An Interview with

WILLIAM F. MILLER

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Conducted by Pamela McCorduck

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Abstract

Miller reviews his early career, including his work on the Argonne National Laboratory computer and teaching at the University of Chicago Institute for Computer Research. He then focuses on George Forsythe and his role in establishing a computer science program at Stanford University. Miller joined the Stanford Linear Accelerator Center and the Stanford mathematics department in l964 and the computer science department at its formation in 1965. Miller contrasts what happened at Stanford with what happened at Harvard and the University of Pennsylvania, where other early computer science programs were started. Miller explains the relations of the computer science department to the computer center and the mathematics and electrical engineering departments, and how these relationships strengthened the university's computer science program. Miller also provides some details about the early funding of the department by the Atomic Energy Commission and the National Science Foundation.

WILLIAM F. MILLER INTERVIEW

DATE: 22 May 1979   INTERVIEWER: Pamela McCorduck

LOCATION: Stanford, CA

MCCORDUCK: This is a conversation with William F. Miller at Stanford on 22 May 1979 concerning the formation

of the Stanford Computer Science Department. Now, what was your initial interest in computers?

MILLER: Before coming to Stanford? I got introduced to computers while I was still a graduate student at Purdue

through my professor who was a professor in physics at Purdue who later went to Chicago. I followed him there. In

the meantime, I began to learn about computing from Al Perlis, who was still at Purdue at that time and I went to the

Argonne National Laboratory in the summer of 1953 to work on the development of the computer system called

AVADAC, or the Argonne's version of the Institute's digital automatic computer. They were also working on the

Oracle which was the Oak Ridge, something or other Logical Engine. And they were two Princeton type machines. I

wrote routines and kept a notebook on the development of the various parts of the major reactor program. Because I

had the notebook I became, in a sense, the operating system. People had to come to me to ask me, "Well how do you

put this together, how do you put that together?" Because I was a student and I had to work all night I would make

loaders and would load all these little pieces between the time when the more distinguished scientists quit work in

their evening and before they got back the next day. So I became kind of a little human operating system. That's how

I got involved in computing.

MCCORDUCK: What made you decide to come to Stanford?

MILLER: I had eventually, working at the Argonne National Laboratory, I became the director of the Applied

Mathematics Division which had the computing service responsibility as well as research and application

responsibility. I was also a professorial lecturer at the University of Chicago at their Institute for Computer Research.

 I became interested in educational programs but, as is often the case; it is more accidental that I came here than

planned. I was looking for a place to go on sabbatical leave and I had been approached to go to Weizmann Institute.

That would've been in the summer of 1965, or for the 1964-65 year. I happened to talk to George Forsythe about it, said I was thinking of going on sabbatical. He said, "Why don't you come to Stanford?" And that led to conversations which eventually led to their asking me if I wanted to take a position here.

MCCORDUCK: Did you come here temporarily first?

MILLER: No, those discussions went on at a more rapid pace. I came with a joint appointment between the faculty and the Stanford Linear Accelerator Center and the Computer Science faculty, which was then part of mathematics. But I came with the understanding that the department would be formed, which it was, in January, on 1 January 1965. I was a charter member.

MCCORDUCK: Yes, [unclear statement] what were some of the things that persuaded you to come to Stanford?

MILLER: I was, I had done what I was doing long enough and I thought it was time for a change, among other things. Secondly, I was able to combine my interests in physics instrumentation with interests in computer science and computer science development--that I enjoy teaching, that it was an opportunity to pursue a teaching program more vigorously than I had in the opportunity at Chicago. I think those were the principal reasons.

MCCORDUCK: Now, there was a certain amount of conflict about the formation of a department of computer science--why should this be a discipline unto itself, and so on. Do you recollect that?

MILLER: I wasn't involved in very much of that. I think George Forsythe fought most of that battle by the time I started talking with Stanford people, which is principally Fred Terman, George Forsythe, and Pete Panofsky. I think Terman was rather determined to start a department. I might have contributed a little bit of it by the emphasis on having a separate department. It had not yet occurred at the time of my discussion, those were going on in the spring of 1964. In fact, I came here in March to give a seminar, sort of an initiation of those discussions and I recall that I accepted an appointment in July of 1964, I recall because I broke my leg playing baseball that day--so you can

remember these things easily. But it was early in July, the idea that I would come in January which is when I did arrive. But, the department was planned pretty much by that time. I don't know what impetus I gave to that. I think George Forsythe and Fred Terman had pretty much settled it before then.

MCCORDUCK: Why was Frederick Terman so determined that there should be a department of computer science?

MILLER: I don't know. I don't know whether he had great vision or whether Forsythe bugged him a lot.

MCCORDUCK: In fact, Albert Bowker had commissioned a report from Louis Fein in 1957 where Fein recommended that a school of computing sciences be formed, the idea was not new to Stanford. The question...

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...but in any case Bowker was apparently very much committed to this idea.

MILLER: I think quite a number of people could see a good prospect for a computer science effort. Stanford and people like Bowker at Stanford probably understood that better than most because Bowker was involved in date analysis as a statistician, he had, then, a separate statistics department. He probably viewed computer science in a narrower sense than we view it now, or construe it now; nevertheless, I think he says the virtue of it having its own opportunities and its own appointments. But I wasn't a part of those earlier discussions; I knew of the Fein report because Forsythe told me about it; but I really did arrive at a time when the decision was already made.

MCCORDUCK: And that did have something to do with your decision to come here? That there would be an academic entity?

MILLER: Oh yes, that was an important part of the decision. At Chicago there existed an Institute for Computer Research which was purely a research institute with some teaching responsibilities but there were no students

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admitted to that department or that institute. They taught in other programs, in the school of business and applied mathematics programs. I viewed that as an obstacle to the development of a good student group. That was very important to me to be able to have graduate students, the work of graduate students; I developed a large cadre of graduate students when I was teaching.

MCCORDUCK: There still must have been some kind of identity crisis going on because you, in fact, got into the act at one point and wrote a paper called "What is Computer Science?"

MILLER: I did?

MCCORDUCK: You did. A rather long one.

MILLER: There probably was; I don't remember the paper. I'm sure I wrote several of those at different stages probably, some of them while I was still at Argonne--Do you happen to know the date of that?

MCCORDUCK: I'm sorry, I don't. It was a very prescient paper addressing...

MILLER: I, because I cannot recall the paper I don't remember the purpose, but it may have been for other institutions more than for Stanford at that stage.

MCCORDUCK: Was that part of Stanford's million by that time, to convince other institutions?

MILLER: No, but I had a lot of broad contacts by the time I came to Stanford. I operated on a lot of national advisory groups and I was frequently asked that question, "Why did Stanford start a department, what is computer science?" So I very likely either gave a talk on it to some group or felt compelled to, for efficiency reasons to write a paper on it.

MCCORDUCK: Were you asked with a sense of skepticism or a sense of awe?

MILLER: Some of each was present. Some people felt that, there was skepticism, in some areas. There was some healthy skepticism. There are two kinds of skepticism. Some people were thinking that computer science wasn't a discipline and never would be. Others felt that it was likely to become a discipline but one had to be careful about its development. It's a different kind of skepticism, that you want to make sure it develops with good problems, high quality of academic work, that's a different kind of skepticism and on which I think is healthy. Because it keeps people from getting into the more flaky parts of the activity.

MCCORDUCK: That brings to mind a related question that's kind of a puzzle in my mind, why is it that a small group of places, which I guess you could describe as a first with the most, like Harvard, the Moore School of Engineering, and places like that--Why didn't they take off when places like Stanford did?

MILLER: This is a problem which economists and technological development people have debated for years; there are several schools of thought. The organizational theorists believe in the conditions for success and the individual theorists or personality theorists believe in the leadership aspect of it. I suppose that there is an element of both present. I think it worked here because there was the right leadership. The desire, a group of people came together, wanted to do it, had an adequate vision, and put it in effect. Among other things, there was a strong recognition of what you had to do to make it work.

MCCORDUCK: For example...

MILLER: Well, you had to have a curriculum. We spent a lot of time on curriculum. It was a very sensible curriculum. You had to have good students and good problems. We spent a lot of time making sure our graduate students were well prepared. We did not develop an undergraduate major; we had curses for undergraduates, but we concentrated on graduates and research areas as far as our majors were concerned. I think that was a good move on our part.

MCCORDUCK: Why?

MILLER: It put us on the forefront of ideas. We. there is a time when you students are what you're known for, but early on, your ideas are what you're known for. They get out in front faster.

MCCORDUCK: If you were forming a computer science department today given the scores of good and middle departments that exist, is that the route you'd take?

MILLER: It would depend upon the institution. But, at an institution like Stanford, yes. Yes, that's a good route. I also think that students who come into computer science as graduate students need a broad background. I would, of a general nature, be inclined to fewer majors and broader majors rather than lots of majors. So, I wouldn't find it necessary to find a computer science major. I think our program here of a major in the mathematical sciences where students can take a concentration in mathematics or statistics or operations research or computer science but still get a broader major is adequate preparation for any of those subjects. I'd probably stick with that idea.

MCCORDUCK: You had two very big going concerns when you first came here, the SLAC Computation Project and being professor of computer science. Did you find yourself in conflict in any way?

MILLER: Well, there's a conflict of one's time, whatever you do. I was used to working very hard. I was used to going several things at the same time. In some ways the work at SLAC was easy. At SLAC in terms of the systems, the design of systems, I was really repeating things I had already done. I knew how to do what had to be done there, so it was more of an application. There were new things and the work at SLAC uncovered new problems, which is kind of nice, but by and large I could move in a implement ideas that I already had and they were quite adequate, the designs that I developed in the mid-sixties were followed for about ten years before new designs were called for. So that wasn't as heavy a burden as it might have been for somebody that didn't know about that kind of application. Then, I had a heavy load of graduate students, a lot of advising. At one time I had 13 graduate students doing

theses with me, some of them may have been shared with other people, I don't remember exactly. But that's too many. I decided that I cannot comfortably handle quite that large a number. Half a dozen is a better number. You give them better attention. But we had to do a lot of those things to get started.

MCCORDUCK: As I was looking through George Forsythe's papers, I noticed that there were two or three themes that kept recurring. One of them was: what is the proper role for the computation center with an academic computer science department? Do you remember some of the issues?

MILLER: Well, I remember those issues. The Center started as an adjunct to the department. When I arrived here, Forsythe was the director of the computation center and then when we became a separate department, he was chairman of the department. That debate was common in many places. There was a feeling that if the computing center became the play object of the department that it wouldn't serve the needs broadly of the institution. Generally, we came down on that side eventually and separated the computation center management form the management of the department.

MCCORDUCK: Though, for quite a while there was a duality in the sense that Ed Feigenbaum was, for example, director for a while.

MILLER; Yes, he was a professor in the department and director of the center and that, during that period, there was a very close relationship and those relationships drifted closer and further away depending on what's happening in each, the department or the center.

MCCORDUCK: It must have come to you at some point that this was not going to be just a good department, this was going to be a great department. Do you remember what point that was?

MILLER: Well, I had some sense of that, yes. There were a number of us who were being collected together who represented certain centers of excellence, if you like, Feigenbaum from Berkeley, McCarthy from MIT, Forsythe and

Herriot and Golub were from Stanford. I came from the program at the Argonne National Lab which was then one of the driving information science programs in the country, according to people like Allen Newell. I represented on complementary kind of interest. At the time we came together in early `65, we certainly hadn't arrived in the sense of our being the outstanding department. We had the opportunity, we had the momentum and we had good leadership in George Forsythe. One of my roles was to help hem get money, among other things, which I did. I helped him in some of the decision making from time to time; he tended to rely on it, because I had a great deal of experience, both in dealing with bureaucracies and in dealing with people. I had gathered some very good people at Argonne and I think I had reasonably good judgment of people talent and he and I worked on that together. I think the key in our development came when we had to make a decision about the promotion of Nicholas Wirth, who was then of the department, and Klaus was really very good and we all knew it . There was a lot of discussion about his promotion but at one point, in a faculty meeting, I said that the real issue we had to face was not whether Klaus was very good and good enough to have tenure here, but was, would his appointment be the best appointment we could possibly make if we had our choice of anybody in the country. So the department started thinking about that and in that general area, viewed Wirth and Don Knuth and Bob Floyd as the three most outstanding prospects. And generally it came back on feeling that either Floyd or Knuth would be our best bet at the time, but the faculty couldn't make up their mind between the two of the. Forsythe, walking our of the faculty meeting, said he didn't know what to do and I said "Let's get them both." And he said, "Well how can we do that?" And I said "If you can convince the faculty that we should get them both, I'll find the money." And the faculty was easy to convince, they thought that was a terrific idea, if we could get them both and I helped him get the money and we got them both. The only mistake we made was we ought to have kept Klaus Wirth too. `Cause Klaus was then, and has proven to be an outstanding computer scientist. And if we could have had all three of them we'd of had the market cornered. But, shortly thereafter, in one of the national committees or on some occasion, I don't remember which, a colleague told me that if he went to count the two best computer science departments in the country he'd have to count Stanford twice. What he was really saying was that if you divide up the department in two groups, each would stand alone as strong as about any other department in the country. And I think that we were fortunate to do that good recruiting. We didn't have a lot of extra baggage when we started the department. We started it with a dedicated and a sort of trim faculty. They were all productive, not a lot of baggage from being held over from some other department. And that was an

advantage.

MCCORDUCK: Do you think that it was an advantage that it grew up in the mathematics department and moved away from there as opposed to, say, electrical engineering a s Berkeley has, or MIT?

MILLER: Well, that's an interesting question, because I believe in that era it was an advantage. It may turn our, in the long run, that there are some advantages to being joined to an electrical engineering department. One of the major thrusts of computer science today is intimately connected with computer engineering through the development of integrated systems, software put into hardware. That may be easier to do, to foster the kind of cooperation between the theoreticians and the computer scientists and the engineers if it's all in one department. So only time will tell if in the short run we had an advantage. In the long run we may not. I don't know. At this stage I'm very optimistic because our electrical engineering department and our computer science department work together quite intimately so it really may make little difference. But there were some advantages to the rather mathematical orientation of the department when it started.

MCCORDUCK: And those were?

MILLER: I think we tended to focus on fairly rigorous problems that could be recognized as rigorous problems, we followed the paradigms of more rigorous disciplines and established it as a science as opposed to an engineering or applied discipline. I think that was a fortunate turn for us.

MCCORDUCK: I'd like to ask you a little bit more about your fund raising activity. Again, that was one of the other recurring themes ... Forsythe always seemed up against the wall as far as money was concerned and at one point in fact, just before you came, in 1964. He wrote a note to himself saying I'm going to quit. Not as a professor but as the head of the computation center because he felt that Stanford was simply underfunding him in a very dangerous way. What kinds of things did you do?

MILLER; I didn't do a lot of fund raising for the computation center, at least not until 1968 and later when I became associate provost for computing. But early on I principally concentrated on getting funding for faculty in the department. And I did that through various grants that I had and through my activities at SLAC. I was able to hire people in my group at SLAC who would either cooperate with or be a part of the departmental activities including visitors as well as permanent members. Then I had other grants and contracts which, on which I was the principal investigator and on which we could employ faculty. So it was principally through government grants and contracts, I didn't do much private fund-raising until much later. A lot of the private fund raising for the computation center was done by Ed Feigenbaum, and I helped, but I think he was the prime mover for it. Later, when I became provost, I did a great deal more private fund raising. I participated in the fund raising, part of which has gone into the building of the Margaret Jacks Hall and fund raising for the computing system there that is going to be an important system for the graduate students and the faculty in the department. That fund raising, the latter that I mention has come from private sources. That's something that I did later; early on it was mainly through government sources, grants, and contracts which had the breath and flexibility that we could put faculty and research associates and students on.

MCCORDUCK: Was this a function of the fact that things were a little bit easier in the sixties to justify, or were you simply able to be so persuasive? That's putting you on the spot, I'm sorry.

MILLER: Well, I like to separate people from their money. I have approached fund raising including funding for grants and contracts as an intellectual exercise. I believe it's an important intellectual exercise and I approach it vigorously and with real interest. Part of it is explaining to people what you are doing and part of it is showing what the prospects for future developments in certain areas are. It's very similar to teaching. I have always enjoyed that kind of articulation of purpose and prospects, which is important to fund raising. I don't know, maybe I'm persuasive, have found the right formula, I'm not sure what. I like to do it. That's the key.

MCCORDUCK: I was speaking to Albert Bowker yesterday about this and he said that he didn't think it could be done how as it had been done in the early sixties. He said there weren't enough resources to start up a new discipline and one as expensive as this.

MILLER: Well, the department was not so expensive. The computer center was very expensive, but the department wasn't so expensive. I think it is hard. We're going to see a challenge in trying to establish the Center for Integrated Systems. I don't consider it any more difficult, and yet it's as big a challenge as starting the department. We really hope to start a center which will involve integrated circuits, design automation, bringing computer science methodologies into the design of computers on a chip. The capital resources needed are very great, but we have already in this area a lot of grants and contracts. The pattern is not dissimilar to the one that we followed in starting the department. It may fail, but it's off to a pretty good start. If we succeed, I guess we'd have to say Bowker misjudged that one.

MCCORDUCK: Much of the evidence is that George Forsythe was simply a tireless lobbyist for computer science, who would go anywhere and speak to almost anybody on the topic. Was that your impression?

MILLER: Yes, he was certainly a missionary. He would talk to any group, students, alumni, administrators, other institutions. He had his thoughts clearly in mind. He dept changing them from time to time. He infected a large number of the rest of us with that same kind of missionary zeal. that was characteristic of the whole group we brought to Stanford, that was true for Feigenbaum, it was true for McCarthy, it was true for me before we came to Stanford. If you went back in the files of our institutions that preceded our coming here you'd see very much the same pattern.

MCCORDUCK: Do you think that was a sort of self selecting process? Or was that just the kind of person who would go into computers that early in the game?

MILLER: Oh, there are all sorts of people who went into computing. I think that the people to whom I'm referring were all people with strong academic training and strong academic goals and principles and I suspect there was a lot of self-justification going on there. We were explaining to ourselves why we were doing what we were doing. And I think that translates into a kind of missionary zeal.

MCCORDUCK: That's an interesting idea.

MILLER: I'm sure that's what I was doing. I left a very active research program in nuclear physics and some of the things I did are still used today by some people and I'm sure for me it was a certain amount of self-justification and I think that was true for a lot of people. The other part, I suppose, the practical part, that if you talk to a lot of people and you spread the word you'll find it easier to get support. And there's a certain political quality to it, but I think it was at least as much the former as the latter.

MCCORDUCK: Generally speaking, were government agencies open and interested, or did you have some trouble persuading them to put money into this?

MILLER: What was then the Atomic Energy Commission and now the Department of Energy, and the National Science Foundation both were quite active in supporting computing. At one time the AEC did far more than the NSF but eventually they, the more fundamental research, especially the research in the theoretical work, numerical methods, theory of algorithms, began to be most heavily supported by the National Science Foundation. The NSF also supported directly computing centers to stimulate applications in other disciplines. That was a far-reaching and aggressive program. I don't know who started that program; that was certainly a wise idea. Also the companies themselves were encouraging introduction of computing through discounts on equipment and various forms of support. IBM, Univac, Control Data, most major companies supported or gave discounts or some advantage to the universities in their selection to stimulate it. That had a huge impact on computing in this country. The program which I suppose was either started by Univac or IBM, or maybe both about the same time -- did what they intended to do, to introduce computing into universities more rapidly and more broadly than would have been the case without it. this meant students were turned out earlier with experience and interest in computing and would know what computing could do for their companies, the place they went to work, so there was a clientele out there for buying more computers. No question that that had a huge stimulus...

MCCORDUCK: This was the program where a given number of machines were given to schools...

MILLER: Or given at a reduced price, these discount programs change over time, but there were periods of time when they were quite substantial.

MCCORDUCK: On 4 January 1967 you wrote a memorandum to George Forsythe and you cited the continuous rebuffs from the university administration: "They are discouraging and they tend to force a pattern of short range planning." I guess this is before you were provost.

MILLER: Obviously it changed then!

MCCORDUCK: "We are soliciting on our own irrespective of the development office, which hasn't been very helpful." It isn't clear whether this was an implied threat that you were going to go to the development office and say "look, please leave us alone. We are going to get our own money" or whether you actually did go out and raise your own money.

MILLER: Too long ago for me to remember. I think that it was true then and probably continued to be true even later--I probably was guilty of some of that myself when I was provost--that the department was allowed to operate on its own devices a great deal. I did try a lot of private fund raising then and later. I found it harder than I would have expected in computer science and later sort of understood why.

MCCORDUCK: Why?

MILLER: Because fortunes in computer science were new. And you get contributions from old fortunes, not new fortunes. People can give you a piece of their paper empire but it doesn't help you very much, and it's just now happening that people who have made money in computing want to support computing. I was a little ahead of my time trying to get money for computing then. I was a little ahead of my time trying to get money for computing then.

There's another phenomenon which is generally true, incidentally; that engineers, people who make their money in engineering tend to give money to a lot of other causes than engineering. They sometimes give to philosophy, the humanities, but it never happens the other way around. You seldom see somebody who made a lot of money as a writer in the humanities giving money for engineering.

MCCORDUCK: Well there are a couple of reasons for that!

MILLER: But at any rate they don't. So part of what I ran into was the normal experience, that people tend to give for a lot of purposes; and secondly, there wasn't that much money available from people who had an interest in it. there was another factor I'm sure; since there was a lot of government support a lot of people thought "well, let the government do it." But I did begin a fund raising campaign which never yielded as much as I'd hoped but has led to some of the things I mentioned earlier.

MCCORDUCK: Very good.

MILLER: It took a long time to get the building. You see we began building plans about the time of that memo. It was more than 10 years later that we finally moved into that building. In fact, while I was provost, I had to come to the conclusion that we would not be able to raise enough money to build a separate building for computer science. We had not succeeded in raising enough. The private fund raising for the chemistry building was a risk enterprise and there was a lot of older money in chemistry. I came to the conclusion that we weren't going to be able to do it and that our best bet was to do a renovation of the Quad for which we could more reasonably expand some funds that we had in hand. We had to renovate the Quad anyway. Basically I'm the one who made the decision to, with a lot of consultation, to be sure to bring the computer science department into the Quad.

MCCORDUCK: It's an interesting political move because the computer science department now is in that wonderful pole position. They look straight down Palm Drive and psychologically it's...

MILLER: They're right in one of the key places in the Quad. But it was not so much for the particular location that I made the decision, it was the general idea that we would be better off if we would engage in renovation of those buildings because we didn't think we'd get the money for a separate building. I think everything that has happened since has borne out the fact that it would have been a bad risk to try to get a separate building. We have not yet raised all the money for the library. I think there is still some gap on the chemistry buildings. Yeah, it's a great location, it's a marvelous building.

MCCORDUCK: It really is. All right, thank you very much. Is there any question that I should have asked that I didn't have the wit to ask you?

MILLER: I don't think of anything in particular.

MCCORDUCK: Ok, fine. This has been a conversation with William F. Miller, 22 May 1979 at Stanford.

END OF INTERVIEW