An Interview with

FREDERICK WEINGARTEN

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Abstract

Weingarten describes his duties as a program officer in the Computer Science Division of the National Science Foundation (NSF). He details the proposal review process. He discusses the impact NSF has had on computer science through improved facilities, applications, education, and support of research. Weingarten concludes with a comparison between the research support philosophies of NSF and the Defense Advanced Research Projects Agency.

FREDERICK WEINGARTEN INTERVIEW

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ASPRAY: I would like to begin by asking you just a very few questions about your own personal history to set the context for this. Could you briefly account your career before you came to the Foundation originally?

WEINGARTEN: Actually, there wasn't much of it. I came to the Foundation pretty quickly. I was a bachelor's in engineering from Cal Tech and went up to Oregon State - did a master's, and Ph.D. actually in mathematics, although most of my work up there was in computing. Those were the days when you weren't too sure that computer science really meant anything, and I would have been the first computer scientist who had graduated from Oregon State. I didn't want to be the first, so the degree was in math, but I really worked on that. I took a post doc at Livermore where there were a lot of supercomputer, or at the time what were supercomputers, working for Sid Fernbach. As that post doc wound down to the end, Sid came to me and said, "How would you like to work in Washington?" Sid was on the advisory panel to the computer facilities program at NSF. I came out here and interviewed and got the job that spring, so I had only had about a year out of Oregon State when I went to the NSF.

ASPRAY: What were your duties to be here?

WEINGARTEN: It was interesting. They were in great transition at the time. Milt Rose was the head of the math section, I guess they called it at that time. But within the math section was this sort of burgeoning computer science. They were sort of supporting research out of some of the math programs, and they had the computing facilities program going. Well, NSF was reorganizing all that into something called the Office of Computing Activities. I was an assistant program director. I was officially to help with the computing facilities program, so I worked with Arthur Melman and Glenn Ingram on that program. But, more broadly, we were all moved up into this Office of Computing Activities and ended up doing whatever had to be done, and the computing facilities program was just one. And shortly after that, about a year later a computer science program was formed and I became the program director for computer science. So I like to say that I was the first computer science program director at NSF because of that,

although it dated back. But I still worked on the facilities program. We had a program called the Regional Centers Program that was designed to tie smaller schools into the big computers. We are doing the same thing these days with networking. It's the same thing I worked on that. It was an education-oriented set of programs developing computer applications in education and in research. It was fairly loosely structured. We tried to help whomever we could.

ASPRAY: Let me continue on quickly with your own career and then we will come back and investigate some of these more carefully. How long did you stay in those positions?

WEINGARTEN: Well, my first tour of duty was about two years and I left to go to Claremont, came back about two or three years later then for the longer stint when I ran Special Projects. That lasted about seven years. Actually, it wasn't called Special Projects at the time; it was called Computer Impacts on Society, and the management at NSF decided that that was just a little provocative title to have. This was the heyday of Proxmire and the Golden Fleece and all that sort of thing, and so it was... [laugh]

ASPRAY: After that you went to OTA, is that correct?

WEINGARTEN: After that I went first on detail, and then severed my ties with NSF and was on contract to OTA, and then became a full-time employee.

ASPRAY: All right. How difficult was it for the Foundation to attract program officers at the time that you originally joined them?

WEINGARTEN: Well, I don't know that I can talk generically. I know it was very attractive to me. Interestingly enough, I didn't know a lot about NSF. Particularly in my field NSF was not widely known. The support of computing was dominantly from ONR and DARPA. So I wasn't even too sure what NSF was when I came back here. I found over the year or two the first time there though that it was not very hard to get good scientists to be willing to

come and spend a year or two. There may be a little bit of golden era in my interpretation of that, but it seemed to me at the time, with people like Lee Hayworth at the top, that service at NSF was considered to be a call. That was like being asked to be an editor of a professional journal, a way of serving the discipline, and also was in fact a good learning experience to take back to the university. How it is now I don't know. I think maybe the tenure pressures have made it harder to attract young people these days, but the competition for jobs is much tighter. Federal salaries at that time were very competitive and attractive, so you weren't taking a substantial hit. So I think these days NSF faces in comparison a little more serious problem. Salaries are depressed; it's more bureaucratized, so in a year or two you can't make as much impact as you might have made in the past, and young people can't afford to get off the tenure track.

ASPRAY: Yes, what about people who aren't rotators, but are in for a long term as program officers?

WEINGARTEN: When I went there NSF wasn't all that old, so there weren't too many. I am trying to remember whether I met any there were a few old hands that had been around for many years, but by and large the programs were rotators or temporary appointments. In my second tour, back in the 1970s, there was in some quarters a sense of distinction that the rotating personnel were the real scientists and the real brains of the outfit, that the permanent people had settled in and become bureaucrats complacent. Towards the end of my seven years I began to really resent that, but that was the perception, and I think that was the perception in the outside world too. I know at the end of that seven years when I went to OTA I also sent my resume around to universities. I didn't think at that time it was unreasonable to think of going back into the discipline, and they were highly uninterested even though there was a reasonable shortage at the time. So I think that perception also had gone to the outside world.

ASPRAY: Before getting to the real substance of talking about the programs, I would like to learn a little bit more about the day-to-day life of the program officer and his interactions with various constituencies - how proactive you could be, what your contacts were with the potential grantees and with the director of computing and higher level people in the Foundation - that sort of thing. Can you just start telling me about that? I'll direct you if I have to do that.

WEINGARTEN: Sure [laugh]. Well, there was a very unique problem that we faced. Well, part of it was what I had mentioned before that NSF was not well-known in the outside world, that the research community tended to turn to DARPA and ONR. So you had two problems. One, to come along and say, "Well, we have some programs too. And you might find NSF more interesting to deal with in some ways, scientifically."

ASPRAY: What sorts of arguments would you give to that?

WEINGARTEN: Much greater flexibility than it was, that the grants would not be tied to defense priorities, to agency missions; that the NSF granting style was to make a grant based on general research capability in sort of general directions and then let the researcher go. There were people who weren't lucky enough to have access to these large pools of money that DARPA tended to spend. And if we wanted a computer science discipline that really stretched across the research universities in this country NSF was going to have to play a role to move it out of Carnegie-Mellon, Stanford, MIT the DARPA circle. So those were the kinds of arguments I had to make; it was very difficult. I remember a meeting once up at MIT where the various program directors from different agencies were sitting around describing their programs, and Licklider was there. And so I described my programs and where I thought we were going and what I wanted to do with it. And Licklider turned to me and said, "How much money do you have?"

ASPRAY: [laugh]

WEINGARTEN: And I don't even remember, but it was in the high hundreds of thousands, I think - certainly not a million, some hundred thousands. And I said that and he just laughed, and he said, "I spend my money at a million dollars at a time." [laugh]

ASPRAY: This was when Licklider was at DARPA, not when he was at MIT?

WEINGARTEN: Yes, although he was making... It's a little fuzzy, because he was making his transition back to MIT I

think at the time. And Bob...

ASPRAY: Fano?

WEINGARTEN: Fano, yes, was it? It's really got me fuzzy. But I will never forget how I felt and said... I mean, I was just this young program director [laugh] feeling so proud of my few hundred thousand dollars.

ASPRAY: Were you on the road to lots of different schools to?

WEINGARTEN: Yes, I travelled a lot to visit, and spent a lot of time trying to think of how we could spend our money to help shape that discipline, to help it form. So, for instance, we had a little departmental development series of grants where universities that wanted to start computer science departments could come to us and get some funds for faculty development, for graduate assistantships, and so on that weren't tied to specific research projects or to even having some faculty on them.

ASPRAY: Can you give me a couple examples of institutions that effectively used that?

WEINGARTEN: Well, yes, I will fudge, because I don't know the extent to which my money helped. I am trying to remember some of the grants. One was to Duke; one was to Cornell. Did we make one to Purdue? I can't recapture what universities I made those grants to. I must have made a dozen of them or so over a couple of years. And you will find that tradition still in the computing programs - not that it doesn't exist in others too, but there have always been programs in size that its predecessors... to help those departments grow and get established.

There were times when the problem was getting departmental computing facilities and experimental facilities, so computer science could get less theoretical. There have always been these departmental development programs in some form or another. So we had that. I was very concerned about trying to define areas of research. Computer scientists didn't even know what computer science was in those days, so you said, "Well, I have got a program in

computer science," you could get anything as a proposal. Well, if we want to focus that money, especially since Licklider says it's not much, you want to focus it to develop some fields. You had two choices; one was to put it all into automata theory, which was the only well-developed research field in which everybody had standards and they had publications, and they looked like scientists, because they were all mathematicians. They knew how to play the game, so they could have just swallowed up everything. Or you had to start working on the other areas, and that required proactive working: the holding of workshops, calling people and saying, "Let's hold a workshop on operating systems, on computer language design, on whatever." Bring some people together. Again, that's still a mode that's used at times at NSF. But now it's used, I think, to fill the gaps, to sort of explore the edges. At that time it was central to developing computer science. "What are the key research issues?" The kinds of problems a program officer has to understand. "How do I know whether it's good research or bad research? How do I expect them to communicate what they found to their fellow researchers?" That was a serious question in computer science, because they didn't tend to write papers. They tended to distribute programs as a result of their work.

ASPRAY: And what was your response to that?

WEINGARTEN: Well, there was a great debate. I don't think they ever really settled it out. In fact, the response probably wasn't terribly healthy. The response was to form very narrow, conservative, specialized research areas in which they could communicate. And so, as a result, computer science is now very balkanized. This flow between basic research and computer science and applications and other areas of computer science that used to enrich it doesn't take place because they had to form little pockets where, in fact, you could publish and you could start building theories and communicate - that it was necessary to establish it academically. A number of researchers would say, "Look, my program is my product. It's what I should get my thesis on." Winograd got his thesis on us - lots of people. Ivan Sutherland basically got his thesis on Sketchpad. In those days you would build a system.

Well, that troubled people because it didn't quite look like science, because how do you build on the shoulders of giants? [laugh] But I wouldn't say it's something that was ever resolved. It's something we were always wrestling with. But, on the other hand, if you would hold these workshops, word would get out that NSF is now ready to get

some proposals, or is looking for some proposals, in database design, operating systems, whatever. The community would start to send in some proposals.

ASPRAY: How did you judge quality of proposals? Did you rely heavily on advisory boards? Did you feel that you and the other program officers that educated yourselves by going around and talking to people... Just what were your mechanisms?

WEINGARTEN: Well, we always drew on the traditional NSF method - it was peer review. The trouble in computer science in a discipline like that is that peer review results in reviews all over the place. There was no consensus on these projects, but again, because the discipline itself doesn't have a well-established set of standards. Now, what we would do, at least what I would do and what I think my colleagues did, was not try to substitute our own judgement, because we didn't know a lot of what was going on, or at least feel that we knew enough about it to make our own independent judgments.

So what we would do is something in between, which was calibrate reviewers. The community was small enough that you could do that, and so you learned to trust the judgement of some people. They would say, "Well, that's good," you would tend to weigh more... So what you would end up with maybe four or five reviews - a few terrifics, a few crummies, and something in between, and you would calibrate on who said it and what their reasoning was. The text became very important. In fact, when NSF started to automate that review system in the 1970s, we fought that vehemently because we were afraid that NSF was going to move to a numerical grading rankings on proposal reviews, and we depended absolutely on the text. I didn't even care whether a box was checked, or what box was checked. In fact, the reviewers wouldn't check a box off. There were times when I would just sort of read the review and check a box that seemed to be what it was, but it was all the text. And the bureaucratic superstructure was very thin. And so we were given wide latitude. Milt and John Pasta and Curtis knew enough about the field too and calibrated us. So we operated that way. The panel was used more in after the fact. They would come in and calibrate how the decisions were going. We would not throw a stack of proposals to the panel and have them... In the computer science program. Now, the computer facilities program was run differently. That had a real panel review.

ASPRAY: What was the reason for that?

WEINGARTEN: I suspect because the investments were greater. Even now, as I think about it, if you have a rolling competition, just a program that's well-defined, takes in proposals at one end, gets them reviewed and makes grants out the other end, you don't need a panel system, except as an oversight, occasionally checking. If you have a one point competition where everybody sends in their proposals at one time, then a more efficient way of doing it and a fairer way of doing it is a panel review. Everybody reads every proposal, or some form of that. And there's a real competition. If you are spending a lot of money - I mean, these facility grants were quite a bit of money, especially at the time; some of them were up to a million bucks - then you want a more formal review process too, sending it out. So we had site visit teams and then panel reviews. Again, partly, I suppose, not just the size, but because there was an internal competition. We always sort of had the fiction that the regular research programs were not one-to-one competitions. It was just a rolling program when we ran out of money. We had a threshold.

ASPRAY: I've had people who were especially close to DARPA say to me about NSF that there was no direction at NSF in some sense, that they would sit back, wait for a set of proposals to come over the transom, grade those some way, and take the best of them, and that they weren't pushing research in this area or that area or another area, advancing it - that they weren't shaping the course of research. Do you want to respond to that?

WEINGARTEN: Well, I'd respond by saying that's not a criticism; that's just a description of NSF. That's a description of the way this country had decided by and large to relate to the research community. And if that be criticism, that's the way it goes. It's the simple peer review, unsolicited proposal.

We had a problem, though, in forming a new discipline in that environment, because we had to respect that tradition at NSF, but at the same time we had to do some stimulation. So the work we did, the fact that it might not have been very visible, might not have been terribly aggressive, but was instead a little more subtle and suggestive, calling people up and saying, "Do you know what we've got? We are looking for some proposals..." The workshop is one

example. I mean, you hold the workshop because, "We're going to define the research agenda in databases." Well, you hope to hell that what that means to the research community is, "Oh, NSF must be interested in funding someone," and, "Whatever this workshop decides are the principle directions of research, I'll bet NSF is probably is going to be more interested in funding, or the community will review those more favorably." That sort of steering takes place, but it is certainly not the ARPA style of going out and saying, "We want..."

And we thought, and I believed, too, that there are tremendous flaws in the way DARPA does it. And those flaws come down even now in the debate over a civilian DARPA, that when an agency operates with too free a hand to decide what research ought to be funded, tells the community what to do, and has lots and lots of bucks to put behind it, the dangers of going off in the wrong direction or of undue influence on what is basically a basic research community that's a great danger. DARPA has contributed a lot to computer science, but I think this country has needed both cultures and if we had to have one or the other I would not want DARPA. [laugh]

ASPRAY: I would think that that DARPA description of what goes on at NSF is not only slightly inaccurate in the way that you suggested in terms of your encouragement of workshops and such, but perhaps also in your prioritizing of grants after they have come in. I mean, I assume that many years you have had more good proposals than you had money for. What was the process then? I mean, you got your reviews back and you had some general sense of where things should be in high or medium or low category, but you had presumably some say in which ones of those on the borderline got promoted and which ones didn't - which ones got bumped up a category or down a category. Can you talk about that?

WEINGARTEN: We were not automatons operating by some formula. They could have hired some clerks and it wouldn't have been a very interesting job had it been that way. But where we got the sense, and really we tried very hard to follow this, was that our sense of the priorities came from the community. We travelled a lot; we went to conferences; we held our workshops; we had advisory panels; we were constantly in touch with the community. I can't think of a time when we exerted programmatic judgement on priorities, meaning putting a little more money in operating systems - a little more money here or there, that we didn't feel that we were reflecting what the community

wanted to do, or what the wise heads in the community thought was an appropriate way of doing it.

I distinguish that from the sort of the individual proposal by proposal, the case by case decision, because that was more personal judgement. "Well, this person is young; they don't quite know how to write a proposal. I think the reviewers were overreacting," or, "Here's a really neat idea and the reviewers are just being too hidebound. I'm going to go with it anyway."

Interestingly enough, when I did that I never got negative reactions back from the scientific community for doing that. Every once in a while somebody would write me and say, "Well, how come you turned me down when you gave so and so a grant?" But I sort of discount that. I mean, in general they didn't say, "What are you doing with your money?" I gave a grant to Charles Csuri at Ohio State, who was at the time an artist. He was in the Art Department. He had exhibited at the Corcoran, I think, but he wanted to explore the use of computers for art. I gave him a grant not because I wanted to see him do more art, but I wanted to see what somebody coming in from the outside making different kind of demands on the graphical capabilities of computers would do, and I thought he was capable of doing that. He has been a major shaping force now in computing graphics. But that grant was denounced by Congress, that I gave a grant to an artist in Ohio was brought up in some appropriations hearings.

ASPRAY: That grant was defended?

WEINGARTEN: I don't know whether it was defended or just ignored. NSF didn't particularly pay a lot of attention in those days to these day-to-day bitches and moans that came out of Congress. And similarly, Clough, he was at the Overland Conservatory. He was exploring how to use computers to make music. Well, there were a few people like that who were wondering about how to communicate with a computer - graphics, music, whatever, in and out. Their work has had enormous impact, now that we have enough computer power to focus a lot of attention on the interface. In those days it was considered crazy. So I mean I would occasionally take fliers like that. But even there he got good reviews. I did not have to step all over the review process to do it.

We also had the ability, though, to move very quickly on our own judgement on small grants. That was subsequently taken away from us in the 1970s and I understand it's back now. There is a little more program director discretion, but there was an increasing layer of bureaucracy.

ASPRAY: So if I understand the general thrust of your answer me that you had more control over the shape being in prioritizing the research disciplines by the initial allocation to this program or that program, rather than after the fact in a case by case decision basis. Is that a fair statement?

WEINGARTEN: I guess that's where managerially we decided to... Of course, and remember, in the earliest days we are talking about two eras; I have got them blended together here in the earliest days there was a computer science program.

ASPRAY: That was exactly the follow-up question I wanted to ask you. I mean, it seems like it's much more easy to do that in your second stint, because the program has been broken down into a few areas that are very well-defined. And did they have separate budgets for each one of these?

WEINGARTEN: Yes, they had separate budgets and separate program directors, but they had very fuzzy boundaries, because computer science was still not amenable to that kind of cutting up. And I was just talking to Rich DeMillo the other day, and he is thinking of shaking them up again, putting them in a bag and stirring them up, because he is not satisfied with the cutting up. It doesn't have these traditional [boundaries]. If you were designing from scratch the math division at NSF you would design it probably the way it is: just lay it out, because that's the way mathematics is. The computer sciences... I mean, any two computer scientists would probably divide it up completely differently.

ASPRAY: And it seems to change every few years.

WEINGARTEN: That's right, and these program directors loved it. This seems strange, but I rarely saw real turf

battles go on, because probably because there was so much to do. They didn't really spend a lot of time grabbing off this thing. Now, you have got to understand also, my second stint here, the world was divided up that way, but I had something called Special Projects, which was everything else [laugh].

ASPRAY: So you have an unusual perspective.

WEINGARTEN: Yes, I still had a fuzzy, general program with no boundaries.

ASPRAY: What about the relationships to some of the other divisions? To mathematics, to engineering - those two especially. Presumably there was some funding going on in those programs that had bearing on computing. What kind of working relationship did you have with the mathematics division?

WEINGARTEN: Not bad with mathematics, because, of course, Milt was head of mathematics there and I had a mathematics degree, and so we felt akin to it. I think mathematics was relieved to see the computing stuff get out of there, because that was always taking a chunk out of money that they thought ought to go to mathematics.

ASPRAY: Right, do they continue to do grants on numerical analysis and some mathematical programming. I don't know where that stood.

WEINGARTEN: Yes, I think it slopped. Anything that looked like mathematics we tried not to do, because our job was to build a new discipline. So the last thing we wanted to do was to try and grab off some of that; the last thing the mathematicians wanted to do was to cede the whole area of mathematics research over to us. It was in both sides' interests to keep those lines fairly tight, and like I say, Milt had very good relations. And for a while, in fact, through much of the 1970s the math and the computer science were in the same division - there was a math section and a computer science section. We were physically separated in the building. I think the math community got more and more edgy about that. I am reading not anything I know directly but things that were in the air, that they started to feel that maybe computer science was growing at the expense of math. When it came time to pick a new division

director, should it be a mathematician or a computer scientist? So that became a little more increasingly edgy. When they broke apart I think it was to the relief of both sides.

We, on the other hand, found great difficulties in being lumped in with mathematics. This was not so much the administrative structure at NSF but more broadly. At the time, for instance, there was an oversupply of mathematicians. The country was awash with mathematicians, and there was an undersupply of computer scientists. But NSF counted them all up in the same pot. So we were trying to argue that we needed to expand this discipline and the numbers showed that there was an oversupply. When we wanted to expand the number of doctoral fellowships, both cooperative and informal NSF fellowships, those were administered by the National Academy of Sciences. There were no computer scientists in the National Academy. It was handled by the Mathematical Sciences Board, or whatever they called themselves over at the Academy. And most of those grants - probably dozens and dozens of topologists and hardly any computer scientists, because for a very simple reason, that there were very good topologists, a lot of them walking around. And so, since the Academy was giving out those grants on quality and there were mathematicians of quality, they were not about to toss some money to a fledgling computer scientist, so we suffered a bit as the stepchild in mathematics. At the time, I think it's fair to say, most people considered themselves computer scientists... Is this true? It came out of the mathematics side of the house. I think that's probably true - certainly the ones we dealt with - Conti, Hartmannis. There were a lot of mathematicians. So there was a real affinity there.

ASPRAY: What about with engineering?

WEINGARTEN: Engineering is a different story. Engineers always thought that computer science was part of engineering. There was computer engineering that was a major discipline and you see that fought out. NSF in some ways just echoes what goes on in all the campus. And it was fought out, and you still see at Berkeley it's the Department of Computer Science and Engineering, at MIT; some places they have split, some they haven't; some they have both. The same thing at NSF, except that the relationships were soured further by what I think is still and always has been a schism, a psychological schism, between the engineers and NSF and the scientists. You know,

there's always been the proposals to have a national engineering foundation, and because engineering gets short shrift, and they always feel left out. So that resentment has always been there. I don't know what their reaction was when computer engineering was pulled out of engineering and put into size SIIS, but I can't imagine it was very friendly or very happy.

ASPRAY: Well, how did the problems manifest themselves in the time that you were there and working in the day-to-day program office?

WEINGARTEN: Not much. NSF is probably as balkanized as any.

TAPE 1/SIDE 2

WEINGARTEN: Now, I suspect they gave Milt Rose and John Pasta more heartburn than they ever gave me, because I imagine they were mainly, when they evinced themselves, at the managerial level - fights over budgets, over charters, that sort of thing. As a program officer I got my money each year and I spent more. [laugh] I learned how to play that game.

ASPRAY: Were there very many grants that were given jointly by the two?

WEINGARTEN: Not too many. There were a few. Al Schutzman in engineering was a friend. Actually, one of the few joint grants was one that got us in a little bit of hot water. Not that there was anything bad about it, but it was to somebody working on coding theory at Stanford who slipped off into encryption and that attracted the attention of the NSA and all kinds of hell... Well, that was a joint grant, so we would occasionally do some things jointly on an individual basis, but I think institutionally the engineers... And I have heard that the same kind of problems were between chemical engineering and chemistry, - certain areas of physics and chemical engineering.

ASPRAY: Was it regarded that engineering would take care of grants in coding theory, information theory in the

early years, data communications, maybe control theory as it relates to computing?

WEINGARTEN: Sure, because those were traditional engineering. All that signal processing stuff, and a lot of the optimization. Richard Beldman, I think, (dragging names out of the past) Beldman with his, what do they call it, the optimization theory was an engineer. But it's also a bit, I think, like mathematical physics or physical mathematics, that there are also people who work in both fields who are doing about the same thing, using different languages, going to different programs at NSF. And at a larger strategic level, I don't think the directors and assistant directors ever cared, because they thought that was the way science went. And I think they were right.

So they did not spend their days worrying a hell of a lot about program overlaps. The budget books were very sensitive, because Congress would worry, and OMB would worry about program overlaps. But from day to day occasionally you would be pushing through some grant and somebody else at the Foundation would hear about it. But that's my personal opinion - not very often.

ASPRAY: Tell me about the relationship of computing, or the place of computing, within the Foundation itself. How much attention did it get from the director? How did the traditional sciences feel about it?

WEINGARTEN: [laugh] This is going to sound paranoid. Computer scientists have always been in the position up till now of defending and fighting a discipline to senior management level that didn't grow up with them. So if you were a computer science department chairman you were dealing with deans, college presidents, even at technical universities that never used a computer in their research, often didn't have anything to do with them, and, "What on earth is this?" You know, "Every time I'd buy a gadget why do I have to start a new discipline to study it?"

The same thing at the Science Foundation. I think Lee Hayworth was very sensitive, very interested in forming OCA and so he would support it. But afterwards, people like Guy Stever... Guy Stever never believed that computer science even existed. He used to resent it. He would say, "Well, why don't you have car science? Why don't you have...?" You know, every time you would bring something up to him it was a constant battle, and the battle was

often on at the assistant director level, depending on the assistant director, who was in there, who was not in there. But it was often very difficult to define and defend the discipline within the structure.

And you still see it. I mean, it still exists, I think. I am told that despite the fact that he came out of IBM that SIIS continually had to fight with Erich Bloch about what the discipline of computer science was all about, and why you weren't supporting computer science if you gave a biologist some money to compute biology. That wasn't computer science, Erich.

And that's something that we have always felt other disciplines don't have to fight. They don't have to fight their legitimacy as a discipline. We have become a little tired of it. The way we have become tired of it is we are just not going to do it. We are here. We are big enough now. The country thinks we are important enough. We are going to stop fighting that; we are going to start sitting at the table like any other discipline, and if people doubt our legitimacy that's their problem, not ours.

But in the early days of NSF that was always a problem. And it was a problem that DARPA didn't have, you see, because DARPA, the generals knew that computers were very important. They weren't hung up about whether it was a basic discipline or not. If DARPA wanted to call it computer science, it could; it didn't matter. It was really important stuff. That's why, even today, 75 to 80% of federal support of computer science research is defense. And it's why computer science still has such a small part of the R&D budget, although it's such a driving technology in society. And again, those are directly echoed. My theory is still that NSF mirrors the economic community, because they are exactly the same fights that departments had to go through and are still going through.

ASPRAY: I noted that when OCA was set up, it was organizationally reporting directly to the director of the Foundation. Was there some reason for that?

WEINGARTEN: Yes, because the program was infrastructural. There were four thrusts. One was education; one was facilities; one was developing new applications in research (the CER program); and the fourth was to develop

this fledgling computer science. So in some sense, I viewed OCA as kind of a nest within which you could develop a basic research program - a friendlier nest than say mathematics might be. But basically that. But most of the money at OCA was really to be spent across disciplines and more institutionally.

Now, there's always problems with taking programs up and putting them directly under the director. It makes for a very unbalanced organization, so it wasn't very stable. I think when they broke it up again and relocated it was sort of inevitable, but as a way of nurturing it and protecting it - in fact, in some ways, protecting it from those battles that they had to go through with the tradition that I described earlier, with the traditional layers of the community. Putting it directly under the director was a good idea.

ASPRAY: What about direction from outside, from Congress or from the president? I know that, for example, Johnson, in one of his annual speeches, was very strongly supportive of education, science education; and computing was mentioned as one of the key elements. Did that have any effect on the appropriations for computing or the kind of political freedom to grow within the Foundation of the computing activities?

WEINGARTEN: Well, certainly, in some ways. I mean, the forming of OCA was always supposedly based on the Kemeny report and there was another report on computers and research. So these came... They cited OSTP, International Academy, the ? on them so clearly. And Kemeny was widely respected at the time. So in that sense, yes; this was the idea of developing a new thrust to take advantage of this technology. And that helped form OCA and it helped put money into it.

I think probably had computer science had to grow incrementally the way it was growing as a little piece in the math program, NSF probably never would have gotten it... It would have taken a lot longer. Things like that can have an impact. Lyndon Johnson is a special case, though, because there's a matter of giving and taking away. Lyndon Johnson, as the Vietnam war got more and more prominent on his radar, began to hate the academic community to the point where it wasn't clear to me that NSF was being treated at all well [laugh] in the White House.

On the other hand, there was another speech where he used the term "Networks for Knowledge." And nobody knew what he was talking about. Where did this word come from? You know, the usual things, to find out who put the words in his speech. He didn't do it, but somebody did. Couldn't find out; couldn't find out what they were thinking of. But there was this huge directive throughout the government, "Fill in the gaps because he said there was going to be an initiative on networks for knowledge."

And I think that was the start, a very gallumphing start, of what became NSFnet way back then, because for a long time there was this little program that wasn't allowed by OMB to ever do anything substantive. You know, "We don't want another DARPAnet. You guys can't..." But we did planning and always kept little things going. And I ended in that program in special projects. Still keep the little planning grants and economic studies. But the technology was catching up with that dream. And pretty soon Larry Landweber came in with Theorynet, and I made that first planning grant for Theorynet and then went to OTA. And that became the roots. So sometimes that may take ten or twenty years, but that really dates back the reason NSF was prepared to move in that direction was that original "Networks for Knowledge" speech that Lyndon Johnson made. I think that's what it was.

ASPRAY: Let me turn to a rather different set of questions. Using your categories of the four areas of facilities, applications, education, and research, let's talk about the impact, the programs that the Foundation had during your period of time there, and what you assess their impact would have been. Let's start with facilities, since that's the longest, most traditional of the programs, as far as I can tell.

WEINGARTEN: Yes, although it was closed down in the mid-1970s, early 1970s, sometime around there. It was extremely important, and I think you can see how important it was when you track university investments in computing after NSF stopped. They stopped. We went into a real desert and by the start of the 1980s U.S. research universities were terrible in the facilities they had for research computing. That's what provoked the Lax report not so much the everyday facilities. I mean, they had learned to accommodate that and to spend some money on departmental computing and for students and all that sort of thing. But in terms of the really high-end research hardware they had stopped investing. NSF closed it down basically because of OMB. But the argument was, "Well,

that program was necessary when computers were new, because universities had to somehow make room on their budgets for that new investment. Now that computers aren't new - everybody has them - now it's an incremental thing and universities can take care of it for themselves." That was made in total indifference to certain kinds of market and capital economics, I think, that universities faced, partly in due to OMB rules themselves.

ASPRAY: Did the Foundation fight this hard?

WEINGARTEN: I could never tell whether they gave in gracefully or whether there was a lot of fighting. At the time, when that finally happened I wasn't part of that program and was watching it from the side. I was distressed by it personally, and I know a couple other people there who were, but it may be that the compensating sign was that we weren't going to lose the money. The money was going to end up going into computer science. And so I think the leadership also saw the opportunity to get some more money into computer science and to get off of this buying computer hardware as the dominant mode.

ASPRAY: And indeed, was that true? Did essentially those funds get devoted?

WEINGARTEN: Yes, well, partly. [laugh] You know, they figure out their budgets are these big pools of water that people are always trying to find boundaries in. Yes, I think getting the facilities program was a very large drag. But what happened was the idea that NSF would stop helping make capital investments on computing facilities, and all the research programs would in turn start paying for computer time. And the universities would use that revenue stream. Well, they didn't do that. The money just sort of went into more research across the board, including computer science.

So I think that program was very important, and its importance can be seen in the fact that they are back at it again. They have the national supercomputer centers, which is another form of that. They have other kinds of departmental computing programs. So that fight is still going on.

ASPRAY: I assume that the reason that a facilities program was established in the first place is that computers were seen as tools to these other traditional scientific disciplines, and not for the development of computing as a discipline in its own.

WEINGARTEN: Right, that's what I mean about an infrastructure investment, which is why it made sense under the director, because it really served all. When we would go out on a site visit on a facilities' grant, we would take along with us on our site visit team biologists, chemists, physicists. We would try to determine where the strengths were and what we were going to hear about, but it was often a fascinating parade of applications to research really across the board.

ASPRAY: Were there facilities grants given towards the end of the facilities program for reasons of building up computer science, training of students in the computer courses, and that sort of thing?

WEINGARTEN: No. I think that would be really stretching it to argue that. There were occasionally grants that pushed the state of the art of computing and pushed into new applications and to the extent that that in turn flows back into computer science, feeds the problem sets and gives them experimentally, yes. And there were still universities where the computer center and the computer science department were linked. I think that's broken almost everywhere now. But there were times when that link was there, but I don't think it would be fair at all to categorize those as ever intended to help computer science. They did have a little facilities program they started up for computer science departments mainly to buy them VAXes.

ASPRAY: One of the things that seems to come out of the little documentary evidence that there is on this period is that nobody had a very good understanding of just how much of a need there was for computing equipment and how expensive this program was going to be. And the impression I get from this is that the need just swamped the program in some sense. Is that accurate?

WEINGARTEN: You mean the facilities program?

ASPRAY: The facilities program.

WEINGARTEN: No, I really don't think so. I mean, certainly, the boundaries started to expand. You know, when you start a program like that you really focus on the top 20. And after a while the top 20 sort of get cooking along, and then you start looking at the next 80 and then the next 100, and so on. And so in that sense, your definitions of what it is you are trying to accomplish start to fuzz and expand out, and your constituency starts to fuzz and expand out. So, sure, but that the demand only in the sense that the number of things we could have done, of course, was infinite. But the money was being spent well. It was leveraging a lot more money because of the way we spent that money. So if we spent $10 million on computing it probably leveraged another 20 out of the manufacturers, out of the universities, and so on.

ASPRAY: That was a requirement of the program.

WEINGARTEN: It was a requirement, that's right. But then there is more indirect leveraging, because if you give a grant like that to East Texas then the University of Texas goes to their trustees and says "Mumble." [laugh] That happened with the supercomputer centers. NSF paid five grants and what happened is, well, MIT, they go out and buy one and Cal Tech buys one and Ohio State. So that kind of indirect leveraging.

And that was certainly going on, or even in unsuccessful proposals a manufacturer would offer a deep discount as part of a proposal to NSF. The proposal would get declined but the manufacturer would somehow, "Let's see if we can't do some other fundraising." So it was a tremendous stimulative effect. There was never any thought nor any reason to expect that NSF should be just sort of picking up the computing bill for the U.S. academic community. But somehow, just being out there, making five, ten grants a year, major grants, to research universities was a way of saying to college presidents and trustees and so on, "This is an important investment; you guys want to keep up; you have got to do it. NSF thinks it's important enough that they're putting money into it." That was where I really thought the impact was. No, I wouldn't characterize it as swamped at all.

ASPRAY: You have suggested one way in which we could track the importance of this by looking at university expenditures after the program was ended as compared with during the time. Can you give me some other suggestions of how I might analyze the importance of this, either through case study or through some other global statistical ways.

WEINGARTEN: You mean, again, of the facilities program itself?

ASPRAY: The facilities program.

WEINGARTEN: I think it would be very expensive, difficult research to do it that way, because you would really want to track the investments in computing. Not only that but you would have to split up because I suspect university investments continued to grow. They didn't contract at all, but they are more responsive to internal university. So, like I said, there was more on education, more on the lowest common denominator of computing need across the university campus. One of the things that OMB didn't understand is that there is a distinction between national purpose scientifically and institutional purpose. It may be in our national purpose to see chemists to move as quickly as possible to a higher plane of chemistry research, if indeed using computers... some will argue it isn't... If in fact better chemistry is done on better computers there is a national purpose, even though an individual institution may not see that as its purpose.

And so, it's not so much overall university expenditures; it's university expenditures on scientific facilities, so you would have to be careful. Actually, I think they stop counting too. John Hanlen used to do these out of the Southern Regional Conference. They used to do these surveys of who had what computer and so on. He got tired; the money dried up, and the range of what it was he was trying to count expanded beyond the possibility of really [getting information]. You started asking universities, "What computers do you have?" and they would say, "We don't know." [laugh]

ASPRAY: Let me turn to a question in education. I don't know very much about computer education except that I know that over time the Foundation changed its institutional organization for education. It was part of subject-specific areas at one time and then at other times it was separated out so that there were education programs within the Foundation.

WEINGARTEN: By education you mean computer-based education - computers as educational instruments, or do you mean education about computing?

ASPRAY: Actually, I had both in mind. Maybe we should separate the two.

WEINGARTEN: Yes, they are quite different, because computer science education per se has always been a discipline within the main line of NSF. You know, we would occasionally fund a curriculum study or some little thing, but basically that was not... OCA was concerned with computers as an educational tool. The model that Kemeny had put forward at Dartmouth - a terminal in every kid's hands, the early research that had been done on CAI sort of indicated there was some potential there. That was more of what we were interested in than curriculum development, or education about computers, although some of that does come along. And I think when the program moved over into the education directorate, some of that other became more legitimized, so they had more projects on computer literacy and that sort of thing. But we thought the real action was going to be in computers as a tool. And our hopes far outstripped the capabilities of the technology at the time. So we funded Bitzer at Illinois...

ASPRAY: PLATO?

WEINGARTEN: Yes, the PLATO system; what's his name at Utah - the MITRE-based [project], and Pat Supeys, we gave him some money at Stanford. He was working on CAI to try and expand those applications.

ASPRAY: And did you view any of their work as really fulfilling the goals that you had set?

WEINGARTEN: Well, since we weren't DARPA, our job was to fund the research. I think we felt that there were a lot of interesting and very promising research results coming out. Some of Supeys' work with autis tic children, or language learning. It just seemed every time we funded something it was well-done, it would end up with a very positive correlation. What we couldn't really break through - and we probably couldn't have unless we were a civilian DARPA; well, even then the technology was not ready for it - was the economic barrier. You know, the how to get this stuff out in sufficient quantities to really make an impact on an institution as large as the U.S. school system.

And I think somehow the enormity of that was something we didn't have to worry about; we didn't worry about. We were funding research and exploration and in the back of our minds was confidence, "Well, if the technology doesn't hack it right now, either in terms of capability or price, ten years from now it will." And we were right. We were wrong in that we underestimated tremendously the resistance of the educational establishment to change and the amount of institutional change that that technology would be forcing in order to be really useful. Skinner used to talk about that as a result of his research on learning - that the schools would have to completely change what they do and how they operate, so he was kind of pessimistic about his research.

ASPRAY: I did see quite a few grants at the college level to set up programs for using the computer in chemistry education, especially, but also physics education and biology. How did you feel that those worked?

WEINGARTEN: Well, that was interesting. That fits somewhere in between what we were trying to do with Supeys, Bitzer, and those things and the facilities program, because it was a way of extending infrastructure to smaller schools, with the regional center programs. But clearly you wouldn't do it to strengthen research. [laugh] These were dominantly four-year colleges, with a strong research faculty. We have a little bit of that because we are NSF, but that was more infrastructure building, and it was intended to strengthen science education, again assuming that the computer kind of line of reasoning... Partly that if we were spending money on facilities and research institutions and trying to now move research onto this new mode, this new methodology research, the conclusion was that undergraduate science students had to start learning it, too. It's an argument that's used for moving instruments down into undergraduate level - deeded access. Secondly, that the computer itself could be an educational tool, and

it was a way of chewing away a little bit at this economic barrier that we saw about bringing the technology to bear in education.

So the Regional Center Program was in some sense partly a technological experiment. I mean, it might be the counter-example to what DARPA's criticism was. And the fact that in this case NSF said, "Well, let's see how one might link smaller satellite colleges into major university research centers technologically. What are the systems problems? What are the hardware, software problems." So that was a time when we were just moving in all kinds of directions. [laugh] And there were only four or five of us.

ASPRAY: We haven't really touched at all on applications, or promoting research in industry, or industry-academic interactions. Can you make a few comments along those lines?

WEINGARTEN: In the earliest days that was not a tremendous concern. I mean, we looked to the computer industry to help us by giving discounts and by working with the universities to get new facilities and new equipment installed, to help develop software, and so on. This sort of industrial policy idea that somehow we had to really link them together I don't recall as being terribly strong. The U.S. computer industry was the world's computer industry. There was no technological threat. In fact, what we had to fight off was the argument of why should we be funding academic research when IBM does all the research it wants. So we had to get letters from Gomerey [?] and other people at IBM saying, "Jeez, we can't do the basic research and NSF is really important," and so on. It was just not something that we worried a lot about. Maybe another reason is what Ken Flamm has talked about a bit, which is that there's always been this incredibly tight link. Just sort of that we just kind of take for granted between computer science and the computer industry, or in government programs, in the sense that in the technology flow there's not a technology transfer program that is demonstrable between federal computing research and industry - just the opposite. And so it's not something we ever viewed or I ever thought of as a serious problem. Now, you had another piece about the industry cooperation?

ASPRAY: Or industry-academic interaction. There weren't very many grants that I have seen directly to the large

companies - the Burroughs and the IBMs and such. There were a number of grants to places like SRI International and to some of the small think-tank type industrial firms.

WEINGARTEN: Like BBN or some of those?

ASPRAY: That's right.

WEINGARTEN: That's just because that's where the research talent was in a particular area - particularly in education. BBN had some good people in computer education. SRI had some people working on some interesting institutional. I tended to make some in my special projects program, because if I wanted to fund some work on, say, computer security, I wasn't going to find anybody in that traditional computer science department to fund that kind of research, and in the area of computer crime. Don Parker was at SRI. So some of these institutional impact questions were being studied more at the think tanks than there. So again, that was not an attempt to somehow bridge or bring together the two. It was just that was where some interesting work was being done.

ASPRAY: All right. Turning to research, this is the one that's sort of the hardest for me to get my hands on, because it's sort of amorphous to me. Maybe the question to ask you here is if you were going to pick out a few examples from the period of time where you were a program officer of either notable successes, notable failures, or somehow, especially characteristic funding, either to individuals or to institution, or even to narrowly-focused fields, what would you select?

WEINGARTEN: Oh, I don't know. Seriously, it's like a ship that crosses the Atlantic and you ran the tiller for a few hundred miles. I mentioned Chuck Csuri, which kind of tickles me, because that was always a grant that people sort of looked funny at, and it turns out he's got a company. He's done a lot of major research and whenever you see film clips of computer animation, Csuri's got this stuff. You can always tell his style type thing. A lot of work in computer graphics. I funded some basic work in the hidden line problem, and so on.

ASPRAY: Who worked some of the researchers?

WEINGARTEN: I can't even remember. I really can't. I guess in the very early days my grants, rather than being evaluated in terms of great discoveries, ought to be evaluated in terms of starting some careers. So again, you were saying, Hartmannis and Hopcroft at Cornell - people like that, theoreticians - were having troubles getting funding from DoD. It wasn't that I can go back and track great findings. In fact, it's DARPA that has the big, spectacular stuff - the Sketchpad, or the Winograd [?] system. You know, they can point to their things. Or Raj Reddy's language understander. We filled in gaps; we started careers, helped get programs or departments going. But I never really worried much about that and never went back that way.

It's funny; I have been out of computer science research for about a decade in the sense that I have been working for Congress trying to connect that. Now that I am back in the research community I am amazed at the number of people, both on my board and elsewhere in my community, who remember me, who say, "Oh, yes, you had given me my first research grant," and that really tickles me. And I think, "Well, okay, I did have an impact." But probably the most important impact was most likely institutionally at NSF - in getting a computer science program started, and starting to form it. I think it's far more important than any one individual's grant, because we did have to fight this view that, "Well, what is computer science?" And we worked like hell on that to convince the Congress, to convince OMB, to convince the NSF management (which we should not have had to do) that this was something that [needs to be] done [?]. So I think, as a traditional bureaucrat, I view my successes more in terms of the bureaucratic impact than the individual scientific impact that we might have had.

TAPE 2/SIDE 1

WEINGARTEN: My bigger impact, I think, was when I ran special projects, and that was not in computer science, so I don't know where that fits in your scope, but in the study of computers and society virtually everybody now in that field somehow got their start through my program or through some association with my program. When I went over to OTA they had started a project on the computer impacts. They had an advisory panel of all the top names, and

the project director at OTA had a panel meeting, brought me into it, and said, "Well, I would like you to meet some of these people." I walked in the room, and two-thirds of them were my grantees. So I said, "Well, I already know them." This sounds egotistical, but I think if you went into the field and talked to the key people there they would credit that program and what we were doing.

ASPRAY: Well, who are some of the key people you funded?

WEINGARTEN: Well, Don Parker in computer crime, Allen Rustin in privacy, Jim Rule in privacy, Sherry Turkle, who has written books, social psychologist, Bob Kling at Irvine. The list is longer [laugh], but a lot of them. The other reason I feel that the program had particular impact there was that those areas of research had no institutional legitimacy within the universities. Without an NSF imprimatur, without a research grant, I don't think they would have carried very well. They would have been forced to go off in a more traditional [research]. Rob Kling would be doing artificial intelligence. I think that was his original Ph.D.

ASPRAY: I didn't know that.

WEINGARTEN: He doesn't talk like it. He talks like an a social scientist, but I think he was an artificial intelligence guy. They would be off in traditional disciplines.

ASPRAY: One area that seems like an obvious candidate for special attention is theoretical computer science, because it doesn't seem to be an area that was of great concern to DARPA. ONR may have been willing to support some of the early work, but DARPA wasn't. Is that an accurate assessment?

WEINGARTEN: Yes, and NSF did have, I think, a major impact in theoretical. But in some sense, theoretical was almost too strong, because, as I said before, it had a disciplinary style. It could model itself after mathematics in its communications style and its whole intellectual structure of how it operated. And so, when it came to reviewing, when it came to writing a proposal and getting a grant and doing research in publishing and all that, I think the

theoreticians had a real advantage. Yes, it did get special attention, but NSF did not have to go out and beat the bushes to get them. There was a very strong theoretical program.

ASPRAY: I think, looking back over the grants lists, Arthur Burks was frequently getting grants for automata theory work and John McCarthy was getting grants on a regular basis for various things, some of which were theoretical.

WEINGARTEN: McCarthy got some. Yes, I guess he did get some money from NSF, although basically he was sort of on the DARPA trough.

ASPRAY: And you have mentioned Hartmannis. Who are some of the other people?

WEINGARTEN: I am trying to think. Did Michael Fisher get [a grant]? There were a couple people at Berkeley and Purdue. A numerical analysis in Maryland. It's so hard to dredge them up. There was a numerical analyst at Maryland who basically started their computational science center and ran the computer science program.

ASPRAY: Okay, I think of, in numerical analysis, looking over the grants, Young at Texas and Golob at Stanford.

WEINGARTEN: Yes, Gene Golob.

ASPRAY: And I can't remember anyone else off hand. I am sure there were plenty of others. Okay, other fields that you think were heavily represented by grants, even if you can't remember individual people. You have mentioned computer graphics.

WEINGARTEN: Now, this is the very early days?

ASPRAY: Well, either in the early days or in the 1970s period.

WEINGARTEN: Well, of course, 1970s I was not...

ASPRAY: You weren't directly involved then.

WEINGARTEN: Yes, I think in the 1970s, though, one of the most interesting growths to me was the growth back into computer architecture, because that was my original field of research at Oregon State. And it really dried up. They concluded that you simply couldn't do that kind of research on a university campus. It was too expensive. It just wasn't feasible. And that was just emphasized by the problem of ILLIAC IV and so on, sort of watching that DARPA money go down the drain.

So NSF really backed out of that completely, and then gradually over the 1970s moved back in, basically because as the microchip became more sophisticated your ability to get off-the-shelf components and hook them up more carefully, more easily, made it a little more feasible. So we moved more back into some hardware, and that, I think, was a significant trend, although it was always criticized that we would always stop short of anything that looked like realization. And that, indeed, was a problem.

Software engineering - now, it's hard to categorize that as any kind of success story, because... well, in fact, as a world we haven't cracked that nut yet. That was another area where we started trying to do some discipline building in the 1970s and formed a program and got some people going on it. In my side, I think some of the computer security research that I funded. Kent always thought that the important trend was to move towards what he called "experimental computer science," which was I think, again, a counter-reaction to the theory, to the preeminence of the earlier years of, in order to legitimize themselves, even on their own campuses as a discipline. And many of them were in the College of Arts and Sciences, at least abstract publications. Legitimize themselves, theory moved along well. But there was something a little dirty about fooling around with hardware and providing grants [?]. And so by somehow calling it or framing it as experimental computer science, where it's okay to write a program, let's do some of these things. And that was stimulated, not just by research grants, but by these departmental computing facility grants, that, one of the reasons computer scientists stopped playing around, besides the dominance of theory, was

that the computer centers worked hard for these machines. They weren't going to let these kids mess around with the operating systems and programming languages and stuff. They wouldn't let them anywhere near. So they had to have their own stand-alone systems with some substantial capacity so they could do something besides the toy problem. And the VAX came along and offered just exactly that platform.

ASPRAY: This was during your second...

WEINGARTEN: That was during the 1970s, yes, that that grew. You know, I am just tossing it around, but maybe that was one of the more important transformations.

ASPRAY: Along those same lines, people like Marvin Minsky have argued against this mathematical formal structure in computer science saying that there was this alternative track of experimental computer science, and he wanted to see it with his connection with artificial intelligence, monkeying around with machinery at that level. That brings to my mind this question about artificial intelligence research. I don't see any support for it in the NSF. Why is that?

WEINGARTEN: Because DARPA was putting millions. NSF always had a balance wheel.... Well, it worked both sides of the street. NSF had a balance wheel theory that there are areas that DARPA was not funding. And clearly, DARPA was just dumping piles of money into it. Secondly, the AI people would tell us, "Look, unless you're willing to dump that kind of money in, we don't want to fool around... There's no such thing as 'toy AI.'" As Minsky defines AI, which is this tinkering - big things building big systems - it's expensive research. They don't want to fool around with the individual researcher grants that NSF had. And finally, I think the traditional computer science community. To the extent that the peer review system tends to converge programs towards tradition, towards middle of the road, and it certainly does that in some ways when funds are tight. The traditional computer science community has never viewed AI with a wholly beneficent view. Now, if AI were a cheap little area to explore we probably could have done it anyway. But it wasn't cheap, which meant that the traditional forces on NSF would have prevented us, I think, from making any major... and there was no inclination on our part to do that. Clearly, DARPA was doing it, and I think in

terms of the nation's priorities that the overall allocation of funding was quite proper.

ASPRAY: When you think about the other funders, this balanced concept, did you think of anyone besides DARPA in the 1970s?

WEINGARTEN: Not really. DARPA really was the formative agency for computer science. I mean, they weren't the first. ONR probably was the first, but they really swamped with their money and with the centers that they set up.

ASPRAY: And in very narrow fields. NIH...

WEINGARTEN: NIH, certainly, yes. And there were computer scientists, very reputable computer scientists that we dealt with who were funded by NIH. NIH even did a little AI work.

ASPRAY: Feigenbaum.

WEINGARTEN: Yes, Feigenbaum, that's right. And Jerry Estrin had some stuff, not AI, but stuff at UCLA and so on. But that was sort of welcome, this additional input to the pot, but not viewed as something we had to balance as such.

ASPRAY: If you wanted to make a few general remarks about NSF's role in the advancement of computing, or themes that you would encourage us to concentrate on in our book, what would they be?

WEINGARTEN: Well, one is this constant frustration that a new discipline or an emerging discipline has trying to make its way in a traditional scientific community. It has been off and on a painful process at NSF, I think, and at times an unnecessarily painful process. And I guess my biggest frustration, even so, and it's not just because of where I sit now, is to see the broader science priorities the way they are. Not that somehow we should stop doing biological research or high energy physics or anything, but it is just totally out of whack. I think there has been a

failure of broader leadership in the science policy community to sort of bring computer science along as one of the major contributors. And what happens, you see, then they turn around and complain, because what happens when they don't exercise that kind of stewardship is that the political system looks and says, "Well, God, computers are really important to our economy and all." And then they try to impose industrial policy kinds of programs on top, and the traditional scientific community gets edgy about that. They don't like that political interference from a Congress or a White House that's responding to sort of their view of the economy and industrial needs, and their minority position that the discipline has had.

I think another area that's fascinating, but I think you are digging into it is sort of this funny, love-hate relationship between NSF and DARPA. You are absolutely right; we looked at DARPA. NIH, well, they're good friends. And it was yin and yang. I mean, their [DARPA's] operating style is different, the kinds of grants they had. You know, they were the big guys on the block at just a few universities. We were the defenders of the small grants and the institutional infrastructure building operating with a lot less money. And we envied them. We thought they ought to have more feedback from the scientific community than what they did, and we thought they operated very haphandedly and so on. But we certainly envied the money they could bring to bear. I think that's leaving a legacy that could be kind of difficult to shake, particularly if DoD funding starts dropping precipitously. That depends, I guess, on whether current events are a perturbation on a longer track or not. Computer science may regret having its dominant funding coming from DoD.

There's just those two things that make it kind of interesting. I mean, it has been forming. It's a discipline that formed in my lifetime, which has been interesting to watch.

ASPRAY: Okay, well, thank you.

WEINGARTEN: Okay, sure.

END OF INTERVIEW