

An Interview with

VAL TARESKI

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Abstract

Tareski briefly outlines his education and recounts his decision to go to the National Science Foundation before discussing his work in the theoretical computer science program. In this context he describes the artificial intelligence orientation of the military agencies that funded computer science research and details the duties of a program officer. Tareski describes at length the proposal review process and the role of NSF in developing theoretical computer science. He concludes with some thoughts on Kent Curtis' management style.

VAL TARESKI INTERVIEW

DATE: 15 November 1990

INTERVIEWER: William Aspray

LOCATION: Rochester, MN

ASPRAY: Could we begin by having you recount quite briefly your career, starting with your education up until the time you came to the Foundation, as background?

TARESKI: As the case of many people that got into the computing area in that time frame, my undergraduate and graduate education was in engineering, or typical for others was physics and chemistry - the other two key areas that tended to have people feeding into the computer area. So I had an undergraduate and a master's degree from North Dakota State University in electrical engineering. It was at the Master's degree level that I first got interested in computing, and that inspired me, after teaching a couple years in electrical engineering, to go on to a Ph.D. program at the University of Illinois, and one of the, at that time, newest programs in the country in computer science. I spent four years at the University of Illinois working on my doctoral degree and left quite a number of people, including who was to become the eventual head of Office of Computing Activities, Dr. John Pasta, who was then head of the Department of Computer Science at the University of Illinois. He left about a year before I was finishing my degree to join and become the head of the Office of Computing Activities at the NSF. When I was getting to wind down he gave me a call and asked me if it would be possible I might be interested in spending a year on a rotator basis at the NSF in the Office of Computing Activities. I thought it was an interesting activity, so in fact, instead of going back to education at that point, I did take that opportunity to go to the National Science Foundation.

ASPRAY: What was it that you found so attractive about the place?

TARESKI: The opportunity to get a much broader perspective on research going on. I had, of course, completed my Ph.D. work, but a very narrow [perspective], as is typical for a Ph.D. degree program, and talked to all my colleagues who were in a singularly narrow approach. You got real good at knowing about your particularly narrow discipline, but knowing that I wanted to go back into education, knowing the importance of having a broader perspective, I felt

the opportunity to be at the Science Foundation where you were forced to be broader and more aware of the current activities would be an excellent opportunity to enable me to be a better teacher.

ASPRAY: Would you care to speculate about why you think they found you an attractive candidate for the post?

TARESKEI: That's an interesting question. I don't know if I can really answer that. I know that it was always a challenge. After I got there I found it very difficult to get rotators, although part of the policy at the NSF at that time, I think is still probably partially true, is that they had a core permanent staff - a professional staff - and then they had a certain percentage of the people that they wanted to come back from the field, work a year or so at the Foundation, and return back to your normal assignments. Well, that was a nice idea in theory, but in practice it's difficult for an active researcher to just drop what he is doing or put in some state of suspension and go to the Science Foundation and spend a year or so there and then return, and try to pick up where he had left off - maybe in the middle of a research contract - whatever. So in my case, just finishing up a degree program, it was a little bit more opportune to make the transition to a position of this nature than it would be if after I was already involved in a research program. So that's part of it, I think, was just the fact that I was near completion of a degree made it easier to be available to take a tour of duty, if you will, at the National Science Foundation on a rotator basis.

ASPRAY: What were your responsibilities to be?

TARESKEI: They had just formed, or had for a year or two, a program in theoretical computer sciences. And the program at the University of Illinois that I was working under was very heavily oriented toward the theory side, although there was a very heavy orientation in another group, which I wasn't involved with. I was supposed to pick up and try to encourage and develop funding out of theoretical computer sciences.

ASPRAY: And what state did you find that program in when you arrived?

TARESKEI: Well, it had been run on an ad-hoc basis. There hadn't been a program director for several months, I believe. I don't remember the exact timing now, but it had been a vacant position, and so somebody else had been

taking that on as an overload to their regular program to try to keep this program running. So it was not strongly supported nor given as much attention as of course it really did need. And so it gave me plenty of opportunity to jump in and try to do some very positive things for the theoretical computer science research support in the United States.

ASPRAY: Who had been the program officer for that area before?

TARESKI: I don't recall who it was. The acting person was the section head, Kent Curtis, was the only thing I remember at the moment.

ASPRAY: What kinds of research areas had been supported in theoretical computer science up until that time?

TARESKI: Some of the traditional theoretical foundations of computer science were the primary things. There was a little bit of artificial intelligence type of thing that had gotten some research support. There was a smattering of miscellaneous things, but primarily the abstract computer science type of things seemed to be the predominant thing that had been supported and tended to be what was continued to be supported after I took over, except I did it more aggressively.

ASPRAY: Automata theory and complexity theory?

TARESKI: Although automata theory was then starting to fade at that point and complexity theory was the one that was getting the lion's share of attention and I expanded that some more because it appeared that that was a significant gold mine that needed further work.

ASPRAY: Why don't you tell me about your strategy for building up theoretical computer science? What did you look to to support; what did you support? I don't know how proactive you were.

TARESKEI: Well, you are, of course, restricted in some sense in proactivity by the standard rules of the National Science Foundation, but I started an active travel program to attend conferences as well as to do site visits. Many of the people who were supported on the program hadn't had a visit from the program officer for some time. And so first I wanted to get a feeling for the kinds of things that we were supporting by travel, plus going to conferences to encourage people who had never thought of the NSF as a supporter of the computer sciences because we were a relatively new entrant into the area and our office was somewhat nonvisible compared to the more traditional funding agencies, to encourage and to cultivate more proposals. In fact, the numbers proposals that came in after about a year and a half that I had been there started increasing (theory areas) significantly as a result of that cultivation.

ASPRAY: Who had been supporting theoretical studies before this time?

TARESKEI: It was some of them in the Office of Scientific Research in the Air Force; the Navy - ONR had done quite a bit in certain thrusts; DARPA.

ASPRAY: Could you elaborate on that for me?

TARESKEI: Well, the military agencies basically were the ones supporting various trust areas that they viewed as possible attachments or connections into military goals.

ASPRAY: What were examples of some of those areas?

TARESKEI: Well, DARPA was at that time still in their big trust for AI support, for example. So the artificial intelligence community was getting very significant amounts of support from targeting activities. Speech recognition was one that I recall off the top of my head. There was some stuff at Stanford too. I don't recall exactly what it was, but it was very well supported for those targeted areas from DARPA. I don't recall any longer, but I know ONR had had some targeted areas that they were looking at. They weren't so AI-oriented, but were things that they thought would have some payoffs for the Navy and similarly for the Air Force. I don't recall a particular target. The Army

was a little bit, but much, much less. The Army was always a poor stepsister of the military support areas. And that was pretty much it. There was no other funding support of any significance from any other government agency for theoretical computer science. There were a lot of applied computer sciences type stuff - software and hardware, but almost nothing else from the support for the theory areas.

ASPRAY: AI from NIH?

TARESKEI: Only if it was medically related. Again, very application-oriented. Yes, I did interface with the NIH people, now that you mention it. I don't recall any of the players any longer, but I did have some limited access to the biological-related activities. That's when I first got acquainted with the idea of biological cells... the information cells.

ASPRAY: Yes, because I also remember that Ed Feigenbaum was getting a lot of his support through NIH for the AI work.

TARESKEI: That's right, I remember that now.

ASPRAY: Can you give me some examples of some of the main institutions that you visited in your site visits? You said that some places hadn't been visited for some considerable time.

TARESKEI: I am trying to think of some of the tours I made. It varied, but the west coast, because of the time delay and the long travel time, was one of the areas in the theoretical area hadn't been visited much. So I went out to Washington State University, University of Washington campuses; California - Berkeley, the University of Southern Cal, San Diego; the University of Texas I visited a couple of occasions. I don't remember any others off the top of my head right now, but I know I did have to play quite a bit west coast, or west of the Mississippi type thing, because that was the farthest and hardest to travel to, and travel budgets had restricted. So I tended to try to do big tours so I could economize the cost of travel by hopping, somewhat similar to what we do today to save money and try to make a two-week tour, but go from point to point and try to make more bang for the buck when travelling to those

further sites away from the east coast. In fact, I did very little travel, interestingly enough, to the east coast sites, except by car to some of the local institutions, which did have some interesting research going on that was partially funded by the National Science Foundation.

ASPRAY: So you didn't go to places like Cornell, for example, which I think of as a theory center?

TARESKI: It seems like I have been on the Cornell campus, and that must have been the time period, because I haven't been there since. So I probably was there somewhere along the line. It's somewhat fuzzy, because some of it was there for official site visits; other times I ended up at these institutions as a result of attending conferences that were emphasizing areas of interest. But sometimes the conference type thing you were focusing on the conference and so you didn't quite as familiar and into the campus as you did when you were doing a strict campus visit.

ASPRAY: If I remember correctly the profession structure today there are two major theory conferences and groups that go on. Was that the case at the time?

TARESKI: Well, they were in the formative stages about that time. The stronger one at the time, and definitely the emerging one, was the one sponsored by the ACM-SIG area. And that was the one that was the healthiest, if you will, and was drawing very strongly; it had some very good leadership at the time. And toward the end of the three-year period that I was there then the IEEE-sponsored Foundations of Computing started getting some strength and then became a significant player as well.

ASPRAY: Yes, did NSF play a role in getting these conferences going, or supporting them in any regular sense?

TARESKI: Not during the time that I was there - only in the sense that I tried to always attend and be very visible and to blow the horn for NSF funding.

ASPRAY: And did you feel that it was your role to, or you were able to push certain areas of support within

theoretical computer science?

TARESKI: Well, it was an interesting process, which I had to learn, of course, having not been on the giving-out money side and only been on the taking-in at that prior time period. At that time (I assume they still do this) there were advisory panels for the office and we had advisory panels at two levels. We had one level just for our section advising the computer science per se, and then there was a global for the whole office - the larger overview of computing in general. At that time they were still doing some funding of just computing resources, although it was being phased out as I got into the office. And so we had had these panels come in once a year - these advisory groups - and basically picked their brains, and get them to, as a group, somewhat agree on those areas that warranted more research support and those areas which perhaps were lesser important for the near-term development of the computing sciences and computing. And so, using that foundation, then the section would discuss priorities and then each of the program directors would key on that and try to use that as guidelines as to how they would answer the telephone. I mean, the telephone rings constantly saying, "Well, I have got a research project x. I am doing such and such. Is that something of some interest or strong interest to the NSF?" And so we would use these panel discussion periods as a basis to being able to answer those questions and even encourage or discourage particular applicants from applying for NSF support. I didn't personally, I guess, when I first got there have any particular axes to grind. As I participated in those panels and travelled to conferences then I did start forming some opinions based on what I heard and saw and perhaps did skew then, if you will, funding based on what appeared - for example, theory of computing, which seemed to be a hot area that I didn't appreciate fully when I first joined the Science Foundation. And I did certainly move a lot of support in that direction as I started understanding just what kind of impact that theoretical work could have on computing understanding.

ASPRAY: Did you have other areas under your control at the time in addition to theory?

TARESKI: Within theory at that time we were still supporting AI, but we called it intelligent systems, I guess. That was, if you will, a much smaller part of the budget that I was dealing with. Other than that, though, that was primarily it. Anything that was pure software or more purely hardware went under a different program.

ASPRAY: Which of the sections did you reside in?

TARESKI: It was computer science and engineering, as it was called at that time.

ASPRAY: So this was Kent Curtis' section?

TARESKI: Kent Curtis' section, right.

ASPRAY: What was the attitude towards AI at the Foundation?

TARESKI: That's a hard one to answer. It cut across disciplines besides some support in the Office of Computing Activities, or at least what was the Office of Computing Activities when I started there. It was changed while I was there and became a division of computer research, if I remember correctly. There was also some support within the regular engineering programs. I don't recall the titles, I believe it was Schulman was the name of the program director in engineering that I collaborated with on a number of occasions when there were proposals that I wasn't sure were "straight CS" but had some engineering flavor in. In fact, I think we even jointly funded some projects between the two programs as a result of that joint activity. But it was a small endeavor because at that time DARPA and other military agencies were pumping so much money in it. The only thing we were doing, in effect, was covering those kinds of things that didn't have what appeared to be direct military applications in the near future.

ASPRAY: Aren't most of the AI studies fairly capital-intensive thought?

TARESKI: At that time a lot of work was developing working systems, which, of course, then represent a lot of hardware, and a lot of expense. The things that the NSF funded tended to be the non-hardware, the more theoretical studies, trying to, "Let's get a handle on AI and let's understand it theoretically," which wasn't being done much at all by the military funding agencies.

ASPRAY: Can you think of some examples of people you funded in this area?

TARESKI: I am sure I have some old notes that if I dug through them I could regenerate them, but at the moment I don't recall.

ASPRAY: I was going to ask that question more generally about support in theory, though. If we wanted to take the theory program from your area and look at people's work or institution's work that you thought of as successes during that period that you had supported, who would you point us to?

TARESKI: Berkeley was a very significant player as an institution that we supported a number of people at in the theory area. The University of Washington we had some support in the theory area that I recall. Cornell, I am fairly certain, especially on information retrieval related things I recall a project that we funded in part through my program at the time, which seemed to be doing some significant things and I believe some good results came out of.

ASPRAY: Somebody like Jerry Saltin?

TARESKI: I remember Jerry's name, but I don't recall if he specifically was the person that was supported at that particular program. Again, of course, the history records show those grants. I am coming up dry right now for any other pointers specifically that were just theoretical computer science as opposed to AI-related or information system supported related, which were the other kind of things that were in the program, which were somewhat more interesting to me personally. Because my research had been in AI before I left Illinois, specifically pattern recognition for medical records. Interestingly enough, the support had been by the Atomic Energy Commission because of the high energy physics pattern recognition - the goals that they had at the time.

ASPRAY: One of the things I note in your list of institutions is that the big three are not listed. Was there support for Stanford, Carnegie-Mellon, MIT?

TARESKI: From the Science Foundation, yes. From my program, initially no, because most of those people had, in fact, gravitated to DARPA. Interesting, though, while I was there there was a time when there was this big cutback in DARPA funds for AI-related research and the Science Foundation got a special supplement of money targeted to try to pick up "the best of the more fundamental research from those institutions." So indeed, then we did visit the MITs. I did not; Kent did in most cases, because they were large site visits through a whole other situation. The MITs, the Carnegie-Mellons, and the Stanfords all did get visited, and we did pick up researchers during that period of time in the AI-related areas to pick up more fundamental aspects. So it was later in the jury when the support tended to dry up from what had been the pork barrel that the NSF then started picking up some researchers from those "major players."

ASPRAY: This must have been in 1972 when Heilmeier cut back on the AI?

TARESKI: I believe that's right.

ASPRAY: Can you tell me about some of the problems that you had as a program officer in developing the program that you thought was a good program that you thought was a good program? What were the challenges?

TARESKI: [laugh] Staying ahead of the paperwork, in part. It was always a challenge - it still is today - trying to figure out what the hot buttons are to sell your program to the people above you. We have had within the Foundation our program reviews that would be reviewed by the office and by the directorates above us and so on. And you would get a window time - so many minutes - that you got to present the really super nuggets out of your program and why your program was super and great. And the feedback basically was only your budget next year, so you never knew which things were really playing well and which weren't. And so it was always a real challenge trying to figure out which nuggets to promote and which things to try to encourage a better funding picture for the areas that you were trying to support. I found that the most frustrating in that the feedback was very minimal.

ASPRAY: Who would be present at such a presentation?

TARESKEI: Typically it would be the director of the Science Foundation and a couple of his assistant directors along with most of the people from the division then that would make the presentation.

ASPRAY: Would you get more feedback from other people within OCA? Did you get more feedback from Kent Curtis?

TARESKEI: We worked very closely with Kent in developing the materials, and so the feedback from him was immediate and direct, but it wasn't he that really determined the final budgets other than perhaps a small tweaking between the three programs within the division or within the section.

ASPRAY: What about John Pasta?

TARESKEI: I didn't really get a good feeling for exactly how much latitude John had, quite frankly, at the level I was at. I saw John very little, conversed with him very little. In fact, one of the longest conversations I had with John was during my exit interview when I was leaving. [laughter] Not that we didn't have, you know, a decent relationship. It's just that he was on travel and he still continued his research. He was still active; about probably 25, 30% of his time he was still doing active research in theoretical, computational physics while he was still directing that program. And so he was a busy man. And so I got the feeling, you know, I didn't really want to bother John with my problems, because I really I needed to go through Kent. I believed in the hierarchical approach. And as a result the feedback was minimal. I never personally got a good feeling for it. Maybe some of the other people who were on the professional side would have a better feeling for how that feedback would work. I didn't really get a good feeling of it while I was there from the office level back down to the section level.

ASPRAY: It seems to me that the rotator system has some quite obvious advantages bringing new ideas in on a regular basis, but it also has some problems in that, especially if a person is only there a year, it takes a while to learn

the system, to learn the mechanisms.

TARESKEI: It was a real problem for the real short-time people, and that was part of what motivated me to stay longer is that I felt that I was just getting to a point at the end of the first year that I was able to make a dent, if you will, in being to do something, and be able to really start fashioning the program. I have talked to other rotators since then and they have come away with somewhat the same impression, although apparently they have made some efforts. One of the faculty members at our institution was a rotator a year ago, and they have made some efforts, at least in his area - it was chemistry, to get the rotator up to speed a little faster, so it was possible within six months for him to start feeling like he was doing something manageable. But that wasn't the situation from my case. It seemed like at that time period. Or maybe it was just me; it just took me longer to get up to speed, but the other rotators that were there - there were several during the time I was there in other programs within the office - similarly tended to feel that they were just getting up to speed, if you will, at the end of the year, and here it was time to go already. And so that part was a problem indeed, getting to learn where the hot buttons were and how to do things within the Foundation. On the other hand, yes, coming in, bringing in those outside ideas were very great. The conversations in the office were much enhanced by this outside, fresh blood, if you will, that came in constantly. That was a great feature, I believe, of that rotator system.

ASPRAY: Was there some sort of special training that was given to rotators to get them up to speed?

TARESKEI: If there was I didn't see it. [laughter] It was just suddenly, "Here's your desk and here's the kind of things you do, and here's a load of proposals that we're presently processing," and I think that maybe in an hour I overviewed the process and then it's just... "Sink or swim. Start at it. And if you have got questions come by and ask." And so it was kind of a learn by doing process, and I didn't even know about the program review process in terms of meeting the Foundation directorate and this sort of stuff until fairly far along, I believe, in that first year.

ASPRAY: How did you choose reviewers for proposals?

TARESKI: That was an interesting problem. I had no idea how to do that, having always been on the other side of it. Being on the writing side, I had no idea what the process was, and I was given some guidelines on that. But part of it is just plain old "who you know" from your area of expertise - from supposedly your research area that you have been active in and conferences you have attended you have gotten to know some of the players. And so that was the first key was working with some of the players that you already knew, and the second key was, in fact, the study, the references that were listed in the proposal itself. And they induce some expiration of some of those references on a one on one basis to see if that reference would make sense as a reviewer. That was developed quite a bit while I was there. In fact, I helped John Lehmann develop a database for at least our section of reviewers, and it had not really been a division -- no division-wide database, but there hadn't even been a section database before that point that reviewers would use. And so, as it turned out, we started overlapping reviewers and of course reviewers would complain, "I am getting too many proposals." I'm getting from several reviewers and it's starting to flood me, and so we started developing a database for this section and put it on the computer - timesharing at that time, which was pretty significant. We bought outside timesharing service to do that, which was another interesting little game. John and I worked on a program to develop a database and then also to try to automate some of the forms processing of soliciting reviews and also doing review follow-ups from the reviewers who hadn't yet responded. The database was a growing one, and as the database helped, then of course it very much helped then selecting fellow reviewers in the future, because then you could go back and say, "Oh, yes, I used him six months ago, and it's about time I can use him again," and "Oh, there's a name I haven't used for a while," or "Oh, there's a name I don't even recognize. I wonder where that name came from." So that was something that was being enhanced at least in the computing activities area during the period I was there. And I understand, then, that whole thing was put on the database within the Foundation's computer and integrated into an overall process in the whole Foundation in the later years.

ASPRAY: What did you think of the quality of the reviewer's reports you got?

TARESKI: [laugh] It varied all over the map, all the way from those one-liners, "This is a good guy; fund him," to pages and pages of material at the level of detail that was probably too much. The reviewer spent too much time on it, but it was fantastic feedback for the PI, if you could cull out the things that would be somewhat pointers to who

the reviewer was. Sometimes you couldn't, unfortunately. It twined the interview in such a way that you couldn't even feed it back then to the investigator. But it was a very wide spectrum and it was a real problem. If you got two or three reviews on a proposal and two of them were these shallow things it made it very difficult to make reasonable decisions as to whether the proposal was fundable or not. And so that was another significant challenge in the funding process is trying to get a reasonable set of reviews that you could use as a basis to argue for funding or argue for declining funding on particular proposals.

ASPRAY: Yes. How much uniformity did you find in the reviewer's reports?

TARESKE: It seemed to depend more on how much reviewing a particular individual had done earlier. Those who had been reviewing for the Foundation or other agencies, the reviews were fairly good. They were things you could trust. On the other hand, since it was a very new and evolving field, we were bringing on-line a lot of new reviewers who hadn't been in the process before and those were much more challenging to try to make use of the first time or two around.

ASPRAY: For any given proposal, the three reviewer's reports you would get in, did you get a fair amount of uniformity in what they thought of them, or was the field not well enough developed that you would get them up and down the spectra.

TARESKE: It depended on the proposal. Some proposals were in areas which were in a fair uniform agreement as to the importance of and the reviews then would be fairly uniform - either good or bad, depending on what was being proposed. On the other hand, in the more controversial areas, or areas where there had been roadblocks or other problems developing, then you would get some wide splits in opinions as to, first of all, the importance of the work as a general issue, and then secondly, whether this particular researcher might be able to do something significant or not in that particular research area. And that basically forced me when I was there. I tended to, when I got that sort of review, I would go out and try to get some more reviews to try to see, to try to see if I could see if I could find a consensus, at least at some sense. And some times - I recall one proposal - I never did get a consensus. And that's

real challenging then, as a program director, as to what to do as they were making recommendation to my boss, in this case, Kent Curtis, as to what to do with that proposal. So sometimes we would go in and we would have hard soul to soul discussions on the proposal and try to make some determination how to proceed with that proposal from that point. I think that particular one fell through the cracks because you ran out of money. So then you could take the approach, "Well, if you really want the money in the next six months you might consider this proposal dead. If you want us to hang onto it, wait another year for the next funding round, we might be able to look at funding again." So there were other things that came into play sometimes that influenced what you did with those questionable proposals.

ASPRAY: How would you judge the number of high quality proposals versus the amount of money you had to give in awards?

TARESKEI: Well, at first I was able to fund basically almost anything that was really high quality and going down into "very goods," because the program was relatively unknown, so we weren't getting very many proposals out. As I beat the bushes, if you will, then it started being more and more challenging and the weeding-out process became more excruciating, if you will. Like most people you always hate to say no to anything that looks even reasonably promising, and there were times where you had to toward the end do more of that, although the funding did keep increasing at a very nice pace. But the number of proposals increased faster than funding, particularly when we were dumped on with a bunch of the DARPA people. Even though we got an increment to cover them, we got no where near the increment equal to the loss to the community, and so we did have to do some ugly reading, if you will, at that period in terms of turning down people who had appeared to be doing very good work and just didn't quite review very well under the NSF format. They perhaps were reviewing very well under the DARPA format, but the in the NSF review format they failed to make the mark.

ASPRAY: Do you know whether, this is sort of funny terminology, but NSF was able to hold on to those researchers when there was an upturn at DARPA, or did those people that had been DARPA-supported go back to DARPA support later on? I know it extends beyond your time at the Foundation.

TARESKEI: Of course I tried to track some of that by continuing to attend the appropriate conferences, but I don't really have a good feeling for that question or to the answer to that question. Some of them, as it is always going to be the case, switched their areas of interest so that they could indeed move back into the new thrust of DARPA. I was aware of a few that did that. What percentage or how big that number is I really don't have a good feeling for it. Perhaps the person who came on after me or even two times after me would be in a better position to be able to respond to that.

ASPRAY: Did you know that you had a fixed pot of money at the beginning of the funding year?

TARESKEI: Pretty much.

ASPRAY: Or was it that you knew your section had a fixed pot and you had to fight with the other two?

TARESKEI: No, I never had to fight with the other programs, which was nice. The numbers were fairly fixed. The only danger we have learned as I was there is sometimes you can lose some of it because of presidential decree that there is going to be a broad-brush cut of the funding, and then of course that would move all the way down. And suddenly your 1.5 million is only 1.3 and that was right about the time when you were funding some of the last few people that you really wanted to fund, and suddenly now you had to run into budget problems. But within the section while I was there we never really had any evidence of infighting between the three programs. And I think I had a good chance to see if there was any, because one year Kent went on leave, and so the three of us program directors acted in concert, if you will, as section management as well. And during that time period, if you will, it would have been an excellent time to "have done some fighting," and I don't recall any problem of that type.

ASPRAY: Who were the other two?

TARESKEI: At that time it was John Lehmann and Tom Keenan.

ASPRAY: I have met and interviewed both of them. If you were going to emphasize some themes and historical study of this, what themes would you think are important ones to come across in this manuscript?

TARESKI: Well, I would say the period that I was there I think was one of the very important periods in computer science formation. A lot of schools were still trying to define what computer science was, "Did it need to be a separate degree program? Should it be a separate degree program?" And all those sorts of issues were very hotly debated topics that I saw and heard as I was traveling around to conferences and I was aware of because, indeed, I had just gone through a program that was still in its formative stages at the University of Illinois and had competed in competing programs there at that school as had occurred in many other schools. Berkeley was a classic case where they ended up, I believe, with three different programs in the computer sciences during that period: one very theoretical, basically each one a different college type thing. And so it was an interesting formative period. The Science Foundation reflects what's happening in the science community, and so we moved from an office status to a divisional status, recognizing that computing didn't need the support as much as computing science. And I felt that formation, and the people that were at the Foundation at the time were very important in the internal discussions and forming how the Science Foundation was going to respond, but also in interacting with the community and feeding back our own opinions back into the community about how the community ought to be developing computer science. We influenced that by funding. You have an identifiable target for funding - theoretical computer science. Then departments and universities start saying, "Well, gee, maybe it is a valid discipline because indeed, there's real money out there supporting research in this area." And so that, I think, was very important and a very key activity going on during that time period for the whole area of computer science. And in fact, of course, there's still some discussion, but there no longer near the same level now as they were at that time period about how to form computer science programs.

ASPRAY: Other general themes?

TARESKI: Well, a general theme was just winding down [when I joined NSF], and so I have very little to do with it,

but I thought it was important. It was just the Office of Computing Activities had to of course fund a lot of facilities - a lot of computing facilities. And I think that was very, very important for many of these institutions to get going on computing in various disciplines, not just computer science, but computing as a general thing. And that, I think, was a very important role. My own home institution, for example, participated and benefitted from that and helped thrust us into computing. And as I made some of my early site visits there were plenty of evidences of that funding program, of funding and computing facilities did kick a lot of things into gear at many campuses to make computing viable and a valuable resource for researchers as far as discovering, "Hey, I can really do some much better research involving things where I need to do computation by making use of this new computing facility as opposed to using "a Monroe calculator." And so, I think that winding down program that I got to see the tail end of, if you will, was also very significant from the Foundation. If that program hadn't been there I think computing would have been stunted on campuses significantly during that time period.

ASPRAY: Yes. What about the computer in education? I know that was something that had been a very major thrust.

TARESKEI: I believe a group in the office that was still funding that, and then there was the fun and games of running out of space in the building, and so they got shuttled up to a remote building that was off the end of the district. And so I saw those people very little for a while because of them being at a physically different site. I felt, being in education, that what they were trying to do was important and key to some things that were happening. But that wasn't necessarily agreed upon by others apparently, because they kept always getting the short end of the stick in funding within the Science Foundation. At one time, in fact, I believe they basically got cut off. I don't remember if it was while I was still there or if it was after I left or whatever, but they have had a hard time. Science and education in general at foundations had rough days. They had gone through cycles many times around in fact, partly all the way up to the presidential level these things seem to have been a real problem. And I think that's unfortunate. But of course I am biased; I am in education and I see how important funding fellowships and scholarships or just science education per se for undergraduates is. It's been a rocky road in that area. I personally feel it's important and I thought the group in the office that was working on it at that time were trying to make a dent on it but it wasn't clear

that it was a big enough dent given that they had so much trouble continuing to have funding in the area.

ASPRAY: Tell me, if you will, about the overall importance of Foundation funding to theoretical computer science, sort of summing up and assessing.

TARESKEI: Well, we came in at the time I started as basically a weak player in the area, and by the time I left I think we were considered a very significant player. One of the things I did to try to use as information to try to justify the program to my management was when I went to conferences, start seeing both things that happened to the conference, how much of it was supported by various agencies, where the support was coming from, and who was doing it. Early on, very little support, and the papers and the presentations were by people who were funded by the NSF. A lot of it was by the military, and a very significant part of it was unfunded. By the end of the period that I was there the NSF portion rose sometimes as high as 50% of the participants at those conferences I attended. So I think the Foundation became very visible and was playing a very significant role in developing the theoretical computer science, and continued to do so after I left. Now, I have lost some track of it since I have moved onto another area of interest myself. But I believe, just from talking to colleagues in general, that they are still playing a significant, perhaps not as large a role in the theory of computer sciences today. And part of that had occurred during that period I was there, I believe.

ASPRAY: Do you think that it was somehow easier to convince the Foundation to support theoretical computer science than, say, certain kinds of experimental work or engineering work because it looked more like scientific work, like mathematics?

TARESKEI: That was always an interesting discussion, both from within the Foundation and external to the Foundation. It was always interesting play. And I am back on the engineering side now, and there's always that perception that the Foundation doesn't fund engineering things anywhere near to the same extent that they do the physical sciences - more traditional, biological and theoretical mathematical sciences and physics and so on. And I have trouble with that. I saw it from the inside and I saw there was really a problem trying to do a balance. When

does it become too practical to be fundable as fundamental research? My program, of course, was easy, because almost everything, other than the intelligent systems, where I had to fight that problem a little bit, was clearly very mathematical and very theoretical. So my program was easy to argue from the theoretical side that this is really important stuff and is built into the Foundation. But the other two programs, the one that Tom Keenan was supporting and the one that John Lehmann was supporting were much more difficult to justify at times because of the far more practical side of running a computer program, or developing the concept to running a computer program, or building a new piece of hardware or developing the concepts for building a new piece of hardware, like John Lehmann's program was partially supported. But it's a delicate balance. I don't know that the practical side was losing, but it was a constant battle to keep it from coming up short, I think. And there was a sense that we were fading out, when I first got there, of the facilities support and just general computing support, and more directed to research support. And people were coming in with special kinds of projects that were hard to categorize, because some of it still was really kind of, "Well, let's see if we can figure out how to use computing in discipline x." How do you fund that? That's not basic research, is it? So you get into these kind of discussions and arguing money for some of that was very challenging because it kind of applied. It wasn't basic computing, or was it? We were arguing some of those kinds of questions and discussing them. Some people succeeded in getting funding and some didn't. And those who didn't, of course, always felt, well, it's because the Foundation isn't supporting more practical stuff adequately. I don't know, I think the balance is okay.

TAPE 1/SIDE 2

TARESKI: I too have sensed that the practical (whatever that means) type of research and developments gets the short end of the stick. So I am always happy to see that there's a reminder to the director of the Foundation and to the associate directors that we can't short the stuff that, even though it looks a little bit on the practical side, that will still develop and can (I felt very strongly about that) feed the pure theoretical areas. But you're right. It was a problem. It still is.

ASPRAY: One program director for theoretical computer science from a period much later than your time there has

suggested to me that once the Foundation moved to from facility support and from education support and turned to research support that it was theoretical computer science that was the heart of that support, and that it drove the program in the kinds of directions it did over the next 15 years, and that in some sense is the key to understanding the research support area of the Foundation in computing. Do you want to comment on that?

TARESKI: Only to say that's an interesting observation, and I can see how that kind of observation could evolve based on just our previous discussion or previous questions you asked. I guess I really don't have any further comment on it. I don't have a good feeling for whether that would be true. Even though I have touched base and continued to correspond a little bit with John Lehmann and, to a lesser extent, Tom Keenan I haven't really discussed or broached that subject with them recently. So it would be out of bounds for me to comment on that.

ASPRAY: One of the mechanisms that was suggested as part of that comment was that theory started out looking like a mathematical discipline in some of the early proposals, and that as people went on through their work they often found there was a hardware or a software component to this and that they frequently would be spin-offs in those areas and that some of the funding programs and database work and in software work resulting or grew out of the theory program. Did you see any of that kind of growth and splitting occurring during your period there?

TARESKI: While I was there it was more the other way. People who were doing the practical stuff, because they were still trying to define computer science in part, were discovering, "Oh, there might be some real theoretical underpinnings for some of this stuff. What is it? Maybe we should be exploring that. Maybe we should be investigating that." So a lot of the people that I saw were coming into the theory area, not a lot, a number of them were coming into theory area because the practical things that motivated them to look deeper, to look for the fundamentals of computer science, whatever they might be, which was still very unclear at that point in time. Some people argued automata theory was, but there was a lot of disagreement about that as being a fundamental basis for the practical side of computing. And that was what drove, I think, some of the funding that did occur. And all three programs were funding, I think, some things that were going that direction and having the practical component in the software, but they would also have some, at least in some of the proposals, things where they were providing to

study the underpinnings of what they were trying to do on the practical side. And I happen to know the software area, and I recall a discussion with John Lehmann that that was also happening in the computer hardware area. So I think that we were still trying to define what theoretical computer science was. So the gravitation was, for many of the people, from the practical side to be moving into the theory. Now, the mathematicians weren't always, of course, so staid in the theoretical side. And I personally had some problems with that. I did communicate with the math section. There was one program in the math section that had good cross-overs between computing and mathematics. I don't even recall what the program was at the moment, but we did jointly fund, in fact, a few things that looked a little too mathematical, in my viewpoint, to be "computer science." And that was part of the problem, "What is computer science?" So when it started looking like it was just almost abstract math then I would go down and talk with the math section. Sometimes they would take the proposal entirely; other times they would say, "No, there's too much computing there," and then we would maybe jointly review it. And at other times the math section would refuse, "No, this looks too practical to us; it must be computer science."

ASPRAY: [laugh]

TARESKI: So there was that kind of jockeying going on also, so it was an interesting position at time is that I was bridging other areas of the Foundation: the mathematics side on one side - I was talking to two or three different program directors in the math program, which was considered a stable, well-defined program; and two or three program directors over in the engineering directorate on the more applied side of intelligent systems particularly. John Lehmann was also communicating, I believe, in his program with some engineering directorate people as well. So there was this bridge that I think the theoretical computer science program was doing the best job of bridging at that time and jointly funding things that we couldn't make up our mind and we couldn't discern that the scientific community had really made up its mind as to which kind of research that particular object was.

ASPRAY: Unfortunately, this project was started too late for us to talk to Kent Curtis.

TARESKI: That's unfortunate. He had an untimely early death.

ASPRAY: Can you tell me about the man and his work style?

TARESKI: He was interesting. I enjoyed working under Kent. It's partly what motivated me to stay longer than just the one year was that he was a laid-back style, easy to work with, willing to help you if you had a question or concern or problem - willing to work with you. I guess I would characterize it as something of a "hands-off" style of management. If there weren't any problems he didn't come over and beat on you. He didn't give you any grief. If there were issues he would come in and you would have a chat about it, but it wasn't ugly; it wasn't bossy, if you will, in the sense of "Hey, this is the way it's got to be; we've got to do this." He was very easy to work with in that regard. He was a hard person to really know deeply. We socialized a little bit with him - my wife and I - with his wife, ate dinners together and so on. He was a very complex person, and very intelligent, even though he had never managed to complete his Ph.D. degree I had no qualms about the nature of his insight into what was happening. And of course he was like many of the other people, including John Pasta, who came in from another discipline into computing science, as most people in the office had at that time. I can't think of anybody, in fact, on the professional staff during that time I was there that had a computer science degree; it was all degrees somewhere else. But he was very "hands-off" but very good to work with. I had no problems at all working with Kent. I had troubles sometimes, as I say, understanding him - deep person that he was. And it took me a couple of years to get to appreciate that side of him. And unfortunately, I get one year cut off in the sense that he did go on leave the one year I was there, and so I didn't get to develop further understanding him in the time period he was gone. But during that period he was gone I think he must have done a good job of running the section, because we didn't find it necessary as ad-hoc runners to do any major changes or big plays to modify the structure that he had developed for the section.

ASPRAY: How do you think he was received by the computer science community?

TARESKI: Kent didn't go out and blow his horn much, and so when he went to conferences I didn't perceive him perhaps as proactive as I had tried to be when I went to conferences. And so as a result people in some cases didn't even know who he was. The people that really made it their business to know the funding agency, of course, did.

But the new investigators or the people that had been only focusing on their research and just running it through the paper mill without really being aware of the whole process generally didn't know who Kent was. On the other hand, certain people did obviously. The year he was on leave he was I believe in New York, and it was doing things on Non-structured languages of some kind. Their research work that was going on there, and they were very aware of his underpinnings and his ability to move into working in that area. And so they were very happy to offer him that opportunity to come and work with him on that research. So, certainly there were people in the community who were very conscious of Kent's intellectual capacity, but there were a lot of others because of his low-key approach to things that had no concept of where Kent was coming from.

ASPRAY: What about around the Foundation? How was he regarded?

TARESKEI: I don't think I can answer that. The other program directors that I interfaced with, like the math section and the engineering section certainly knew of Kent and I don't ever recall them saying anything derogatory about him, but I don't recall them saying anything particularly spectacular about him either. He was just there. The management chain levels above, I really don't know. I didn't really deal with him, or give him a chance to "have coffee with those people."

ASPRAY: Are there any other things that you would like to say about your experiences with the Foundation?

TARESKEI: Well, I came away with a very positive feeling about the Foundation - very much more appreciative of the importance of that organization in the development of computer science in the United States. It's a very, very critical organization. Without it, I think this country would have some real science holes. But part of it is very much attributable to some very strong leadership. I think, for example, John Pasta in the Office of Computing Activities, was a strong person, who was very instrumental, from what I could see in building computer science programs in the Foundation and selling the importance of computing in the Foundation. At the point I think he may have been a very significant player. Perhaps you have other evidence of that, but my own perception was he was a very key player in moving the office from a general office and a somewhat non-entity, which is the view of offices at the Foundation, to

a separate division which then became "in status" and a very recognizable, normal Foundation function. And I think he was a significant player in having that happen. And, without that strong leadership at that level or at any of the other directorates, definitely the science community suffers. So it's important, I think, for the science community to continue to promote and do things that will encourage good, solid appointments of those key players at the Foundation leadership positions, from the directorate, and assistant directorate and associate directorates down even to the section levels. And I think it's also important for the community to continue to do what it can to support the rotating type program. I think that's invaluable to make the Science Foundation stay vibrant.

END OF INTERVIEW