credit they completely took a hands off approach to. They didn't mess with them. They didn't try to tell them what to do or what not to do. They left the budget alone. And that lab did something that was totally anti-IBM and staggered the world. I was in disbelief that IBM would allow something like that to happen, but it saved their necks in many respects. That's neither here nor there. So I think we're through with Xerox now, and good riddance.

McJones: Could you talk a little bit more about how the core of the Systems Research Center were these people that wanted to follow you over from Xerox PARC.

Taylor: Right, the name of the DEC lab was, I'm sorry I didn't mention that, was the Systems Research Center. It was in Palo Alto, and yes its core came from the Xerox Computer Science Lab.

McJones: But you also looked outside.

Taylor: I did. And there were people from the Xerox development organization that also joined us. You were one of them. And Ken Beckman joined us from PARC, probably in January. I don't remember what month. Do you remember? Maybe it wasn't January, maybe in the summer time because we didn't have any ability to create a video facility. We didn't have the proper room. In fact, we didn't have the proper meeting space until long after Ken arrived, because he helped design it. We had a really neat meeting room for the whole lab. We had conference rooms scattered around the building, but we had one large room that everybody in the lab could fit in, an auditorium. And we had a huge display up front that was computer-generated. Ken helped design most of this. We had it set up so that if you had a software demonstration running on the computer in your office you could pipe it over to this auditorium. Or you could bring in a PC and play it into this huge screen. It was a very nice set up. The lab was ideally set up, I thought, of course I'm prejudiced, for what we were doing. I designed it so that we had lots of light brought in from the outdoors. And many of the inside walls were also glass so that the light would come through an outside window, across a space, and through an inside window into the inner space. We had lots of places where people could sit down and talk. I recently toured VMware's facility, a part of it, and they followed the same principles. It's really a nice site that they've got. I understand that Diane Green was responsible for most of that. I don't know that she ever saw the Systems Research Center of ours, but the VMware site reminds you of that because of the way they use light.

McJones: And the openness, trying to encourage people to come out of their offices and interact. SRC was very good along those lines.

Taylor: At SRC we did a number of different things. I think our networking stuff probably helped the company more than anything else. We built a high bandwidth local area network with lots of reliability built into it. A piece of it could fail, and the network would keep running. And it was mainly for buildings, or campus-wide facilities. And DEC used some of its principles in some of their products.

McJones: I think especially the AN2[24] follow on was the one that had a direct influence on products.

Taylor: Yes, you're probably right. There were several variations of this original idea. And I can't remember the details about them.

McJones: Another major project that was directed to help the rest of the company was the Alpha Demonstration Units[25], Chuck's original machine to use the Alpha chip.

Taylor: Yes, well we built the first Alphas, we didn't build the chips. But we built the systems and at that time that was the world's fastest computer.

McJones: I think it really helped a lot to move up the schedules for software development in the rest of the company because they would have an actual machine to run on.

Taylor: Right, because we could build an actual Alpha faster than other elements of the company. That means we could put them in the hands of the software developers sooner and help accelerate all of that.

McJones: You could probably trace that in some sense back to MAXC in terms of the capability to build prototype machines.

Taylor: Yes, that's fair. We built the first multiprocessor workstation called a Firefly[26]. It was never a product, because we could never figure out how to expeditiously program it. But one of the reasons we built it was to see what the problems would be in trying to build a multiprocessor workstation. <Pause to answer door.> So we used Fireflies in our offices in our work, but we never tried to get the company to make them into a product, because we weren't happy with the ease of programming, taking advantage of multiprocessing. And even today that's still somewhat of a problem for people.

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[24] Susan S. Owicki. A perspective on AN2: local area network as distributed system. In *Proceedings of the Twelfth Annual ACM Symposium on Principles of Distributed Computing* (Ithaca, New York, United States, August 15 - 18, 1993). PODC '93. ACM, New York, NY, 1-11. DOI= http://doi.acm.org/10.1145/164051.164052

[25] Charles P. Thacker, David G. Conroy, and Lawrence C. Stewart. The Alpha Demonstration Unit: a High-Performance Multiprocessor. Communications of the ACM, Volume 36, Number 2 (February 1993), pages 55-67.

[26] C.P. Thacker, L.C. Stewart, E.H. Satterthwaite, Jr., "Firefly: A Multiprocessor Workstation," *IEEE Transactions on Computers*, vol. 37, no. 8, pp. 909-920, August, 1988. http://doi.ieeecomputersociety.org/10.1109/12.2243

McJones: Especially important now those chips aren't getting that much faster but they're going multi-core – two, four, and eight. The Firefly was there 20 years ago.

Taylor: Speaking of things in advance, we also built the first electronic book called Lectrice[27]. And it looks very much like the thing that Amazon puts out today. What's that thing called? Kindle? I haven't seen a Kindle, so I can't tell you exactly the comparisons between it and Lectrice but the descriptions and the pictures look similar. And the Lectrice was done in the early 1990s.

McJones: I think that's right, probably around 1994, 1995.

Taylor: In the mid 1990s, okay. Well, I left in 1996. I think it was done a year or two before I left. Let's see, we did some other things. We did Petal. How would you describe that? A memory that --

McJones: There were several layers to it[28, 29]. The Petal system was a storage system that used redundancy.

Taylor: Yeah, but what were its salient features? I can't remember. I think I can only remember things if they were 40 years ago.

McJones: It was high availability, high reliability of the individual storage with scalable performance.

Taylor: And it worked, I recall.

McJones: So SRC was one of several of the research labs including several in Palo Alto and one in Paris. I think you've had a fair amount to do with interacting with other labs and helping to found them.

Taylor: Right, now talking about other things in DEC and one of them especially that I was involved in besides SRC. We have to go back to when I was at the University of Utah and the year 1969. I became friends with three graduate students there, Alan Kay who we've already talked about, Patrick Baudelaire who we mentioned, and Henri Gouraud who we haven't mentioned. Patrick Baudelaire and Henri Gouraud were Frenchmen, as you might imagine, who came to the U.S. to get their PhD's. Both were interested in graphics, so they selected the University of Utah to do their PhD's. I met them while they were there as graduate students working on their PhD's, and my family and their families became friends. When I left Utah to come to Palo Alto to work for Xerox I brought, two computing staff from Utah with me:

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[27] David Chaiken, Mark Hayter, Jay Kistler, Dave Redell. The Virtual Book. Research Report 157, Systems Research Center, Digital Equipment Corporation, November 11, 1998. http://www.hpl.hp.com/techreports/Compaq-DEC/SRC-RR-157.html

[28] Edward K. Lee and Chandramohan Thekkath. Petal: distributed virtual disks. In *Proceedings of the Seventh international Conference on Architectural Support For Programming Languages and Operating Systems* (Cambridge, Massachusetts, United States, October 01 - 04, 1996). ASPLOS-VII. ACM, New York, NY, 84-92. DOI= http://doi.acm.org/10.1145/237090.237157

[29] Chandramohan A. Thekkath, Timothy Mann and Edward K. Lee. Frangipani: A Scalable Distributed File System. ACM Symposium on Operating Systems Principles, 1997, 224-237.

Jim Curry and Bob Flagle. And after I was at Xerox for, I guess, a year, not long after Patrick Baudelaire and Henri Gouraud got their PhD's,I asked them to come join me at Xerox PARC. Henri and his wife wanted to go back to France, and they did that. And Patrick and his wife and children came to PARC. Patrick worked with me at PARC for a number of years, maybe five or so. Then he and his family went back to Paris, where he and Henri eventually built a startup of some sort [Tangram Inc.]. And a few years later this startup began to have problems, and I thought that Patrick and Henri might be available.

So I talked to my boss Sam Fuller and made a case that DEC should start a research lab in Europe and that we had two people to help start it potentially who could really make a difference in terms of getting good people early. The lab would be in Paris, which was a good location for the European entity. Sam liked the idea. Ken Olsen liked the idea, he was head of DEC. So Sam put me to work to go get Patrick and Henri on board and help them get a new lab built, since I had done that twice; once with brick and mortar and once with brick and mortar already there. So that was a lot of fun, and quite easy to do since it was things I was familiar with. We built the Paris lab; it was called Paris Research Lab. It was actually in a little town [Rueil-Malmaison] on the outskirts of Paris, but you could get to it easily by the RER from the center of Paris. That lab was in existence for a number of years. What do you think, ten, eight, maybe eight or so years? We had a lot of exchange of people, researchers going from SRC to PRL and PRL back to SRC, sometimes just for a few weeks, sometimes a few months, sometimes even a few years. It was a very healthy arrangement. And we would supplement one another's research programs sometimes by jointly doing things. The Paris Lab hired some really good people, so there was a really good match of nervous systems between the two groups, a really worthwhile experience.

We had a sister lab in Palo Lab called the Western Research Lab, originally built by Forest Baskett, who helped Sam Fuller recruit me. While he was a professor at Stanford he had worked some in my lab at Xerox PARC, kind of like a quasi-sabbatical arrangement. So I knew him pretty well. And we got along well at SRC with Forest Baskett's lab as well. But we probably had even closer ties with the Paris Research Lab strangely enough. It was a third of the way across the world. Forest left the Western Research Lab a few years after I came to DEC. I don't remember exactly how many. But he built a good lab also. DEC had a number of strong research assets, and they used them better than Xerox used theirs, but they didn't use them as well as they should have.

The first RISC machine inside DEC was built by Forest's lab. Forest actually wanted it to be a DEC product. Eventually, I guess, DEC did embrace that architecture but much later.

McJones: This was the Titan Project at WRL.[30]

Taylor: Right, Titan. Is that the name of the first one? I think that was it. And there was a lot of arguing back and forth between the Titan people and the conventional machine people in DEC, as you might expect. And I can't remember how DEC embraced RISC technology eventually, but I believe they did. Do you recall any of that?

30 Michael J. K. Nielsen. Titan System Manual. Technical Report 86/1, Digital Equipment Corporation Western Research Lab, September, 1986. http://www.hpl.hp.com/techreports/Compaq-DEC/WRL-86-1.html

McJones: Just a little bit. For example, I think they were also having a lot of different groups. They had this culture of different groups competing with each other. Once the investment threshold is large enough, it's not necessarily so healthy. Dave Cutler was doing of the earliest RISC things in the product groups before he went to Microsoft [the PRISM project at DECWest in Bellevue, Washington[31]].

Taylor: Oh, I didn't know he was doing RISC architecture. I thought of him as a VMS [operating system person].

McJones: Well he is a person of many talents, both hardware and software. That was a major one for a while. Then it was really Alpha--

Taylor: That took over, right.

McJones: The first and only RISC that they built; they were also selling MIPS-based machines

Taylor: That's right. It's coming back now. Well that was a big commitment to RISC, once they did the Alpha. That was sort of whole hog. But that came a number of years after Titan, probably five, four, three.

McJones: They started into Titan pretty early in their existence at WRL.

Taylor: I look back on the DEC experience with a lot of pleasure.

McJones: To work for an engineering-driven company.

Taylor: They understood what we were doing anyway, better than Xerox. We could have been working for a company that built washing machines as well as building copiers, as far as Xerox's understanding of what we were doing was concerned. Oddly enough, the invention of the Ethernet made a big difference in the copier construction performance. Once they put Ethernet inside copiers-- do you know about this story?

McJones: No, I don't.

Taylor: Well there's a lot of communication that goes on inside a copier between various elements of it. And a mini Ethernet facilitated that.

McJones: It was a bus.

[31] http://bitsavers.org/pdf/dec/prism/

Taylor: Yes, as a bus, right.

McJones: So the Ethernet was another connection between Xerox and DEC.

Taylor: That's right. Xerox was not interested in productizing the Ethernet, but Bob Metcalfe was, and so he left Xerox and founded 3Com. And I think before he left Xerox and certainly afterwards, he helped broker a licensing arrangement between Xerox, eventually 3Com, and DEC, and Intel to make Ethernet components. Xerox didn't make any of these, but they signed the license that enabled 3Com and others to contribute to building Ethernets. And that made a big difference in getting the Ethernet out into the world. There was a standards fight about local area networking between Ethernet and Token Ring, which was an architecture invented by IBM, I think, or at least developed by IBM. And I don't think that standards fight as a standard was ever, at least not early on, legally resolved the way you would expect a standard to be endorsed by somebody, IEEE or somebody. But the fact is the Ethernet became a *de facto* standard, because people who were interested in buying local area networks looked at the Token Ring and at the Ethernet and they bought Ethernets. So the Ethernet won, at least in the marketplace. I'm sure it's the standard by now. I think it took a while.

McJones: IEEE did initiate a big effort and, from what I hear from ex-Xerox people, mostly changed all the wording around but left the design essentially intact. But yes, the combination of DEC being very enthusiastic and building controllers for every one of their line, and Intel making available silicon, sure helped to make it be a pervasive *de facto* standard.

Taylor: Right.

<Crew talk>

END OF TAPE 4

Taylor: -- my memory, but now it's gone. The memory is a strange thing. Yes?

Beckman: There are Sketchpad films, and Alan Kay used to use them a lot in his talks, as a reference.

Taylor: Well Alan Kay's probably a source of a lot of historical stuff.

McJones: That's right.

Taylor: Maybe he can show you a working Dynabook.

<crew talk>

McJones: We were talking about Digital [the Systems Research Center] and the good collaboration with PRL. One thing I would like to do before we wind down is go back to someone that we touched on only lightly yesterday, Wes Clark, who plays an important role. So maybe we could pick it up in the early 1960s.

Taylor: Well, it fits with the Digital context, because Wes Clark worked with Ken Olsen, before there was a Digital, at MIT.

McJones: Would that be Lincoln Laboratory?

Taylor: On Whirlwind[32]. Do you remember that computer?

McJones: Yes.

Taylor: Was Whirlwind built at Lincoln, or at MIT proper? I don't remember. But they worked together on that machine. My recollection is that when Ken founded DEC, which would have been probably 1957 or thereabouts, his first product were register transfer modules, not a complete computer. Does that ring any bells with you?

McJones: Yes. The story I heard was that their funding source didn't want them to go into the computer business.

Taylor: Right, I think that's right. I can't remember their funding source.

McJones: It was an early venture capital—[firm, namely American Research and Development Corporation].

Taylor: Was it? So at that time, Ken went off and founded DEC and Wes Clark stayed at MIT Lincoln Lab. I think I told you about Wes Clark introducing Licklider to computers at Lincoln Lab?

McJones: Right, a TX-2.

Taylor: Right. He introduced Lick to the TX-2. Then sometime in probably the 1963, 1964 time period, Wes Clark and a group of his fellow workers, including Severo Ornstein, left Lincoln Lab, and went to Washington University, St. Louis. But before they did, they designed and built what is arguably the first personal computer. It's called the LINC, L-I-N-C[33]. It was done at Lincoln Lab. It was named the LINC

[32] http://www.bitsavers.org/pdf/mit/whirlwind/

[33] Wesley Clark. The LINC was early and small. In *Proceedings of the ACM Conference on the History of Personal Workstations* (Palo Alto, California, United States, January 09 - 10, 1986). J. R. White and K. Anderson, Eds. ACM, New York, NY, 133-155. DOI= http://doi.acm.org/10.1145/12178.12187

[Laboratory Instrumentation Computer] as a result. It was a personal computer in the sense that it was controlled by a single person, intended for use in a laboratory, typically a biomedical laboratory, to facilitate running experiments. So it was small, relatively. Mainframe computers of that day took up rooms. This took up a small space. It wouldn't fit under a desk, but it would sit nicely beside a desk, like a small refrigerator. It had a small display, and it was interactive. The operator sat at a keyboard and controlled the machine interactively. And so it was a personal computer. DEC later commercialized it, and it came out as a PDP-12, I think. Does that ring any bells with you?

McJones: That's possible. In fact, they may have also used one of their other designs, such as a PDP-8, to build a version that could run in that environment. But maybe the first one was a PDP-12. I don't remember.

Taylor: I don't either. But there were a number of firsts there. An important one was LINCtape, which became DECtape, which was the first tape drive that had a directory on it. So you could actually use it to look things up and then find them and then go to them.

McJones: And I think rewrite in place, it acted more like a disk.

Taylor: That's right. In that sense, more like it had some disk properties.

McJones: It was very rugged and relatively low cost.

Taylor: It was rugged and low cost, yes.

McJones: A precursor of modern storage media.

Taylor: And Tom Stockebrand was its inventor. Boy, now I'm reaching to come up with that. But Tom Stockebrand was an interesting—is still probably—an interesting guy. Last I heard, he lived in Albuquerque. So when Wes Clark got to Washington University, St. Louis, his research was funded by ARPA. I don’t remember if it was Lick or Ivan who initiated that funding; it could have been either one. But by the time I got to ARPA, his was one of the groups ARPA was funding. So I got to know him early on and liked him a lot. He's still a very dear friend of mine. After a year of trying to get [Larry] Roberts to come to ARPA, I succeeded, as I had said earlier, and we got the ARPAnet RFQ issued, and we got responses, and we settled on BBN. But in that process, before the RFQ, in the process of developing an overview design that would go into the RFQ, Larry was exploring with a contractor specially signed on for this job, the architecture of the ARPANET, that is, how would it be controlled? How would it be linked up? What would its distribution principles be? That sort of thing. One of the ideas that he happened to mention to me was putting a central control computer in the middle of the country. The Department of Defense has a base in Omaha, a SAC [Strategic Air Command] base in Omaha, Nebraska, and that would be a centrally located site to have a control computer for the ARPAnet. That sounded like a really bad idea to me—nothing to do with SAC, but the idea of centralized control.

Now there was a strong element in the culture, in those days, mid 1960s that was reflected in me. I didn't trust large organizations. This is part of the movement of the '60s. I didn't trust large organizations. I didn't trust centralized control of anything. Small is beautiful. There was a whole host of related gut feelings that some people had in those days, and I was one of those people. So that was part of my reaction against this notion. Now Roberts didn't say, "Well, this is the way it's going to be." He said, "Well, this is one of the things that's worth considering." I didn't have a good alternative to put in place. I just thought that was a really bad idea. It's really frustrating when you think something's a bad idea, and you don't have anything to suggest to replace it. So I talked this over with Lick, and he agreed with me, it was a bad idea. I talked it over with Wes, and he agreed that it was a bad idea. But neither of them, at the time, said anything about what we might do instead. So then in 1967, I believe it was, I had an annual ARPA contractors meeting, this time at the University of Michigan, Ann Arbor. By that time, I had talked with a few ARPA principal investigators about wanting to do the ARPAnet, but I hadn't talked in front of the whole group about it. And so at that meeting, I announced that that was what ARPA was going to do. After the meeting, I was driving back in a rented car to the airport, and in the car was Dave Evans, who was our principal investigator at the University of Utah, Wes Clark, who was principal investigator at Washington University, St. Louis, Al Blue, who was my contracting expert and worked for me in ARPA, and Larry and me. I was driving the car, and Wes Clark, at the meeting, had passed me a note as I recall, saying, "I have an idea about the ARPAnet." And when we got in the car and we were driving, I asked him, what was this idea you had about the ARPAnet, Wes?" He said, "Well, you ought to separate control of the ARPAnrt from the communication throughout the ARPAnet." He said, "The way to do that is to put a small computer at every host, and you connect the small computers one to another, and that solves your variability problem, the problem of the differences between machines. You use that as a common language, so to speak, as a connection between different machines. And the other ends of each of the small computers are connected to their local host." And I thought that was brilliant, and I said so. I said, "Dave?" He said, "It sounds right to me." And that's the way the ARPANET architecture got fixed.

McJones: And those became known as IMPs.

Taylor: Interface Message Processors, yes, IMPs.

McJones: Do you know what happened next after that?

Taylor: Well, that idea got put into the RFQ, and so the people who were bidding on it knew that that's what they were dealing with. And when BBN got the RFQ, they looked around for small computers to use for this purpose, and they considered DEC, but Frank Heart is a very conservative guy, and he decided he wanted a militarized, rugged computer, so they picked a Honeywell machine that they would execute drop tests on. Of course, we did have to ship them around the country, and so they were going to be manhandled a bit. But anyway, that idea, I think, in my view was the most critical, key idea to the ARPANET. Wes Clark is its inventor, and hardly anyone knows that.

McJones: Maybe some more people will care about it.

Taylor: Well, maybe.

McJones: Wes stayed in touch with you, visited at CSL, at Xerox.

Taylor: Yes. He was a consultant for us at Xerox and at DEC. So was Licklider, by the way.

McJones: I was fortunate enough to meet Lick once late in the 1980s. I remember Chuck Thacker talking about Wes when he visited while they'd been working on the Alto design. Do you remember any of those stories?

Taylor: I remember one of them. The Alto had a multitasking capability of some sort.

McJones: Right down at the hardware level.

Taylor: Yes, that's right. And Thacker and [Ed] McCreight thought they had invented that notion. Wes was visiting us one day, and somebody, maybe me, mentioned that McCreight and Thacker had invented this idea. And he said, "Oh, well I'll have to send them some reading." This is typical Wes. He's very funny. Anyway, it turns out that the multitasking was in the TX-2, and it was invented by Wes, and it was fully documented in published journal articles about the TX-2[34]. So when McCreight was confronted with this, he said, "Well, I was only eight years old. I didn't read it," or something like that. So I'm sure that kind of episode repeats itself every day, or every few months, anyway, in the history of technology.

McJones: Right. But it's amazing how the people in the 1950s just got things off to a rip roaring start, inventing so many things.

Taylor: Yes.

McJones: So Wes is one of those relatively understated, very important people.

Taylor: Yes. That's also his personality. He's modest to a fault. Do you know about the DigiBarn Museum?

McJones: Oh, it's not too far from here?

Taylor: Boulder Creek?

McJones: Right. They restored a LINC, is that correct?

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[34] James W. Forgie. The Lincoln TX-2 input-output system. In *Papers Presented At the February 26-28, 1957, Western Joint Computer Conference: Techniques For Reliability* (Los Angeles, California, February 26 - 28, 1957). IRE-AIEE-ACM '57 (Western). ACM, New York, NY, 156-160. DOI= http://doi.acm.org/10.1145/1455567.1455594

Taylor: Yes. There's a working LINC there. And they had a LINC demo at the Computer History Museum, where Wes was in attendance, six months or a year ago? You didn't know about this?

McJones: I was unable to attend that. Did Wes actually help restore it?

Taylor: No, but some of his people did.

McJones: Severo [Ornstein].

Taylor: Severo helped and some other people. From a software-oriented point of view, the DigiBarn has a more interesting collection than the Computer History Museum, at least according to Severo and some people, circa 2008, which is where we are now. Now maybe the Computer History Museum will fix that over time.

McJones: Let's hope we'll see some competition here.

Taylor: Yes.

McJones: The DigiBarn has a lot of machines that are in operating condition.

Taylor: Their collection's not nearly as large, but they have an interesting collection. They have several Altos, and a number of other small machines. I think they probably focus more on interactive computing than they do batch computing, because it's a modest operation, run by Bruce Damer and his wife.

McJones: Maybe at this point it would be good to reflect back on your management career, which is what you've been mostly doing all these years.

Taylor: Oh, I thought you wanted to talk about Vietnam.

McJones: That's another interesting little segment there.

Taylor: I don’t know that it's all that interesting, but it was a part of my ARPA days. This was in the 1960s, during the Vietnam War. My job at ARPA was to fund computer research, which had nothing to do with the Vietnam War, in spite of what certain campus radicals would have you believe at that time. That reminds me. The behavioral science office in ARPA was without a director for a period of time in the mid 1960s, 1967, 1968, somewhere in there. And so Charlie Herzfeld, my boss, asked me to be the interim director of the behavioral science program, as well as direct the computer research program, something that Licklider had done as well. He'd started the behavioral science program. So one of the things I did in the behavioral science program was start a project in the Cambridge area called Project Cambridge, which put money in the hands of behavioral scientists at Harvard and MIT, to fund for their

use, a timesharing system. Behavioral sciences then, and probably even today, were data rich and theory poor. And there were tons and tons of psychological data in experimental psychology, sensory psychology, that they could have well used some computer help to process. The behavioral science program, like the computer research program, was totally unclassified. It had nothing to do with anything military. In fact, when the ARPA director would go before Congress, some congressman or congressional staff guy would say about a particular project, "Well, how does this help the Department of Defense X project?" And the director of ARPA would typically say, "This helps the nation, and therefore helps the Department of Defense." Something like that. So there was a different attitude projected in those days than later days. In later days, they saw fit to rename ARPA, DARPA, in order to put the word "defense" in front of Advanced Research Projects Agency, in order to appease the Congress, who were more inclined to give defense dollars their approval than just research dollars without some strong defense connection. Even a mission oriented connection, which greatly weakened DARPA, but that's another story.

So there was a retired physicist, a Nobel laurate from Harvard, who went on a speaking tour in 1969. The Vietnam war was still going on, and I was at the University of Utah, and he appeared at the University of Utah to give a talk. He drew a large audience in a large auditorium, and he gave a talk bashing the the Department of Defense and the Vietnam War. And he used as one of his examples this guy in the behavioral science office in ARPA, who's sitting there, at a computer, helping the behavioral scientists in Cambridge, Massachusetts mastermind the Vietnam War. It was absolute bullshit, from a reputable guy. Retired, but reputable. I was in the audience. It was all I could do to keep from standing up and hollering out, "Bullshit!" or something, because I was the person who'd started that project, and it had nothing to do with the Vietnam War. As much as I hated the Vietnam War, after my second visit there, it was clear that we had no business there, but I'm getting ahead of myself.

Okay, so we're back in 1967 or so, and Lyndon Johnson is now President, and he's running the Vietnam War from the White House. He gets report from Vietnam daily about a variety of things, logistics reports. How much sugar have you captured? How many bullets do you have? All kinds of things. These reports would come from the different services to the White House, and they would sometimes disagree. On one occasion, one famous example is that if the reports were all correct, we would have, at a certain point in time during those days, have captured one third of the world's sugar supply, something like that. And another famous example, one of the services was short on bullets, and the other service, which had the same caliber, had plenty of them. Things were screwed up. Johnson had a bad temper and he exploded one day to McNamara, and told McNamara to fix this. McNamara went to the head of ARPA, who reported to him, and said, "Don't you have some computer guy?" McNamara loved computers. "Don't you have some computer guy there that can go out and fix this, find out what's going on?" I was the computer guy. So Herzfeld asked me if I would go out there and see what I could do.

Well, I had made some friends on the Joint Chiefs of Staff. By happenstance, an army colonel, an air force colonel and a navy lieutenant commander. Now if you're a member of the Chiefs of Staff, just a staff member, and you go into a military command some place, they pay a lot of attention to you. So I knew that if I went out there and appeared before one of Westmoreland's people, they would say, "Who in the hell are you?" But if I took these guys with me, there would be a different story. And so I asked them to go with me to do this, and ARPA had a lot of clout in those days with the military. If someone from ARPA, especially an ARPA office director, asked one of these guys to do something, well they would jump at the chance. And these were three really good guys, too; they were guys that were fun to be with, which is how I happened to get acquainted with them in the first place, all through different circumstances. One of them, the air force guy, was a computer wizard, and had managed a lot of

computing progress in the air force. That's how I met him. I met another one at the defense agency where they run computer courses, which I took. I can't remember where I met the third.

Anyway, we all got on an airplane, and we flew to Vietnam. We stopped off in Hawaii, which was typical. I don't know, there were some odds and ends of things to do in Hawaii that would keep us there maybe a day or two. When we landed in Hawaii, a military staff car comes out to the airplane, and rolls out—I'm not kidding—a red carpet. And there's a stairway going down from the airplane. I paid no attention to this car. I had no idea it had anything to do with me. So we go down there, and these three guys that are with me steer me over to this car. I said, "What is this for?" The car had a driver and it had an attendant, some lieutenant or captain or something. "What is this for?" "Well, these people are assigned to you, to look after you while you're here."

When you go into a combat zone as a civilian, if you're representing the defense department, you're given a military rank that's the equivalent of your civilian rank, and you're given ID cards that have this rank on it, two cards. So prior to going on this trip—I'm sorry for jumping back and forth—one of the requirements to go on this trip was, I had to go down to this place in the bowels of the Pentagon some place and get a picture made, get these ID cards made, and get some shots for cholera and other things. They make the cards right there, plasticized things, and they hand them to me. "Why are you giving me two cards?" I asked. "Well, if you get captured, you keep one, and you give the other one to the enemy. This is so you can be treated by the Geneva rules. They have to know your rank." My rank was a general. On the back of the card, it said, "General, US Army." So I got to be treated as a general. Now can you just imagine? You get captured and you tell the guys that are capturing you, who probably beat the shit out of you first, "Well, you have one and I keep one." I just folded right there, you know. It's absurd, but that's the way they did it, and having two cards came in handy later, and I'll tell you why in a minute.

So we get to the airport in Hawaii, and I'm supposed to be treated like a general would be treated. I say to my guys, "Don't you know how to get rid of this, because I can't do this? This is just silly." First of all, I wanted to pal around Hawaii with these guys a little bit. They knew Hawaii, they'd been to Hawaii lots of times. And the navy guy wanted to take us to the naval officers' club, which he says served the best mai-tais in the entire world. He's probably right. At least, I've never had one any better than that. So there are things we want to do, crazy things. We rented a car and we drove around the island where there is no complete road, brmm, brmm, brmm, along the beach and so on. We did lots of things. And so they got rid of them. I don't know what he said to them, but they drove away. They rolled up the carpet and put it back in the car and drove off. That was a great relief.

So we had a good time in Hawaii, and then we went on to Saigon, and we discovered through investigation, going into the various services, that each of the services had their own logistics report formats, and they had their own definitions of what the items were that were to appear there. And they were not all consistent; hence Johnson's getting crazy stuff. I remembered that I had an ARPA contract with SDC, System Development Corporation—a traditional air force contractor, that knew how to work with the military. There was a guy there named Dick Beeler whom I knew to be a very smart guy. He happened to be an operating system wizard also, but independently of that, he was really smart. Unfortunately, he weighed about 300 pounds, which I think is the reason he's probably no longer alive. But he was a really good guy, and so I said to my military friends, "I can fund a contract with SDC. We'll get Beeler out here and he'll fix this. We'll just let these guys know that we're going to come out and redefine a logistics reporting system that is uniform for all the services." My air force guy, a colonel, said,

"And we probably ought to put a computer here at Tân Sơn Nhất [air base outside Saigon]." There was no computer there. IBM was the only company in the States that could support a computer in Vietnam, so we ordered a 360/50, and it got installed later. We came back to the States and I got Beeler and SDC involved and so on, in the contract. Then Barry Wessler (who worked for me in ARPA, and who gave one of the talks at my retirement party, about our being in Vietnam) and I, made another three trips or so, over the course of the rest of the time I was in ARPA, to Vietnam, to get to see this thing through to its completion. And we saw the computer center being built and finished. McNamara was pleased and so on. But after the second trip that I made to Vietnam, I came back to ARPA, and I told Herzfeld, my boss, "We have no business being there. This is civil war between two factions, and the faction we're supporting at least—I'm not sure about the other faction—does not believe in meritocracy. The people who are running it, that government, are just relatives of other people." And I said, "This is not good." He just listened to what I had to say. I don't remember him taking a position one way or the other. Before I went out there, I thought, "Well, this is a good thing we're doing, because we have these people from the north coming down and killing these mayors of these small communities because they won't cooperate, and we ought to do something to protect that." So I had a complete turnaround. Anyway, I think that's most of my Vietnam story, except for what Barry Wessler can tell you, when you see the CD.

McJones: There was the last little bit in Thailand, right? [35]

Taylor: Oh, there's one more thing I forgot, why the two ID cards came in handy. Well, so I left ARPA in late 1969. Or was it 1970? No, it was 1969, because I spent a year in Utah before I went to PARC in 1970. And I went to the University of Utah. Now Utah is a dry state, and the University of Utah doesn't have bars, or at least, it didn't then. I don't know about today. This was in '69. But there was a really nice military officers' club, not far from the university. And if you could get in the officers' club, you could get drinks. When I left ARPA, I kept one of my ID cards. I gave the other one to the enemy. So when I got to Utah, I went into the officers' club and showed them my ID card, General, US Army, and I joined. They didn't have any generals as members. I was their first general, and they were happy to have me. They didn't say, "You look kind of young to be a general." They just said, "Welcome aboard." So I could take my friends there and we could have drinks. That helped my popularity some. We didn't go very often, but it was nice when we did. So that's how the card came in handy. I still have the card in my billfold today. And every now and then, when I'm at dinner with friends or something, someone will start talking about admiral this and general that, just for no reason. And I'll remember, and I'll say, "Well, I was a general." People will look at me and say, "Yes, sure, and the Pope's a Protestant, right?" I'll say, "Well, you want to bet?" No one's ever taken me up on the bet, but they call my bluff. So I show them a card, and they come away shaking their heads. Another piece of idiocy about how our system works.

McJones: It's been handy for you though.

Taylor: Yes, it has.

[35] In his August 1998 talk at the DEC Systems Research Center, Taylor continued the story of Beeler's work in south-east Asia. See also: Richard G. Beeler. The ARPA-RDC-T/MRDC Computer Laboratory. Final technical report, Accession number AD0744963, Systems Development Corporation, Santa Monica, California, June 1971. http://oai.dtic.mil/oai/oai?verb=getRecord&metadataPrefix=html&identifier=AD0744963

McJones: So, Bob, why don't we talk a little bit about management philosophy. I've heard a number of your statements down through the years. You seem to actually practice what you preach.

Taylor: How did I do as a manager, Paul?

McJones: Well, you were certainly the best manager I've ever had.

Taylor: Now, Paul, I'm not paying you to do this.

McJones: My current manager is also very good. I'd better get that onto the tape.

Taylor: Yes, in case he sees the tape, right?

McJones: He's a younger guy, so he still has room to catch up.

Taylor: I built two research labs, and was instrumental in a third, and so I do have notions, beliefs, about how to build and run a research center, at least in computer research. I don't know that my principles apply to all kinds of research, but yes, sure, we'll talk about that for a little bit. A lot of what I learned, before I got to PARC where I built my first research lab, of course like everything, things at the beginning come from your parents. And my parents were liberal minded folks. My dad was a Methodist minister, in small towns usually, when he was not a professor or a chaplain in the army. But that does not conjure up the kind of person he was. That conjures up sort of a pulpit pounding hellfire and damnation kind of guy. He was the antithesis of that image. He was quiet, he was scholarly. He had read Reinhold Niebuhr and Paul Tillich, who were instrumental theologians in his philosophy. He introduced southwest Texas Methodism to existentialism in the late 1940s, which is a reach. So he was a very inspirational guy. I'm not a religious guy, at least not in an organized religion sense today and wish that he were here for me to discuss these kinds of questions with him, but he's not. Then at school, and especially at college, at The University of Texas, I had some good influences on the importance of being full of intellectual curiosity, that sort of thing, suggesting good books and so on. Then I had some brief industrial experience, one at Martin Orlando, and one at ACF Electronics, so I could get a sense about how industry management and hierarchies worked. My experiences with management in those places were fine. They were nothing to comment on, nothing worth any comment. There was nothing wrong with them. Then I went to NASA and learned a different set of experiences having to do with how things are managed and hierarchies and so on. NASA was a lot like industry. And then at ARPA, I got a variety of management related experiences by visiting all these different sites, because they all had their own problems, their own ways of doing things, and so on.

I had this collection of experiences by the time I got to Xerox at the age of 38. I believed in flat organizations. Mind you, this applies to computer research, not necessarily to running a department store or whatever. I believed in an organization as flat as you can make it, partly because I believe it's important that everyone who's a researcher in that organization knows something about what everyone else in that organization is doing. In computer systems research, it's all teamwork. It's not so much individual performance, and it's really important then for everyone to know what everybody else is doing.

That means flat organization. That means at least weekly meetings of everybody. That means figuring out a way to stimulate people to talk with each other, even argue with each other. That means to figure out ways to teach people the difference between rejecting their argument, versus rejecting their personality. That's something I didn't always succeed at, especially with certain personalities, whom we both know. But it went pretty well. That all emphasizes communication.

The physical nature of the place where you're carrying out your work has a big impact on communication. It has to have lots of places where people can sit down and talk. The labs that I designed have outside walls with a lot of glass, and the inside walls also have a lot of glass, to bring light in, to make it a light, airy, comfortable place to be. Walled offices, with glass walls, nevertheless walled offices, are important for privacy, acoustical privacy, not so much visual privacy. I don't like cubicles and cubicle arrangements. On the other hand, for support people who have to respond to a lot of different people who might come to them, it's important that they be out and more available, like a cubicle arrangement, or an open area, that's okay. They don't require as much of the privacy as an individual researcher might. Their tasks are not as intellectually heavy as an individual researcher's task but it is important that they are accessible. So space is an important thing.

Another critical thing is recruiting. How do you go about getting the very best people? And then how do you go about rewarding them, keeping them? In recruiting them, in my case, I knew computer science was small enough then, so I could actually personally know a lot of the leading professors at a lot of the leading institutions, and therefore, I could find out who their most promising graduate students were. It's important to visit these places often, where many of our employees—new PhDs—were coming from. When I would visit these places, I might go and talk to a promising graduate student and ask him who the promising graduate students were. You might get a different answer than when the same question is put to a professor. Sometimes it might be a better answer, not necessarily but sometimes. So being assiduous about identifying people's qualities is important. Then when you identify someone that you think is worth interviewing, you invite them to come to an interview, and now you structure the interview in a particular way. You make sure that the interviewee gives a talk about something he or she believes in, or her work, previous or present, or her thesis, something that has some substance. This talk is presented before your entire group of researchers; it might last a couple of hours or so, an hour at least. After the talk, you have a schedule for the interviewee to go one on one with as many of your researchers as you can manage, that day and the next day. Each of those one on ones might last 30 minutes or an hour. Sometimes it's two on one, but not usually. At the end of the second day, the interviewee goes home and some time in that week, I think it's usually the next day, but not necessarily, I hold a meeting of all of the researchers, and I ask each of the researchers who wish, especially those who have had one on ones, to tell the rest of us about this person. Do we want to hire this person or not? If so, why, and if not, why? This is called a hiring meeting and it lasts for longer than most people who are in it want it to last. But at the end of it, I have a sense of the level of commitment from the people in this room, with regard to the success of this interviewee if we hire them. Because if someone comes out strongly in favor or a particular person, then the likelihood is that when that person comes to work here, the person who was in favor of them is going to be a supportive person. They don't want to look like they made a bad judgment. And because they genuinely think that the person is good, they believe in helping good people. They might even believe in helping bad people to a point, but they especially believe in helping good people. So this is the selection part of the recruiting process. And once you've invited someone to join you, why then you ask your researchers, those who have some influence with the interviewee, or some influence with the interviewee's friends, or some influence with the interviewee's faculty members, to try to influence the interviewee to come.

I found it to be a pretty effective way of identifying good people. Not just good people, but superior people. A really superior researcher is worth 10 or 20 really good researchers, I believe. And I think that's been demonstrated in front of me time and time and time again. I think a really good researcher wouldn't have lasted very long at either PARC or SRC for the reasons now I'm about to get to, and that is, keeping the people that you recruit. You've got to reward them appropriately. How do you do that? In our case, we asked every researcher once a year to write a one or two page description of the work that they did over the course of the year, usually broken into projects, but maybe the phases of projects, or something of the sort. And then they're asked to take this written description and give copies of it to everyone who worked with them, or anyone who they think might have some knowledge of what they did and how they did it. Then those people who receive this description of the person's work are asked to write evaluations of their own that mirror the particular description written by the person being evaluated. So at the end of this process, the writings have now been given to me and, I have in writing a singular piece of description about a person's work over the year and maybe a half a dozen or a dozen comments about this description. That gives you a pretty good sense of how well this person did over the past year. I then tried to, in some sense, rank order these. However, after you have about, I don't know, 15 employees, this gets to be a job that takes a long time. So I might delegate at this point to the most senior people in the lab, handing out to them some of these collections of writings, to do this sort of meta-evaluation. But then having done that, I'll sit down with the people to whom I delegated this stuff, and we'll have a discussion about the data. And then I'll look at how much money my boss gave me for raises, and I'll sort out the raise pile. If I've gotten enough from my boss, and we've done a good enough job from my boss's point of view, then I'll get sufficient raise money. On one occasion at PARC, my boss Bill Spencer told me "Well, you can't give this guy that kind of a raise." That's the first time and the only time that ever happened to me. That was a bad scene.

In addition to this way of evaluating, every few years, every two or three or four years—I think I did it irregularly—I would ask everyone in the lab to rank everyone else but themselves into one of four quartiles. And then I'd look at the results of this. This was all confidential. They make the quartile ranking on paper, hand it to me, nobody else. After I've done with them, I destroy them, shred them. But if I find a particular employee in several people's fourth quartile, then I'll try to find him/her another job. And I did that every time I did a quartile ranking. I would find someone, more than one sometimes, and try to find them other jobs. There are two good things from that. One is that people who stay together in a group are motivated by having other good people around them, and they're discouraged if they have to work with someone who isn't quite up to it. So, if you can get rid of people who are not so good, the spirit of the place is improved. You can see it just sort of pick up. The other good thing is it enables you to hire new people. If you don't do this, then after a while, you will arrive at a state where you have no more head count, so there's no way to improve your organization. Thus when you do hire someone, you try to hire them with a conscious effort to make your organization better. So all of this might—as we're spieling it off like this—might seem pretty cold, cruel and calculating. If it is, so be it. I think it's the way to run a good research center. These are principles of operation. I don't know if you saved any of your SRC email, but I wrote them down and I sent them to everybody at SRC at one point, called SRC operating principles.

McJones: I probably have it.

Taylor: You probably have it somewhere. And anyway, you know about it. I didn't tell you anything you didn't know. You've experienced all of this.

McJones: Both sides, all sides of it.

Taylor: Yes. So can you give me a critique? How did it--?

McJones: Well, it succeeded. The thing that made it really exciting and made me want to stay was because there were great people, because we had a sense of connection and excitement, that made us want to move forward.

Taylor: Yes, it's the people. It was the people that kept me at Xerox as long as I stayed there. So I understand that. Good people make you want to stay there.

<crew talk>

END OF TAPE 5

Taylor: Another management principle I forgot to mention was that you should look for opportunities to have celebrations every now and then, of one kind or another. Sometimes we had tea time, which is not really a celebration, but kind of a miniature party time, once a week, typically Friday afternoons, but we had the SRC Olympics, where I contrived various silly games for us to play. One of my favorites was, I found in a magazine one of these giant slingshots. You have one person over here holding once of these large rubber bands, the end of it, and another person over here holding another one, and these rubber bands come back into a thing where they join, and you fill a balloon with water, and a third person, or two, pull back this stretchy cord, and you let it go, and that balloon will fly maybe 75 yards, and land with a splash, and so, did you go to this particular Olympic?

McJones: I can't remember.

Taylor: I had teams, I had two teams at one end of this long field, and two teams at the other end, and each team was in a circle, and the object was for teams at one end to hit inside that circle at the other end of the field, and all the team members had to be inside those circles. So somebody, if you shoot accurately, was going to get wet. Well, that was a lot of fun.

McJones: Weren't the SRC Olympics often centered around an event for summer interns who were with us that summer, to give them a little fun?

Taylor: Yes, yes. Well, that was another management principle that's really, really important that I forgot to mention, that you've got to have good interns, and you've got to have a purposeful intern program. I mean, you go out and you seek interns, and you evaluate applications from interns before you accept them. But, yes, an aggressive intern program is really important. It helps you with recruiting later, it helps you spread the word when they go back to their campuses about what kind of place you are, and what kind of people you have, so it's crucial.

McJones: There were many hires from that group of people and they were a good group who went on to do other great things at other places.

Taylor: Right, yes. And in the course of all of this interviewing with you, Paul, the thing that disturbs me most, I think, is that there lots of lots of people who did really good stuff, whose names I have not mentioned, partly because there's so many of them. It's hard to think of all of them, but I'm going to try to think of some at the risk of offending others, and so I don't offend everybody. Well, we've mentioned a few like Mike Schroeder and Andrew Birrell, for example, who worked many years at PARC and many years at SRC, and are still working at Microsoft, and have done a lot of really high quality research. Roy Levin is another one of those people. Leslie Lamport is one of those people. I've mentioned Chuck Thacker already. We've mentioned the group that came from Berkeley in the early PARC days, who also worked, most of them, through PARC, SRC, and now they're elsewhere, some are at Microsoft. There are Sharon Perl and Greg Nelson, and Dick Sites, and Mike Burrows, and now I'm thinking of people who today are working for Google, but some of them worked at PARC, and all of them worked at SRC.

McJones: The Cambridge connection, speaking of Andrew Birrell and …

Taylor: Birrell, yes. The Cambridge University, yes. Roger Needham was a good friend of mine, and we asked him to come spend a month with us every summer, typically, and he did, and so we recruited a lot of people from Cambridge University, and Andrew Birrell and Mike Burrows were two of them. There were others.

McJones: Mark Hayter.

Taylor: Mark Hayter. Right. Let's see, and that reminds me of Dave Chaiken, Dave Redell. Who else was on Lectrice?

McJones: Jay Kistler.

Taylor: Jay Kistler. There's PARC people, too, that I haven't mentioned. Ed Taft managed our MAXC facility, which was a timeshared machine that was viewable from the outside world, over the ARPANET. The ARPANET had a lot of machines connected to it, and for a number of years, the MAXC machine that we had connected to the ARPANET, which Ed Taft looked after, was the most reliable machine on the ARPANET. Now, that's a lot of machines. Ed Taft was a very careful fellow, but he was also very creative and imaginative. I mean, he was the guy who built PUP, the PARC Universal Protocol, which was the packet system for our network. Let's see, who else have I...

Beckman: Willie-Sue.

Taylor: Who? Oh, Willie-Sue Haugeland. I don't know what happened to Willie-Sue, but she was at PARC for a number of years. Oh, I'm trying to think of the one, Warren Teitelman, who was a world-renowned LISP figure who was at PARC for a number of years.

McJones: In the graphics world, you mentioned Patrick Baudelaire.

Taylor: Patrick Baudelaire.

McJones: And William Newman.

Taylor: William Newman was in the Systems Lab. But I helped recruit him.

McJones: Okay. And he wrote a very interesting program called Markup for the Alto that was one of the earliest bitmap editors.

Taylor: Yes, that's right. He and Sproull wrote a graphics textbook, which is a classical textbook. Bob Sproull did a lot of things. He still is doing lots of things. Maybe if I think where the offices were. I know there's one office, and I can't remember the name of the chap who was in it, but he was really good. But, I mean, this was a long time ago.

McJones: Ed Satterthwaite came from Stanford.

Taylor: Thank you. Ed Satterthwaite. And who else was in that association?

Beckman: Bill Newman, you said.

Taylor: William Newman we got. We caught William Newman.

McJones: Was Dan Swinehart from Stanford?

Taylor: Yes. Yes. Anyway, I think I mentioned him earlier in some EARS connection.

McJones: Mark R. Brown—was he briefly at PARC?

Taylor: Oh, Mark H. Brown and Mark R. Brown.

McJones: Right.

Taylor: Two Mark Browns, and Mary Claire van Leunen.

McJones: Who really helped to get the report series started.

Taylor: Oh, and Cynthia Hibbard.

McJones: Right.

Taylor: Mary Claire van Leunen was special because she knew more about writing than any of the rest of us, and she could help people write more clearly, and Cynthia Hibbard, whom I think we found through Mary Claire, was the editor of our research report series, which Don Knuth said was the best industrial research report series that he'd ever seen. That's pretty good praise for our research reports.

McJones: Yeah, I was on the receiving end of Cynthia's advice a number of times and it was superb. She could just make better things come out the other end of that process.

Taylor: Then the two Mark Browns. Let's see. Both of them were at SRC, but I think only Mark R. Brown was at PARC.

McJones: I think Mark R. Brown was at PARC briefly early in his career and then was in the group that came across.

Taylor: That's right.

McJones: And then he became the director of...

Taylor: That's right. Mark R. Brown went from PARC to SRC and then to head the DEC Cambridge lab, which was another lab we had good connections with when I was at DEC. And now he is at Microsoft. He's a Vista wizard, did you know that?

McJones: Operating systems architect.

Taylor: And Mark H. Brown contributes to my tomato garden every year, still. He was interested in and good at a lot of graphical things, and that reminds me of...

Beckman: Larry Stewart.

Taylor: Yes, but that's not who I was thinking of. I'm trying to think of the chap who's now at Microsoft.

McJones: Mark Najork?

Taylor: Yes, Mark Najork, who Mark H. Brown discovered for us, and he's one of the only people we had from the University of Illinois.

McJones: And another thing that Mark H. Brown did was initiate the Algorithm Animation contest, which was another one of these regular summer events for the interns and all the employees.

Taylor: Yes, right. And he also ran some nation-wide contest I think for high school programmers. Something like that. But there's still lots of people that...

McJones: John DeTreville?

Taylor: Ah, yes. Well, I actually think of, go back to the office algorithm. John DeTreville is now at Yahoo, and one of the smartest people on the planet. If you were evaluating smartness amongst all these people, you'd be hard pressed to rank order them, because they were all really, really smart.

Beckman: Hal Murray.

Taylor: Who? Hal Murray. Oh, yes, and that reminds me of the only hardware maintainer I ever knew, Herb Yeary, who could maintain a piece of hardware with no documentation. We built a fair amount of hardware with no documentation, and when it broke, he could fix it. Chuck Thacker didn't quite understand how he could fix it, but he fixed it. Yes, Herb Yeary was rare talent. There wasn't any other Herb Yeary at SRC, and he was at PARC also. And at PARC there was Larry Clark, who built the Garage. The Garage is the place, an adjunct to PARC's computer science lab, which built hardware. And Larry Stewart, who was at both places.

Beckman: Yes.

Taylor: Yes, that's it. He certainly was at PARC, and he was at DEC and SRC as well, and then he went to Boston, and he's a chief technologist for some company in the Boston area, but I can't remember the name of the company. But all of these people who went through PARC and SRC are either no longer alive—there are not very many of those—or they're doing remarkable things wherever they are.

Beckman: Larry Tesler.

Taylor: Wasn't Larry Tesler in Alan Kay's group, or was that only later? He definitely was later, but was he earlier in CSL?

Beckman: He gave a lot of the demos when the board of directors came out.

Taylor: Yes, but you see, he and Tim Mott went off to Ginn & Company and built a text editor for them.

McJones: The Gypsy system?

Taylor: Gypsy.

McJones: As I understand it, Gypsy was one of the very first page layout systems.

Taylor: Right, right. I think that's right. Yes, but I don't think either one of them were at CSL. But maybe they were, but I don't...

McJones: Yes, they might have been in SSL.

Taylor: They might have been, yes, they might have been in Alan Kay's group.

McJones: They were clearly working closely with CSL people.

Taylor: Oh, yes.

McJones: If I understand it, Gypsy was using pieces of Bravo with a completely different user interface.

Taylor: That's right. It did.

Beckman: You mentioned Howard Sturgis.

Taylor: Yes. And of course there was Tom Malloy helping Charles Simonyi. Now there's, Charles Simonyi...

McJones: Almost deserves a lecture of its own.

Taylor: It does. Absolutely. Well, Bill Gates will tell you that Charles Simonyi built the product that took Microsoft over the top—Microsoft Word.

McJones: But he'd done Bravo with Butler at CSL.

Taylor: He did, yes. Charles Simonyi and Butler [Lampson] did Bravo; mostly Charles Simonyi, I think. Simonyi was the meta programmer who told Tom Malloy what code to write. You should ask Tom Malloy if that's true.

McJones: And then Tom Malloy would write the code. I was in a position similar to Tom Malloy back at Berkeley [in 1968]. Charles and I did this Snobol4 system together. It was before his metaprogramming idea, but I got to be in that position.

Taylor: Charles was a lot of fun. He still is a lot of fun.

McJones: Well, you seem to collect very interesting people.

Taylor: Charles came to my retirement party. Chuck Thacker was the MC, the moderator at my retirement party, and Charles gave a talk there, and when Chuck introduced Charles, he described this debugging suit that Charles used to wear according to Thacker, when he'd come to PARC and debug stuff, and I think Thacker said it was a suit with a see-through shirt, and when Simonyi got on stage at my retirement party to give his talk, he said, "Well, I had see-through pants, too, but I didn't wear them today, because it's Bob's day." <laughs> A crazy guy. Well, I wish I could think of more names, but when you get older, the thing that goes first are proper nouns. It's strange.

McJones: Such as names of people, places, things.

Taylor: Names. Yes. Names of people, places, and things. Especially people. I don't know why that is.

Beckman: Anna Karlin.

Taylor: Anna Karlin, back to theory. Yes, when you mentioned theory I should have remembered Anna Karlin. She's now, I think, at the University of Washington.

McJones: One other people that I thought of from outside SRC who came and visited —well this was at PARC—was Klaus Wirth.

Taylor: I think he visited us at SRC, too.

McJones: Very likely. But I think in the mid-70's he visited you at CSL and then he went back home to ETH and worked on his Lilith system.

Taylor: Well, he and Thacker have remained close friends over the years. In fact, Thacker just last week was at ETH giving a talk. Klaus is now retired. That reminds me of Hans Eberle, who we recruited from ETH. He was a Klaus Wirth student. He's in the Bay area, doing good things. What was the name of the guy who did Petal, who came from Berkeley?

McJones: Ed Lee.

Taylor: Ed Lee, yes. And let's see, going down that hallway. Brian Reid's sister.

McJones: Lucille.

Taylor: Lucille Glassman and her husband.

McJones: Steve Glassman.

Taylor: Steve. And then Mark Manasse. Let's see, Steve Glassman reminds me of a game I play on the computer, because he liked this game. And Manasse liked this game, too. I would talk with them about computer games.

Beckman: Did you say Mike Burrows?

Taylor: Mike Burrows I did, yes.

Taylor: Well, if somebody's looking at this tape, or reading this transcript, and your name is not here, it's just my stupidity, because if you were at either PARC CSL or at DEC SRC, you must be extraordinary, or you wouldn't have been there. And that's not just me saying it. Remember, you got there because there was a whole bunch of other people there who said they wanted you to be there, so it's just not one opinion. Okay, well, that's the best I can do to cover it.

McJones: Well, thank you, Bob.

Taylor: Thanks, Paul. Thanks, Ken. That's Paul McJones and Ken Beckman. Very nice of you to put up with all of this. It was Paul's idea. I could not have done it otherwise, and write if you get work.

END OF INTERVIEW

