An Interview with

ALVIN THALER

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Conducted by William Aspray

on

28 September 1990

Washington, D.C.

Charles Babbage Institute
Center for the History of Information Processing
University of Minnesota, Minneapolis

Alvin Thaler Interview
28 September 1990

Abstract

Thaler describes his experiences as a program director in both the mathematical and computer science divisions of the National Science Foundation (NSF). Topics include: EXPRES, an interoperability program of NSF; computational mathematics; theoretical computer science; the work of John Pasta; Jim Infante and the decision to separate mathematics and computer science into two divisions; the role of other funding agencies; and NSF support of computer science research.

ALVIN THALER INTERVIEW

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ASPRAY: I would like to begin by asking you if you would tell me something about your own background, how you came to the Foundation, what your career was like before that.

THALER: I came to the Foundation quite a while ago. I have a Ph.D. in mathematics. The Ph.D. was from Johns Hopkins University. The degree was awarded in 1967, I guess. I started as an algebraic geometer algebraic number theorist was on the faculty at the University of Maryland almost 20 years ago in 1971. I started the algebra program. I was the first program director for algebra actually, assistant program director for algebra did that for a number of years until some time in the late 1970s when mathematics and computer science were merged administratively. I worked very closely with the director of that division, John Pasta, and became program director for special projects in the then mathematical sciences section.

ASPRAY: What's that mean special projects?

THALER: Special projects was then a repository for unusual proposal, conferences and the like, but in a, very controversial for mathematics, administrative move, the concept of an NSF-funded research mathematics institute took shape, and it was in response to some intense activity to start such an institute that we decided that it would be appropriate to have a full-time program director in special projects. Before then it had just been a part-time position run out of the section head. And so I did that for a couple of years. There was funded two major operations: the Mathematics Research Institute at Berkeley and the Institute for Mathematics and its Applications at Minnesota. They both came out of those years. I did that for several years. In 1986 I took a sabbatical and went off to the Courant Institute and tried to learn something about the relationship between mathematics and computer science. And in fact, I worked in the ultra computer group the parallel computing operation, the prototype parallel computer at NYU, learned a fair amount of what goes on with the UNIX community and the problems in designing peculiar architectures in computer science and the issues of interfaces and all of those headaches and problems and stuff in a computer science that lived within a very high quality mathematics department started within a high quality mathematics department and then became an entity onto itself. And then I came back to the Foundation and was asked to run a peculiar research program in the computer science directorate called EXPRES. EXPRES was a project that appealed to the director, Mr. Bloch. EXPRES stood for experimental research and electronic submission, and EXPRES was a project funded at a couple of million dollars a year for three years designed to try to study the problems of having a multimedia communication environment that would allow scientists to use the computer to communicate among each other and to share results. The handle that we used was the NSF proposal submission and evaluation process. But in fact the EXPRES idea had a much broader goal. The idea was to try to allow scientists, engineers, mathematicians to communicate broadly among each other without worrying about the horrible problems of heterogenous environments. And that went on for a couple of years. That project was terminated for a variety of reasons, and then I came back into the mathematics division and I am now running the computational mathematics program.

ASPRAY: Maybe we should spend just a few minutes talking about EXPRES. Can you tell me about the goals and some of the challenges.

THALER: The goals were to try to study the problems involved in establishing an interoperable (That's a terrible word, but it's the word that was used. Computer scientists and engineers in general are not good at language. They tend to use cumbersome words.)... Right now, with the computation and communication environment it's very easy to send electronic mail. The electronic mail is all ASCII text. You can't do multi-font; you can't do mathematical characters; you can't do graphics simple graphics, bitmaps, or anything else. You can send files back and forth that do these things. I mean, the technology is there and it's trivial, but the way the world is constructed it's virtually impossible that your environment can understand a file that I prepared using my environment unless we agree on the same software and in some cases even the same hardware, it's a major headache. We did fund two different organizations universities: Carnegie Mellon on the one hand, and the University of Michigan on the other hand.

Each of these built a self-contained environment for preparing multi-media, mixed font documents, mixed font graphics, bitmaps, spread sheets, animations. And that of course is just the beginning. The idea was to demonstrate the feasibility of interoperability of these two, in fact, very different environments, and to try to have them translate documents between each other using some of the emerging standards. It was very hard. The technical problems were enormous. The social problems were enormous.

ASPRAY: In what way?

THALER: First of all, neither of the environments was as polished as one would have hoped. They both were out of academic institutions. We learned a lot from it, by the way. We learned how hard it is to come up with commercial software... commercial grade software, how important interfaces are, how hard it must be for the commercial sector to come up with a product that is releasable, that in fact works, that doesn't break all the time. We learned about a lot of the social problems in trying to introduce new technology... leading edge technology on people who are somewhat resistant to incorporating even straightforward technology. We learned about some of the mechanical problems, storage problems, security problems, and the like. We learned that (and we didn't know this when we started) such an environment, although technologically quite straightforward, was really quite far from widespread dissemination and use "far" meaning more than two or three or four years from when we started. We started in 1986; we terminated it in 1989. We learned a great deal, as I said, but it was quite clear in 1989 that we would not have an environment that would allow an interoperable communication of documents in revisable format. By revisable I mean FAX works; okay, FAX is fine. But the problem when I send you a FAX is that you can't process it, you can't do word search, and so on and so forth. TEK works; TEK is pretty, but it's the same problem. The electronic communication that we have still think in terms of paper. This is a major gripe that I have. When people talk about automating the office what they do is they turn paper into electronics, but they treat the electronics just as though it were paper, which is dumb and inefficient and inappropriate, and so on and so forth.

ASPRAY: Were there in addition to learning the dimensions of the problem were there some modest successes?

THALER: Oh yes, we did achieve interoperability between these two very different environments. The Carnegie Mellon operation was based on the Andrew [?] file system and the Andrew operating system, and it was a hierarchical, object-oriented, very computer sciency operation. The University of Michigan operation was based on Diamond, which is something that was built by BBN for DARPA, and the Diamond operation was I don't want to be unkind It was designed to be a demonstration for the Defense Department, and it worked in some sense, worked by brute force. There were a lot of things that were just hacked together, and it had some nice features. It had some thoughtfully designed features. It had some nice interfaces, but the structure of it was really very, very different from the structure of the Andrew operation. We also were involved somewhere in the middle (and this is really very exciting) with Interleaf, which is a major commercial player. McDonnell-Douglas, it turns out, had its own project working on notions of interoperability and trying to use emerging standards ODA (Office Document Architecture), which is a standard. We were able to demonstrate just about at the time that the project was canceled that one could have translation of some pretty complicated formatted, multi-font documents from Diamond to Interleaf to Andrew and then back to Diamond UM Express, it was called and it worked. So we did, in fact, demonstrate the feasibility of interchange. It was hard, but we did do it, and fairly inexpensively given the fact that we have small numbers of people. They're quality people. People at universities are just wonderful. And we demonstrated that we could process proposals from transmission at the university to NSF in a revisable format and have the entire thing acted on with no paper at all. We did that in a couple of cases. We did not exploit that. It was expensive. We needed substantial equipment on people's desks. The machine in the Foundation almost without exception then was the 8086 PC. Now we have mostly 286 PCs. We have, in some sense, a primitive environment in the Foundation. And one really needs a decent-sized screen with a decent windowing environment to do anything intelligent, really, but certainly stuff like that. So that's what we learned.

ASPRAY: Are there plans to try to implement even a modified or more modest version of this on a larger basis now?

THALER: Not really. We're trying to do other things. I am funding an enterprise in mathematics called E-Math to try to have an environment for collaboration and communication among mathematicians.

[INTERRUPTION]

ASPRAY: You were in the midst of describing something called E-Math.

THALER: Yes, that's kind of exciting. The buzzword over there now is collaboration technology, collaboration science. And I hope they do some pilot projects, some pilot studies along that line. I think the prospects for incorporating and pushing technology from the academic community are quite exciting and I would very much like to see that stuff continue.

ASPRAY: Let me change topics. Computational mathematics can you give me a historical background of what's been done?

THALER: I would love to, because that's interesting. Computational mathematics is a new program, and in fact I have a lot of trouble explaining to people what computational mathematics is. [interruption] So, computational mathematics. The math division has always supported numerical analysis and things like that. I guess in 1986 when I left for my sabbatical it became increasingly clear that exciting things were happening with technology and with computation, and really different kinds of things. And so the division I think very wisely decided to start a new program called computational mathematics. And the idea of the computational mathematics program was to try to fill those gaps that were being created by the turmoil by what was happening to deal with interdisciplinary work, to deal with things that were clearly going to happen but weren't well understood now. My view of the program right now is that the computational mathematics program is here to take advantage of the kinds of things that are caused by current revolution in technology. There are aspects of the technology that have increasing impact on traditional mathematics pure mathematics, which are really exciting and really quite surprising. The most exciting project in my program is a big award to the University of Minnesota, to something called the Geometry Supercomputer Project, which chews up a lot of money for mathematics. Most mathematics grants are quite small. This is a major visualization and graphics operation using very high quality silicon graphics computing machines. And the thing that's exciting about it is that it is driven by world-class mathematicians. Thurston is one of the principals in it. The activity has three or four fields medalists. Munkford [?] is involved; Freedman is involved; Mandelbrot's involved all kinds of people. And what they're doing is slowly converting lots of traditional paper and pencil and cerebral matter for mathematicians into understanding the power and value of these new tools to help them do what they have done traditionally and to point them towards new programs. I think it's just wonderful very exciting. There's that aspect the aspect of technology on mathematics itself. There is the aspect of mathematicians being involved in science across the board. My view is that there is really a new paradigm of science that's emerging. Experimental science is being, I won't say replaced, but, augmented by something that we're calling simulation science, where in fact the model is no longer the set of equations but in fact the code itself. Mathematicians have a very strong role to play in that a stronger role I think than mathematicians think, and certainly a stronger role than computer scientists and traditional physicists and chemists think. The major advances in computation come more by algorithm improvement than by hardware improvement. One can document this. The hardware improvement is wonderful and it's exciting, but the breakthroughs in factorization and the like the real breakthroughs come by somebody understanding how to distribute the problem, how to attack the problem, how to parallelize the problem. The other coin of the computational mathematics program is that mathematics has a lot to contribute to the technology in an extended sense. There are problems in designing machines in a machine architecture that are inherently mathematical. Visualization the stuff that goes on now there's no scientific structure; there's no scientific underpinning. The idea is if they emerge we'll have to be mathematical. The notion of optimizing code to a particular machine architecture. We all know there's no such thing as a general purpose machine. We all know that there are people who make lots of money optimizing code to particular machines. There's a major need for that. And right now it's a black art. Nobody understands it. There's a science there. You know, it's just the same as picking the right curve from a space of curve. And again, this is 85 years down the road that's what computational mathematics is there for. It's not computational fluid dynamics as important and exciting as computational fluid dynamics are. In fact, I am really excited about the program.

ASPRAY: But in a sense that exhibits many of the same characteristics of mathematical modeling that some of these programs just suggest. It's just that that one's been well-defined.

THALER: That's right. The computational fluid dynamicists are still supported extensively on the program. The computational fluid dynamicists really are positioned culturally and historically because of their traditional reliance on computers. And computational fluid dynamicists are all there with all their problems lined up waiting for the next generation of hardware. And they're fine. There's no problem with them.

ASPRAY: Is this additionally supported within mathematics or with other divisions?

THALER: The answer is yes. We do a lot of split funding. The more mathematical ones, of course, are supported in mathematics. The more engineering ones are supported in engineering. We do a lot of split funding. Applied mathematics cause more split funding than the traditional mathematics areas, and computational mathematics, I think, does more split funding than applied mathematics. The nature of computational mathematics is that it's supposed to leverage and fertilize. One of the things we try to do is make clear the need for equipment in the mathematical sciences   try to encourage it.

ASPRAY: And is it part of your program specifically to provide facilities?

THALER: Well, that's a funny question. When I was program director for special projects one of the programs I started was something called SCREMS   Scientific Computing Research Equipment for the Mathematical Sciences. There's a brochure that's been prepared   that's the recent brochure. (There's a change there, by the way. That program was started in 1980, if I remember right. It was designed in response to the perceived needs of the statisticians and applied mathematicians who, in fact, needed access to computation, but it turns out that there were no computers available in academic statistics departments. And so we started the program called Scientific Computing Research Equipment for the Mathematical Sciences. And I administered that for three or four years until I left and went on sabbatical. It stayed in special projects and has been in special projects for the last couple of years.

But this year, because I am back and because I want it and because it fits nicely with the expertise in the computational mathematics program I am going to be running it again. It's a separate program. And by the way it has changed... the title has changed... We changed the word equipment to the word environments to try to provide for the need for support staff in these businesses. I am sure you know that once you buy the UNIX box your problems have just started. So we are trying to respond to that. So, yes, we do try to respond to that. I spend roughly $800,000 a year in the computational mathematics program to provide equipment to people in other disciplines, people in other areas.

ASPRAY: Out of a total budget of how much?

THALER: The computational math program is somewhere between four and five million dollars I think 4.7 last year.

ASPRAY: What's the reaction of other sciences in this community to your program? Are they interested?

THALER: We work together partly because I have been there and I know all those people. We split fund a fair amount with symbolic and numeric computation Kamal Abdali [?]. That's the principal split. When I was in SIIS I ran the SIIS instrumentation program, actually, which is a different kind of a program. Cherniavsky runs that now and there was still some communication on that. I have been involved with equipment since we started funding equipment seriously.

ASPRAY: A fairly pragmatic question that maybe you can't answer for me, but we're trying to write about the computing and computer science programs at the Foundation. Do you think that this study should be broadened to a point that I talk about those programs in computational mathematics, numerical fluid dynamics and such as part of the overall study, or do they have a separate life of their own over the course of the last 30 years at the Foundation?

THALER: I think they're really different. The Foundation, I think, handled computation very wisely back in the

1960s. They were some thoughtful people back then. They put a lot of computing out into universities in the 1960s. Some people thought too much. I probably would have thought to much. John Pasta didn't think too much. And as a result of pouring all this equipment out the generation of students trained got pretty damn good, and computer science in this country got pretty good. And so the computer became an important tool in universities and computer science departments grew up. What's happened in the last ten years is that it has become increasingly clear that the computer is an essential tool in every part of science, and in fact that's too narrow a statement. The computer is increasingly important in everything that we do and it's going to get better or worse depending on what your stock portfolio looks like and what your view of the world is. And I really think that if you try to broaden the impact of the computer, I think you have got enough on your plate trying to understand the development of the computer science community. We really do have a different view of the world we here in mathematics and them there in computer science.

ASPRAY: Do you want to spend a couple of minutes reiterating what that it is? You commented on it already, but if you can pull it together for me that would be helpful.

THALER: Theoretical computer science and mathematics are really very different. There are some overlaps, but the communities are different. What seems to be important is different. I have some questions about the ultimate value of the theory of computer science. That's another story. It's just not clear to me that it really relates to the needs of technology, and it's also not clear to me that it's an intellectual discipline in its own right. I am painting with a very, very broad brush, but in fact, the kinds of things that we look at in mathematics and the kinds of things that they look at in the field of computer science seem to have very little overlap. An American and symbolic computation program is perhaps closest to our thoughts and needs. Software engineering really has nothing to do with anything that we do, except for a set of measures I can't think of anything. The same is true with the architecture program. There are some architecture problems that are mathematical in nature very, very few, as we look at them. The biggest controversy that occurred in the history of the division, and a lot of bitterness, was when the division of mathematical and computer science was split into a division of mathematics and a division of computer science.

ASPRAY: I haven't heard very much about that. Do you want to comment more on that?

THALER: Oh, that was very interesting. And it was a bitter fight. It was Jim Infante's major loss, although he remembers it differently. He tried very hard to keep the divisions together. He thought that it would have been good for both divisions to keep them together, and they did split for a variety of reasons. The cultures are very different; the award structures were very different. The review patterns were very different. Pasta kept the divisions together, and in fact, Pasta was just wonderful. Pasta was one of the finest people I ever worked for maybe the finest. He was deep, and he was thoughtful, and he sensitive, and he understood both cultures really very well. And he used to say that there was nothing as hard to sign as a declination in mathematics, except for possibly an award in computer science...

ASPRAY: [laugh]

THALER: Because the declinations in mathematics had really very strong ratings in many, many cases. And in computer science the awards had a couple of "very goods" and a couple of "poors" frequently, traditionally. It made a lot of sense too. I mean, there's nothing wrong with that it's a new discipline...

ASPRAY: Not as much consensus about?

THALER: Nowhere nearly the consensus. Everybody thinks he knows which way to go, and, "You're wrong and I'm right." And in mathematics people had been working on (and this is an overstatement) the same kinds of problems for a couple hundred years and they know what's good and they know what's bad and there is a very strong degree on consensus. Very different different sociologies, different needs, different styles different. The computer scientists thought more like engineers in many instances. Mathematicians thought more. In fact, Kent Curtis wanted very much to leave the division of mathematics and physical sciences and move to the division of engineering where he thought he would get a fairer shake. Computer science for a long time was viewed by the physicists and chemists in control as less computer science and computer center. The purpose of the computer scientist was to provide code writers. So the division was split. It was Marcel Barden's decision. Marcel Barden was then the division director... the acting assistant director for mathematical and physical science. This was, I guess, in 1982 or 1983 or something like that. It was very important very important that that happened.

ASPRAY: How do you see the changes since then? What kinds of things have happened because of that split, especially its effect on computing rather than on the mathematical side?

THALER: Computing has gotten an identity onto itself, mostly because of the new directorate and because of Erich Bloch's view of the world. That's hard to answer. One of the biggest differences was the big infrastructure programs, which were in size, which were in computer research. It's quite possible that if the divisions had been kept together we would have a program like that in mathematics. I think those programs have been very good. We still might have that program in mathematics. I am not sure how different things are. Size is more visibility. Computer science has more visibility. They have got more headaches, too. The funniest part of that new directorate is the fact that there are these two pieces in it. There are the traditional NSF pieces three directorates, IRIS and CCR and MIPS [?], that are just bread and butter get proposal, make grants. And then you've got these other two divisions that in fact provide service.

ASPRAY: The other two?

THALER: Networking and the computer centers. And the networking division is a flaky division because people don't really understand what the networking division does and what a network is. The Advanced Scientific Computing Division has as its clientele certainly not computer scientists not really. Really, physicists and chemists. And so you have got this competition inside the division not competition, but these two different kinds of missions. One is traditional supportive research; the other is to provide service centers and networking, and that's weird. I would like to think that the functionality of computer research wouldn't have been any different if it hadn't been split. But it was just an administrative decision, an idea to provide identity to mathematics on its own. Let me also say something else. The basic unit in the Foundation is the division. And many of us thought that it was inappropriate for mathematics and computer science not to have separate identities as basic units. And I think that was probably the best reason for the split. Mathematics and computer science are different. As a matter of fact, within the mathematical sciences statistics and algebra are different. There's a certain point at which you have to stop doing the cut. But in fact, there are no departments of mathematics and computer science other than at [extremely small institutions].

ASPRAY: Do you expect that had there not been this division between the two that your current program would have had a different character than it currently has? This is a counterfactual one.

THALER: That's interesting. The bridges that I would have had then I have now in some sense. I talk to Abdali a lot. It's no harder to split funds a proposal across directorates than within the division. It really isn't. The only difference is one of geography and I have to walk across the hall. I think there's really no difference. I think one of the good things about the Foundation is the level of interaction among program directors with similar interests is...

ASPRAY: Another general question about computational mathematics history. How large has the Foundation's role been in its support over a long period of time? Who are the other players, and who are the other players today? Did DARPA, NASA, ONR, and so on?

THALER: Very interesting question. We have always cooperated with the military agencies. DARPA has been strange in the sense that DARPA never had a mathematics program until 1986. They had a funny program in 1986 and they were sort of looking for things to do to support. We did not work with DARPA at all until very recently. DARPA is a very different agency than NSF. In fact, the easiest way to compare them is by talking about institutional memory. AT NSF institutional memory is very, very important. At DARPA there isn't and shouldn't be institutional memory. Right now there is a strong person in math at DARPA, and he's very smart. And he has a good view of the world, and I work with him very closely. And so does the division director, Judy Summond [?]. And relations are healthy; we split fund a lot. And we talk a lot and we pick each other's brains and I use him a lot as much as I can. DoD agencies, some. They do traditional work. I don't mean to be pejorative in any sense. They do the kinds of work that is of interest to their community. They know what they want, and so do we, by and large.

ASPRAY: Which agencies are these?

THALER: Air Force, ONR, and to some extent, the Army. Hardly anything at all with NASA.

ASPRAY: Anything with NIH?

THALER: Nowhere nearly enough. That's changing. We have some work in computational biology. In fact, there's a half person who is the acting program director of computational biology, who is a statistician by training. We expect to see a major initiative in computational biology in 1992. Another one of my biggest and most exciting grants (it's just a piece of it) is a group in Southern California at USC, in fact. Some incredibly exciting work. Real mathematicians, real mathematical statistics on this DNA sequencing stuff and on population biology and all sorts of stuff. Biology has become a very mathematical discipline, whether the guys running it know it or not. Biology will be the science of the next century. They're flooded by data. They know they're flooded by data. They have got the ability to make some incredibly profound contributions. They really need work done by computer scientists. It will probably wind up done by mathematicians, because... They don't know how to deal with data structures, data bases, and the computer scientists tend not to be looking at the data base problem in the way that helps them.

[INTERRUPTION]

It's exciting; it's really exciting. Judy says that the biologists now are at the position where the physicists were at the turn of the century. They're just starting to understand the power of the mathematical tools that could be available to make real penetration into their problems. NIH is a little slow. They're coming along. Five years ago NIH would never provide a computer workstation as part of an NIH grant. Now they do it routinely. Watson is there running the genome [?] project. Things are changing.

[INTERRUPTION]

ASPRAY: I guess they supported some research in artificial intelligence for some time. Feigenbaum and some of the others?

THALER: Don't ask me what I think of that stuff. [laughter]

ASPRAY: Let me turn towards closure. Are there some general themes or general impressions that you would like me to go away with about NSF computing, or more narrowly, in your own areas of participation?

THALER: The role of computation in science is only now starting to be appreciated. You ain't seen nothing yet. The excitement of what's available at the low end. People always talk about the supercomputers, and they're exciting, but what you can buy now for $3000. The Next announcement have you seen that? The new Next?

ASPRAY: Yes, I have.

THALER: The academic price now of their low-end unit is $3000, and for that $3000 you get a 68040. That's a 14 bit machine with a very high resolution terminal... screen with 8 meg of memory, upgradable to 32. And those are Sims [?] so they don't cost anything with all kinds of software that you may or may not want, but it's thrown in Mathematica, and a reasonable word processor and a really nice spread sheet, just thrown in, with plug in Ethernet, with plug in Twisted Pair. $3000, and that's a real machine. Now, that's going to have impact on the way everybody does everything. By the way, the NRI stuff is really interesting the stuff that NCRI is doing with Bob Kahn and his operation. I want to come back to that. That's a major watershed. NSF's computer support in the past has been instrumental and pivotal in positioning us where we are today with the strength of our conscious and stuff like that. I am not all that sanguine about the impact of the current activity... research activity that NSF is doing. It's not clear to me how closely related it is to industry. You might want to talk to Gordon Bell, who has strong and bizarre opinions about everything.

ASPRAY: I have tangled with him before.

THALER: Well, he's crazy, but he's a genius. Yes, so you sort of wonder about a lot of this academic stuff and how inbred it is. I worry about that. This NCRI grant to NRI... The networking grant to NRI is kind of weird, because it's a big grant of NSF money to another group that's using it as a regranting. And it's a funny kind of thing inside the Foundation, and it surprised me as an old-timer about how easy that sailed through. Couldn't have done it ten years ago, and I am not sure what a good idea it is, except, on the other hand, Bob Kahn is probably a lot better than the people we have here. Kahn is very important to talk to in any history of computer science.

ASPRAY: I have talked to him.

THALER: DARPA arguably has had a much more important role in the development of computer science than NSF. I don't really know how much of what we did was catch up and how much of what we did in computer science was influenced by DARPA. I do know that relations between DARPA and NSF and computer science have always been weird and bad. It's just institutional lack of match.

ASPRAY: Are there areas you see that the Foundation was really the pusher of the area, as opposed to DARPA, though? I mean, I think theoretical computer science and I think Foundation support rather than DARPA support, whereas when I think of the early networking I think of DARPA.

THALER: I am not a computer scientist, and I really can't think of anything. Think of the really important stuff; think of Moses [?] that's DARPA.

TAPE 1/SIDE 2

THALER: In total, the biggest computer science departments... NSF gave very little money to Berkeley and to MIT and to Carnegie Mellon and to whatever the fourth place was Stanford, I guess. If you talk to people you get the impression that the leadership work was done at those four places. Now, it's changing, because of the change in DARPA and because of the change in what DARPA is doing.

ASPRAY: I think of places like Cornell where the support was more NSF than DARPA.

THALER: It's very different. Hopcroft and the theoretical stuff; that's right. Partly because NSF couldn't throw the kinds of dollars into a university that DARPA could. Did you talk to Bill Wolf?

ASPRAY: Not yet.

THALER: Okay, you need to talk to Bill Wolf. SIIS has a new directorate. It's had two assistant directors. Gordon started it. A funny personality just tied things together, had very strong opinions. It's still not clear where the supercomputers are going to go and how much impact they had. Bill came, tried to push the... had three things on his agenda and got none of them.

ASPRAY: The three were?

THALER: Oh, let's see. He had a major initiative in software; he had a major initiative in real software. And it was stepping back from the problem and trying to have a theoretical underpinning to software. That was one. One was to do something about prototyping and instrumentation, and have a major program in that. And the third was to push this collaboration technology business. None of them got anywhere. And furthermore, he lost a fair amount of money in the program. When ETA was closed down he didn't get the money. So, it's a young directorate, and the program staff are of variable quality, that's true everywhere. And it still needs focusing and a real strong initiative of some sort. The collaboration technology, as I said, could be very exciting. I wish it were.

ASPRAY: Thank you.

END OF INTERVIEW