

An Interview with

W. RICHARDS ADRION

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Abstract

Adrion was program director of Theoretical Computer Science for the National Science Foundation (NSF) from 1976-1978. After a brief period with the National Bureau of Standards, Adrion returned to NSF in 1980 as program director of Special Projects in Computer Science. From 1982 until 1985 he was program director of Coordinated Experimental Research, and then served as deputy division director of Computer Research. For a short time in 1986 he was named chief scientist for CISE, and then left NSF to join the faculty of the University of Massachusetts in Amherst. Adrion discusses the development of NSF programs relating to computer science, particularly those programs in which he worked. He begins by describing NSF's funding of cryptography during 1976-1978 and its relation to the National Security Agency. He gives a brief history of theoretical computer science in the United States and NSF's role in funding that area during the 1970s and 1980s, as well as a description of the leading academic centers and personalities. Adrion recounts his work with the Coordinated Experimental Research program, which grew out of a concern to retain good faculty and promote experimental research at academic institutions. Other areas discussed include computer networks, NSF's support of CSNET, the role of Kent Curtis in NSF, and the relationship between DARPA and NSF funding. The interview concludes with comments about the position of chief scientist and Adrion's decision to leave NSF.

W. RICHARDS ADRION INTERVIEW

DATE: 29 October 1990

INTERVIEWER: William Aspray

LOCATION: Amherst, MA

ASPRAY: Could you begin by telling me very briefly about your education and your position before you came to the Foundation?

ADRION: Well, I got my undergraduate degree and my master's degree at Cornell in electrical engineering. Then I got my doctorate from Texas in electrical engineering. I stayed on the faculty at Texas for a while and then moved on to the electrical and computer engineering department at Oregon State, where I was before I went to NSF.

ASPRAY: Now, is the fact that you got your degrees from electrical engineering departments representative of your interests in computing on the engineering side?

ADRION: I guess that's true.

ASPRAY: I will come back to this question a little later, because it seems to me that you were a program officer for theoretical computer science, which seems at the opposite end of the spectrum in some ways.

ADRION: Not necessarily. My dissertation was really in theoretical computer science. In the early days there were two main conferences in theoretical computer science. One of them is now called FOX and the other one is called STOC, the symposium on the theory of computing. It used to be called a SWAT conference - switching and automata theory conference. Switching and automata theory were the parts of theoretical computer science that used to be in engineering departments as well as math departments, so it's not unusual that I ended up in the theory program.

ASPRAY: What was it that attracted you to the Foundation?

ADRION: I can't remember. I guess I had a colleague at Oregon State who was a chemical engineer who had been in Washington for a year in the chemical engineering program. I had visited him there and it seemed like an interesting thing to do for a year.

ASPRAY: And who recruited you?

ADRION: Kent Curtis, who was, I guess, the section head in those days.

ASPRAY: You were recruited to come in as a rotator in theoretical computer science, is that correct?

ADRION: Right, well, there was some discussion between software engineering, which had gotten to be my interest, and theoretical computer science. In the end we decided on theoretical computer science, although my offer [was for] program director of software engineering, if I remember. So, yes, he and I discussed it.

ASPRAY: Why don't we turn to the theoretical computer science program in the Foundation for a couple minutes. Tell me how you found the program when you arrived. How vital was it? What kinds of things were being promoted to the extent that the Foundation ever promotes any specific programs?

ADRION: You know, theory is the oldest program in some sense. I mean, besides supporting computing facilities, the real first research program at NSF was in theory. This was back before there was really a division, when there was an office of computing activities. (I had to track the history of NSF when I was there for later incarnations of my work, so I had to go back through all of their records.) So theory program had been around, and it essentially shedded things as it went through history. I mean, what it is now is only a fraction of what it was in 1976 when I went there. It was all there was in 1968, or whatever.

ASPRAY: A spin-off of other programs within computing, or outside of computing, or outside the Foundation?

ADRION: Oh, no, other programs within the Foundation. In fact, let me tell you what happened. In 1976 when I got there the program essentially covered complexity and I guess a little bit of what's left of automata. But it also included a whole lot of language aspects of theories - semantics and so forth. That in a sense drifted off into the programming language program within the CCR division, as it's called now. Also, a lot of the theorem-proving work, which actually bounced around a lot of programs, and I not even sure where it's located now. I think it's being moved in some new restructuring which DeMillo is doing. But it was in that program. Also, a lot of machine-learning part of AI was in the theory program and that got moved out into Intelligence Systems. All of the numerical and symbolic computing was in the theory program when I started it. I think the person who followed me didn't like numerical analysis so he moved it out, and it was moved into software engineering. Now it's its own separate program.

Basically what has happened to theory is that it's grown in a couple of different ways. First of all, as the field has expanded in the areas that the theorists look at has grown, then it has been partitioned up. Now, some of the things that used to be in there that had to do with parallel architectures have been moved out of theory and into sort of a theoretical architecture program. There's also a sort of systems architecture program at NSF. But as kinds of topics theorists look at have grown, things have been pieced up. Otherwise that program would now be the size of one of the divisions. The other part of it is that theorists quite often tend to move away from theory as they move through their careers and into more systems areas. But they still do theory research in conjunction with their systems research. They still put out students and they still look for funding. In reaction to this there is created sort of bridge programs and bridge systems area into the theory program.

I guess the hottest theory in 1976 was... complexity theory was still rolling along. There was a lot of work that began on cryptography about that time. Well, I don't know if you have tracked down all the controversy that arose at that time over cryptography within NSF.

ASPRAY: No, I haven't. I mean, I have heard allusions to that from various people, but if you can give me a summary of it I would appreciate it.

ADRION: Let me see I can give you a two-bit summary of it. About 1977 or 1978 Marty Hellman, who is really in communications - EE-style communications - at Stanford, and a student of his, Diffy, I guess (I have forgotten his name; it's been so long) started thinking hard about both the DES, the digital encryption standard, which the National Bureau of Standards had issued about that time, thinking about it and whether it was breakable. Could you design an architecture that would actually break it? They also started thinking about other aspects of cryptography. They came to believe that IBM, who had been involved in the DES development for NBS, had left a "trap door" open that allowed the National Security Agency to break the code, although other people couldn't. So they made this claim. They also pioneered the idea of a one-way function, a function that was computationally easy to apply in one direction but very difficult to reverse. So that if you took a piece of text and you applied the function to the text it would be computationally easy to create the encrypted text but computationally very difficult to unwind unless you had some sort of trick - trap door - to do it. While they came up with some ideas about how the trap door functions, they didn't really come up with a good one.

A group at MIT - Ron Rivest and Ali Shamir, somebody else whose name escapes me, thought of an algorithm that would work that way. It was an algorithm that used composite numbers to encrypt a message. It was very hard to determine what those composite numbers were. But putting the composite together and using it to encrypt it was pretty easy. What happened was that basically there was a big to-do in the press, and an attempt by unknown parties to stop the publication of some of its work. So I think actually secrecy orders got put on a number of the researchers in computing - Hellman and Rivest, another person who later became program director at NSF, George DeVada at Wisconsin, because he had been working on some things that had to do with cryptography. All those were eventually lifted. There was an attempt by the National Security Agency to demand pre-publication or review of technical reports in this area. All of that got settled out much to the credit of Bobby Inman, who was at the time the head of the NSA and who sort of figured you shouldn't fight academics but you should try to get them to cooperate with you. He came to the NSF, and I guess there was a time in which there was no director at NSF. I know Don Langenberg was involved in the discussion and he was the deputy director at the time; he's now chancellor at Maryland. I think Dick Atkinson had left for San Diego or wherever he went about that time.

Essentially NSF agreed to help the National Security Agency set up a program in theoretical computer science research, an open program with which I was partly involved in helping out. It took two or three years longer. I had left the Foundation and came back before that program ever got going over at the National Security Agency, but they now have an open theoretical computer science and mathematics program that was essentially spun out of some of the cryptography research at the NSF. So that was pretty much what was exciting in 1976 to 1978. I left in 1978, went out to the National Bureau of Standards.

ASPRAY: To do what?

ADRION: To run the software engineering program out there.

ASPRAY: Why did you decide to do that?

ADRION: Well, I was a rotator with a three-year appointment, and I really had gotten very interested in software engineering. At that time Ruth Davis had just left NBS and they had started up a huge new research program and they were attracting a lot of Ph.D.s to work on the program in what was called the Institute for Computer Science and Technology. It was handy; I wouldn't have to sell my house and move back to Oregon. So I resigned my job at Oregon State and I went out to NBS. I lasted there about 18 months and then I was back at the NSF again. A lot of things went wrong at the Bureau with management. I think there were a hundred thirty-some odd people who were working there the year after I got there. Less than 20 survived two years after that. So all their grand plans in 1978 seemed to have fallen apart. So anyway, somewhere after I had been there about a year and a half and was sort of beginning to poke around at other possibilities, looking at going back to universities and so forth, Kent Curtis called me up and said he had this new program that he wanted to start up that came to be known as the Coordinated Experimental Research Program.

ASPRAY: Before you go on to that, can I ask you a few more questions about theoretical computer science?

ADRION: Sure.

ASPRAY: Can you give me an overview, since you were at least watching from the sidelines when you weren't directly involved with the program, of the history of theoretical computer science and the NSF's role over the last 10 to 15 years? What have been the trends? Who have been the half-dozen major people that they have supported? If one were going to take samples of five or six people's research to highlight in a report, who would you identify?

ADRION: Okay, that's not too hard. It's hard to come up with five or six; I would have to sit and think.

ASPRAY: If it's two or three that's okay but if it's ten or twenty that's too much.

ADRION: That's too many. So I can give you some sort of background, the program again started in... I think it was in the late 1960s but I can't really remember when it started. I know that Curtis was managing all of that activity for a while and then maybe Tom Keenan after that, then Bruce Barnes, and then me. I can sort of account for everybody after that too, but I am never quite sure of when programs actually came into existence. There weren't very many computer science departments. The research programs in computer science at NSF developed sort of in parallel with their computer center programs. Back in the 1950s through the 1960s they were funding all these... you know, buying everybody a campus computer. They bought U Mass one; they bought everybody one.

When academic programs were founded in the mid-1960s, some of them had clearly a strong theory component to them or theory emphasis in them. Programs [that come to mind] are the Berkeley program, which really grew out of the math department, in which I guess Dick Karp is probably the best known person (although he was pretty young in those days), and the Cornell program which was founded in 1966, I guess, when Hartmannis came from General Electric and Pat Fisher. There were three people who came in, but both Pat Fisher and Hartmannis were clearly theorists. Then Hopcroft was later recruited to join Hartmannis at Cornell. About the same time, I guess, MIT started ginning up the program. In theory, those have been the main axes of theory for years; that out of the math program

at MIT... and I am trying to think of who the sort of earliest theorist at MIT was. I tend to think of Albert Myer as the grand old man of theory at MIT and Albert is not very old. Of course, then, nobody in the field is very old. Let's see, Albert got his degree at Harvard. Pat Fisher may have been at MIT. I can't really remember the history of the MIT, because they're really math theorists and I am an electrical engineering theorist. I guess basically the Berkeley and MIT crowds grew out of mathematics, where Cornell and Illinois, the other two that I would think of, grew out of engineering. Since I was at Cornell a little before they founded the department, I am familiar with that and the Illinois people. But Karp is also an engineer as well as a mathematician, so I know their work better than the MIT track.

ASPRAY: What characterizes the difference between the kinds of research and the way they approach problems - mathematics versus engineering?

ADRION: Well, a lot of the people who came in from the electrical engineering side were people whose backgrounds were in communications and control theory - Shannon, information theory. The control theorists started picking up on automata theory when it sort of grew out of Turing's work and so forth. It grew out of the math department, but they saw its application to digital circuits. So their interest was in automata and how these translated into real asynchronous and synchronous sequential machines, and how it translated into logic circuits and so forth. The math people tended to look at more the language aspects. I mean, there is a duality in theory between language approach to representing computation and the machine approach. The mathematicians seemed more attracted to the language and tended to start building hierarchies of capabilities of languages. It was the crossover there with the linguistics people, with Norm Chomsky and people like that.

So anyway, I would say, if you are going to try to track down where the earliest research activities were, they would be the MIT group; the Cornell Hopcroft, Hartmannis; Dave Muller at Illinois (I guess Dave was pretty young in those days, but he was there); Karp and Manuel Blum at... and Mike Harrison, I guess, who I don't think was much of a theorist these days as I once did. Those are the centers that I would look at. I mean, there were certainly people elsewhere, but those were the centers of work. A lot of the Ph.D.s that were produced in the early 1960s then sort of founded more and more departments. So you can trace a lot back to MIT, Illinois, Cornell, and Berkeley, especially in

the theory area. So they had a lot of influence on the field, the people who were there, and their own research interests, because their children sort of spread out around the world. You could see camps of Hartmannis people and camps of Albert Myer-style people. Myer was very interested in circuit complexity and hierarchies of complexity classes. Essentially all of his descendants spread out all over universities looking at that kind of thing. Again, Hartmannis was sort of interested in the p=np problem, and complexity classes, but more language complexity classes. He generated generations of students who spread around. So that's where I would go. Karp is still extremely active and so is Albert Myer. Manuel Blum never appeared to be active. He always just kept turning out incredibly good students and read very few papers himself. He's very into cryptography - sort of late in the game. Muller is very much beyond sort of retirement age, I think. Hartmannis is the grand old man and Hopcroft is a roboticist nowadays. People change as time goes on.

ASPRAY: Is there a two-year set of problems in different periods of time that are the key ones, or is the field just expanding?

ADRION: I think there seems to be areas that are hot, and again, I haven't followed theory in recent years very closely because it's gotten, as they would say in the field, almost too technical. A lot of the results in recent years seem to be structural in almost a pure mathematics sense and less of sort of general computer science interest. For example, the NSF changed its philosophy about funding theoretical computer science several times. That has had a great deal of impact on the field, because it's really the only source of funding for theoretical computer science. Up through my tenure in that position, really, the field was sort of expanding, but I guess there seemed to be plenty of money around and people could get funding who were doing good work. The kinds of things people looked at wasn't the result of deliberate decision on the part of program managers, whereas I think George DeVida, who followed me in the job, took a very applications approach to funding. He liked to see theory. He liked to fund things that were in newer application domains from a theoretical perspective. In other words, he liked the cryptography stuff that he had kind of started up on his own. He liked the parallel architectures, and he liked theory and databases, and he liked theory and programming languages, and so on, distributed systems theory. In order to fund theoretical aspects of all these other areas, he in some sense cut down the funding for main line, what I always refer to as "FOX

and STOC" theory. If you define theory by what gets published at the FOX and STOC conference, which has changed a lot over 25 or 30 years, is main hard-core theory. DeVida broadened the kind of stuff that was being funded and cut down a great deal on the funding for core computer systems - not a great deal, but he tightened it up. The person who followed him in that job was Myra Glattner, and Myra had a sort of attitude that it was better to fund the maximum number of people with a minimum amount of money. So she had a success ratio of probably 80% the year that she ran the program, and she funded almost no graduate students; times were very tight. I'm trying to remember... I think Triniioski [?] actually came in after that.

By the way, each of these people who came in tended to slough off parts of theory. I mean, DeVida was the one who unloaded all the numerical computing. What happened there was that when numerical computing, like when I had it and when Barnes had it and so forth, was in with theory, one tended to emphasize the numerical computing that had a great deal of overlap with STOC and FOX theory. So a lot that had to do with sort of concrete complexity results in the numerical domain, as opposed to numerical analysis in the sort of traditional domain. Symbolic computing I guess was funded over in some other program at that time - it was brought together in numerical computing, although there's a little bit of symbolic computing funded in theory. But DeVida dumped that. When he dumped that, the people who did work in the concrete complexity within numerical computing area sort of got lost, and all the optimization people and everything else that were trying to get funding other than numerical were now in numerical computing. So that had some effect on the kind of work that went along that boundary. When he left and Myra came in and she sort of returned to core computer sciences. She was in the language, semantics kind of area herself, in a lot of the things that got started up in architecture, database, operating systems, and distributed computing theory kind of got dumped. I remember Curtis having (I wasn't at the Foundation at the time, but I still kept close tabs on it) spent a lot of time trying to get other program managers in architecture and systems and software and so forth to pick up these programs that had been started up during DeVida's period. But they didn't really like theory very much. I mean, they really wanted to support experimental work.

ASPRAY: So what's the upshot?

ADRION: Well, I think eventually Curtis beat them into submission and more of that work got funded. You know, the proposals from theorists tend to be better thought out and better designed research proposals than experimental. Experimental people in computer science tend to write terrible proposals.

ASPRAY: Why do you suspect that is?

ADRION: Experimental computer science grew out of Project MAC and many of these sort of large, DARPA-funded things where everybody ran around in hat [?] code and built things. They said, "I think this will work. I'll build it; send me some money to do it," and never really had to think of it on a small scale, small science NSF style, "What am I trying to prove here? What kind of experiment am I going to do to show that it's true or not true, and how am I going to validate the results of my experiment?" Computer scientists who work in the systems area are only recently beginning to understand how to write a proposal. There's always been this real gulf between the systems people, who were largely funded by the Defense Department, and the theorists, who were largely funded by NSF. The programs at NSF that were funding systems people tended to have lousy reviews but they still aggressively were funding people to try to get NSF a role to play, which I think it had to, because DARPA is only interested in somewhat narrow domains and narrow focused foci.

So you basically had the situation where theory-style distributed systems proposals got lumped in with regular systems -style distributed systems proposals and the theory ones did better. I mean, their reviews were higher because they were cleaner and better-written proposals. So that had some effect. Trinioski brought back in some of those things because he felt they weren't getting treated well by all these systems characters, and he wanted to make sure theory was treated well. He [developed] the program a lot while he was there, because he was, I think, a very good champion for the quality of the work and willing to go poke around in other people's programs and argue that his program was by and far stronger and had better people in it than architectures or software or whatever. He also started trying to develop centers of excellence, where he would do more than fund the usual theory grant, which has always [consisted of] one student or no students and summer support and money to go to Fox. He started putting post-doc support and two or three graduate students in a group where he saw critical mass, like at MIT, or Berkeley,

or Yale, which is a program that he [helped develop] but which has since fallen apart.

After that there have been a series of short-term program managers without a lot of imagination in theory. I mean they were just there for a short time. It takes more than a year to figure out how to play the game. I think they have been just making decisions. The success ratio was way down and the number of theorists is way up, so they just tried to figure out how they could fund. There's really almost no discretion in there. I mean, no action taken by program managers anymore because they don't have enough money to do it. Basically after they finish funding all the ones that have excellence they are out of money. So their only discretion is whether they give Karp \$150,000 or \$140,000 and save \$10,000 for something else. So they don't really have time to decide that they ought to shove some money into database theory, or whatever. The other thing that has happened in recent years is that DARPA has decided it will want to play in the theory game, and they had these joint DARPA-NSF theory programs, which have been more or less successful.

ASPRAY: Does DARPA fund on its own also?

ADRION: Theory, no. A FOX and STOC theorist has two places to go. Basically, you can go to NSF or you can go to the NSA crypto program, and then they're really only interested in number theory kinds of things and not interested in broad theory. So if you're a computational complexity person, unless it has to do with cryptography, you really have only got NSF to go to.

ASPRAY: Let's turn to CER. So you came back in I guess it was 1980.

ADRION: 1980 - January, 1980.

ASPRAY: And CER was a new program that was being set up at the time.

ADRION: Right, it didn't exist, I guess, officially until fiscal 1981. I should try to remember. I came back and I took

over what is called a Special Projects program, which had been run by Frederick Weingarten, who had left in the middle of the night one night... [laugh]

ASPRAY: Why was that?

ADRION: I don't know. I think he just got tired of NSF. He had been there on and off for quite a bit of his career and he wanted to do something else.

ASPRAY: He went over to OTA, didn't he?

ADRION: Well, actually, he didn't immediately. He went to CMU for a while and he taught there, and then he acted as a consultant and he did some contracts for OTA, and then he went to OTA. He now works for the Computing Research Association, so I see him quite often, since I am on the Board of Directors of the Computing Research Association. Anyway, Rick disappeared. I guess he sort of left Kent a note and said, "I am going" in the middle of the night one night. He was gone. So I came back to take over the CER program, but I also inherited Special Projects.

ASPRAY: What did Special Projects involve? When I think of Weingarten's program I think of things like security and fraud and social aspects.

ADRION: That's what it had. It had most of the database research that NSF was funding at that time, which I found surprising. It had all of the social aspects that computing worked with that Rick was fond of promoting. It had, you know, transported data flow and privacy and security. It did also have some of the cryptography stuff. It had networking. It was sort of funny. The division was large at one time when it sort of had an applications chunk and a research chunk. And then it got collapsed down. When it got collapsed down, Weingarten inherited a lot of this and that. There used to be a program that was involved in, I guess, looking at networking. It really involved - I can't remember her name - a sociologist, Absalla [?], who hangs out with Murry Turroff (I can't remember her name) at New Jersey Institute of Technology. Anyway, they did a lot of networking from the aspects of computer conferencing

and electronic mail, and how people were going to interact. Anyway, Roxanne Starr-Helts. She is really a sociologist that tries to look at how people interact. Networking was there, a lot of database stuff, which surprised me. All the social aspects of the Rob Kling [?] stuff. Then this business on fraud and privacy and all of that, but that was kind of winding down, because that was his interest. I never really got any proposals in that area. I got a lot of proposals from the Rob Kling crowd of people, which, in substance, I was brought in to shut down - I hate to say that - not entirely, but to cut it way down in terms of priorities. And the networking stuff actually, CSnet, was part of the Special Projects.

Kent had had this idea that had been developed through the advisory committees that to deal with the crisis in computer science meant that you had to attack it along a number of avenues. One was this infrastructure program - the CER program. Another was the network - extending ARPANET-like technology to all of the computer science departments. The third was some programs to support young investigators. There was a short-lived program called the New Experimental Method. I think Young was not allowed. The president got away with calling it PYI, but it had some other. We gave three awards out and that was the end of the program. There was also a post-doc program at that time, in which we gave out one award, and it died. So all that was part of Special Projects.

He had already solicited proposals for CER through a "Dear colleague" letter. So when I arrived at NSF, there were eight proposals there, maybe - seven or eight - somewhere in that number.

There had been no program announcement or anything, just this letter that he had sent out to departments. Most of the proposals were sort of slapped together. There was one proposal that was extremely well-written. That was the Washington proposal. Apparently, they had sort of gotten wind of the fact that there would be such a program several years before, and they had worked on it.

ASPRAY: Can you tell me the history of the program, why it came about, who had pushed for it, who had initiated it, and so on?

ADRION: I think there were a lot of articles written at the time by various people in the field - the Snowbird meetings - who were concerned about the issue of losing faculty, not being able to get experimental faculty. The fact that basically only MIT, CMU and Stanford had the kind of facilities and support which allowed them to do research that was competitive with IBM and Xerox and DEC and so forth, research labs. In fact, DEC didn't have any research labs in those days. It's not so much the issue of them to be competitive. It's the fact that if you wanted to bring along a new generation of students who had been exposed to systems. You had to be able to do that research on campus. It had traditionally been done in computer centers. That's why I had to go back through the history of all the NSF support to try to see where the research programs had come out of the computer center support programs - back in the 1950s and early 1960s - also, to try to track whether or not NSF had done anything that had later been exploited by DARPA with more money. Indeed, quite a number of things NSF had funded for \$20,000 or something, an idea which the person had then taken off to DARPA...

TAPE 1/SIDE 2

ADRION: From my own perspective, I think the CER Program came about because Kent Curtis had heard it enough since 1972... so, almost a ten year period then that this was a problem, that there wasn't the kinds of facilities to do experimental research beyond the big three.

ASPRAY: How far was this program intending to reach down? To the next 15 schools? Next 50 schools?

ADRION: Let's see if I can remember. In 1979 or so when the budget arguments were made, it was called the Large-Scale Experimental Research Program (LSER). I guess they were talking about up to 30 or so centers. I always think it's important when I try to tell someone the history of the CER program to go back to that time, because I now chair the committee that oversees the CER program. Small world. From my perspective, the current CER or CIR, as it's now called is a long way from the ideas that were in the LSER program back in 1979. There the idea was more similar to S&T centers, the engineering research centers, than what CER turned out to be. And I can give you some reasons why that happened.

The original idea was to fund large research projects like MIT had - you know, Project MAC and so forth - at places other than MIT and Stanford and CMU. MIT, Stanford, and CMU, it has always been said, volunteered not to compete in this program. That's the legend. I believe it, but I think they were volunteered by essentially the representatives on whatever it was called in those days... the computer science advisory panel. But anyway, the idea wasn't just infrastructure, just buying equipment and paying for some support staff. But these were to be Project MAC style - some of them. Some of them were going to be straight infrastructure centers, but some were going to be centered around a particularly large research project or another operating systems research project.

So the first n proposals that came into the program, where n is around six or seven or eight (I should remember), only the Washington proposal was successful, and that was a research project. It had graduate students supported on it; it had post-docs; besides equipment and so forth. They were going to build an operating system, which they partly succeeded at, I guess, over five years.

That was the last project of that kind that ever succeeded. There were certainly other attempts, and it certainly became clear that what you had to do was propose a research project. The main guide to success was propose a research project that looks technically extremely exciting and pulls people away from what they were doing into a new experimental area. But you don't ask for any funding for that; you just ask for equipment and support staff. Partly that came about because of the review committees. As I said, in the first set of proposals there was one good proposal, one sort of half-good proposal, and the rest of them were, you know, "Mary and Harry, send me your stuff. I will staple it together, and we will mail it in to Kent and see if we can get this grant." They were just embarrassingly bad proposals [although they involved] some very good people. The next year people had time to write good proposals. There were 20, 25 proposals and many of them very good. But two things happened. One, there were so many good proposals, and, although there was a considerable chunk of money in the second year, there wasn't enough money to go over all the really good proposals. And among the very highest rated proposals were two proposals that were entirely infrastructure. Illinois was one of them, in which all they said was, "We do all this good stuff and basically we don't have enough equipment to do it with. So send us equipment and support staff." But

there was no unifying theme. They weren't going to do anything different. All their equipment was outmoded. DARPA had sort of pulled back on their support of Illinois. So they became a model for sort of a general, "Just send us equipment" grant.

Cornell was the same thing. I mean, it's different. "We are a bunch of theorists and we would like to try experimental things. Send us some equipment." But they didn't ask for any support. So of those two proposals, I guess Illinois wasn't looked at as the strongest proposal. But everybody liked the Cornell proposal. In fact, the Yale, Wisconsin, and Cornell proposals, which were funded that year, looked good to the panel because they were largely theorists who had been converted over into being experimental people, and here looked like a chance. In fact, they had all had a track record of doing mostly theoretical work, getting some equipment from NSF from the equipment program, doing a little bit of experimental work, getting some more equipment. And then they were in for a CER. It looked like another chunk of equipment. In fact, it was extremely successful for those departments. Well, Yale was funny, but Cornell and Wisconsin are both widely recognized as very strong systems groups now. They bear almost no resemblance to what they looked like in 1981. So it was very successful.

The other key to the change in the attitude about the program was that there was a proposal that was ranked the highest in 1981, which wasn't funded. And that was a proposal from Berkeley. And the reason it wasn't funded was that we were able to get DARPA to fund it by putting a lot of pressure on them. It took a lot of effort on my part, and Kent's part, and the assistant director at the time for math, physical sciences and engineering, who went over and leaned heavily on the head of DARPA who leaned heavily on Bob Kahn to fund Berkeley. And Berkeley was a large research project. But it didn't get funded so it didn't show up. People in the field have this tendency, just like they do in all fields, to look at what was successful last year.

ASPRAY: Of course.

ADRION: Not only do they look at what was successful but because we had all four of these proposals and we were really tight on money, we cut back on a lot of support other than the infrastructure support. I guess it's partly my

fault, because I have always believed that the large research projects should be funded. So then the proposals started coming in looking like, "We want equipment," and no one really tried successfully a research program after that - a large, single-focused, multi-investigator research project. Successfully. People did try it, and they got shot down pretty badly each time, although on a few occasions we were able to divert them out and get them funded partly through other programs within the division. This happened with Purdue, it happened with U Mass, and it happened with Princeton.

ASPRAY: Were they shot down because they couldn't pull off the unification, or because your advisory board or your reviewers weren't willing to consider them?

ADRION: NSF review panels tend to be conservative, especially when you have a situation where you have more deserving proposals than you have money. So the case would be that there would be four or five proposals that were standard infrastructure proposals that looked safe, and they certainly needed the money, and then there would be this radical, "We're going to throw everything we do away and work on this one large project" proposal, and people would just be skeptical about it. I mean, now that I am on the other side of the game again, I find NSF to be that way in a lot of areas. Science and technology centers tend to be fairly conservative. I know there were some fairly radical proposals that were put in that didn't do very well. But there certainly were radical proposals. In the end, if there are too many proposals and too little money, it gets conservative. So I think that's what happened.

The CER program has struggled with what it wants to do. The program hasn't. I think the community has struggled with how far it wants to go with the CER program for a long time. I would say every time the subject came up (every three years programs have to be reviewed at NSF) and every time it came up before advisory panels people were split into these multiple camps. There were the camps that said, "Okay, we have already gotten 15, 20," whatever... how many had been in place for that time. "Our department is well-funded. We should just keep investing in those places until we get them up to the MIT, CMU level," because clearly they were still not at that level. Then there were those who said, "The program's done everything it should do. Let's close shop and go home." Then there were those who wanted to continue to fund it out until you got to number 100 on a list and wanted to deny anyone who had already

had one the chance of resubmitting.

So what has happened over time is that, first of all, most programs beyond 20 or 25, 30 (pick some number in that category) don't have enough breadth. It's beginning to change, but up until recently they don't have enough breadth to mount a general infrastructure proposal. So they may have a strong group of a half dozen people. They may have 25 faculty, but they don't have 25 *good* faculty. They would come up against Wisconsin's 33rd renewal and they would get blown out of the water because Wisconsin not only was good when they started, but, two CERs in a row, perhaps three, have been successfully started. It has done them a world of good. I mean, they are a first-class department, and Boise State just doesn't hold up.

So what's happened is the renewals always got better reviews than the others, and so there was almost never any money for new starts after a while. So they put a restriction on how much money they would give renewals, just try to save more money for new starts. Then they created the small program, which was aimed at departments with six good people out of twenty - a small CII program, which they have started up now. That's actually pretty successful.

But really, back in 1977, '76, it was clear that you had a lot of consensus about the problem. The problem was that they weren't attracting faculty and weren't keeping them. MIT graduate students would either go to Stanford, CMU, MIT, or off to industry; they wouldn't go to Cornell, because they didn't have a network access to their buddies back at MIT and they didn't have facilities they could do their research. So we had to do something about that, and everybody agreed that the CER program and CSnet and all that were the answers. But 12 or 13 years later we have no agreement about what to do. In fact, one of the things that I have been encouraging Kerry Hedges to do, and we have agreed to do but we haven't gotten the chance to do it is to create a new Feldman panel to try to look at what computer science needs. What's the problem? I mean, I don't think CER is the problem anymore. I don't think you waste money by continuing to hand it out through a CER program, but there may be something else more important. The small CER program and the minority CER program and all these others have specific missions that I think make some sense. But the big program is sort of reduced to adding one place per year plus renewing more. The high-performance computing industry talks about bringing those thirty institutions up to the level of the top three or four

and doing something that is scaled much larger than the CER program currently does. It would then have to be tied into some significant research activity and not just straight infrastructure. But that's my opinion.

ASPRAY: Can we go back to CSnet and talk about network foundations. It seems to me that there has been a long interest in doing some networking foundations in the Office of Management and Budget. They seemed to repeatedly step in and prevent any major programs, especially one where they were managing some sort of physical operation.

ADRION: There were a number of problems. I mean, the assistant director for Math, Physical Sciences and Engineering in 1976 when I came (and I can't remember who that guy is any more - it's too long ago, but you can look it up) hated networks. If you mentioned networks he would go into shock and glaze over.

ASPRAY: For what reason?

ADRION: I don't know. I think he felt the ARPANET was there. Use that, you know. NSF doesn't get into the business of building things like that. It sort of reflected the OMB attitude.

CSnet has an interesting history. It was in 1970, or 1972 maybe - somewhere in that range - in which the computer science advisory committee said, "We really need a network for all of the computer science community, not just the DARPA people." It took ten years before all the internal hurdles could be overcome, and also before the OMB hurdles could be overcome. When I came back in 1980 to take over the program I was travelling around a lot of places evaluating CER proposals and I would talk to them about Csnet and they'd say, "Well, we really probably don't need that." Some people, the people who wanted to be just like MIT and have everything MIT has and wanted to hire MIT faculty, were quite enthusiastic about it - Wisconsin, for example. But I remember the University of Connecticut just thought that was the biggest waste of money that they could imagine - networks. I mean, they would call somebody up or they would go visit them when they wanted to; this network business was just a waste of time.

I think, and this is only a guess on my part, the only reason CSnet got through at the time it got through were maybe a couple reasons. Weingarten and I had funded something called "Theorynet" just before I left to go to NBS, in which we paid for a typical Telenet centralized service for the entire theory community. It, in fact, included Australia (whatever the service was, I can't remember who was supplying it - Tymenet or Telenet or something). It went to Australia, so the theorists in Australia got in. And Weingarten hired his friend, van Heltz, to do an analysis of it. And it turned out there is a lot of research collaboration that could be directly connected back to Theorynet. So that, plus the fact that Kent could sort of slip CSnet in this package of CER; CSnet was needed to connect the CER places together. You know, the experimental young - whatever they were - grants and post docs. It was all a package; it all went in together as a package. The network was pretty modest. I mean, more than pretty modest funding. That CSnet succeeded on the 3 million bucks or whatever it was, the 2.5 million that was spent on it, is amazing. But it did.

Computer science was always sort of proactive back in the 1970s and 1980s at NSF in the sense that we used to go out and beat the bushes and tell people to send proposals in, and, "Yes, we would love to fund a proposal in area X. Why don't you send us a proposal?" CSnet was a disaster. Larry Landweber wrote this proposal, which was essentially, "We're not on the ARPANET. We want to be on the ARPANET." [laugh] "What we want to do is buy a bunch of 56kb lines and some imps, and we want to hook up to the ARPANET." The reviewers all came back and said, "God, that will cost a fortune," and "Why?" "Figure out something else." And basically, along came maybe four people... or three people... Curtis shoving everybody around. Even though they had terrible reviews, they funded the Landweber's proposal for study; to hold a conference or a workshop and try to write a better proposal. I guess Dave Farber and Vint Cerf showed up at that workshop. And Vint is an excellent organizer of things. So he figured out a structure, a multi-leveled service structure that could layer CSnet underneath ARPANET. Kahn was there and agreed that he would tie in CSnet into ARPANET essentially for free, which was an important leg-up that CSnet had. Farber came along with the bottom level service, with his silly Mailnet thing that he had developed at Delaware - the Phonenet, I guess it became called in CSnet terms. It's silly now that I look at it, but it was the cheap service that everybody could get involved in initially.

It worked primarily because a lot of people spent a lot of their personal time for three years making it work - Farber

and Landweber particularly, a little bit Peter Denning and Tony Hern. And their students. I mean, eventually BBN [Bolt, Beranek and Newman] was brought in to bring some management sense to the whole thing. UCAR layered on top of that, for reasons that are immensely complex. Probably you don't want to waste your time on it, but to give you the history of it - why UCAR? It's essentially that no one trusted BBN, and BBN had proposed to run the whole thing, and they didn't want BBN to run it. The community decided they would create their own corporation to run it. I was gone for a couple weeks, and during that period of time somebody somewhere in NSF made a decision that they wouldn't. NSF had never gotten involved with funding a new corporation. So after they had gone to a lot of trouble in Delaware to get this corporation sort of half put together, somebody somewhere said, "No." And someone said, "Oh, well, there's these associated universities and all this lying around. Why don't you talk to one of them?" And they talked to USRA and they talked to UCAR; UCAR seemed to be willing to do it, and the price was right. They didn't have any overhead rate.

So that's how CSnet got started, and it was wildly successful - I think one of the most successful things that NSF did. I mean, it spread. People were on there; other people wanted to talk to them. Almost everybody got on there. Service was terrible, basically, but people were then able to figure it out. At the same time CER started expanding the number of places DARPA was interested in, so DARPA started paying for real 56kb connections to a lot of places. So the model I always had, based on Vint Cerf's model, was this stuff always sort of trickled down. It was really, really happening that way; the base was broadening out. You know, Backwater U was getting Phonenet and the people who originally were on Phonenet were all getting on ARPANET - Cornell and Wisconsin and U Mass and so forth, so that CER schools all eventually got on ARPANET.

CSnet was doing fine, and the idea was to make it self-supporting. It was almost self-supporting at a time in which Curtis wrote the Curtis-Bardon report, which said that "It's time to get back in the supercomputer business. If we are going to have supercomputers we can't afford to put one everywhere. So we're only going to put a few out there, and that means we need a big, national high band-width network to connect them all" - Sciencenet, which was what it was called until it turned out someone owned the copyright on that. NSFnet came into being. And then CSnet got itself into kind of an awkward situation. In some sense it was good that I was the first program manager for NSFnet

because I still liked CSnet. [laugh] I think otherwise they would have just blown it off at that point and let it go down the tubes, which would have been bad for the field, because it needed to exist, as it did, long enough. Now it's spun off to a private corporation and people are still getting service on it. So everything worked out fine in the end. NSFnet has been considerably more complicated. I followed that until I left the NSF and then I got too busy to travel the network politics circuit.

ASPRAY: Who would be a good person to talk to about that?

ADRION: About the history of NSFnet? Well, I presume you will talk to Steve Wolf who has been there. He was sort of the third program manager for NSFnet. There was this Irishmen whom I am drawing a blank on - Dennis - I remember Dennis was in there between me and Steve Wolf. But Wolf is certainly the person to talk to. I would talk to Dave Farber, and I would talk to Bob Kahn. You are going to get peculiar perspectives from all of these people, because they have their own agenda. It might be worthwhile talking to Ken Wilson, just because he was always skeptical of the NSFnet. Bernie Galler would be a good person to talk to at Michigan.

? may be right. NSF may not [laugh] really be able to manage large projects. I think if Bloch weren't there it would have been even harder to manage a project like NSFnet out of the NSF. I strongly believe, and it has been my position in sort of semi-lobbying for the high-performance computing initiative, that NSF is the only agency that should be involved in setting up a national research network, because it is the only agency that doesn't have a captured community of its own. I mean, it tends to look at science broadly. But the NSF traditional review structures are really a liability when it comes to dealing with a large project like building a network. CSnet was small enough, and the money was essentially all appropriated up front, and so there was never a question of adding more money. Tens of millions of dollars is a lot of money. Trying to figure out how you are going to peer review that in some way is difficult. Also, NSF has never really attracted the kind of staff that you need to oversee a project of that magnitude and hasn't really been able to find the BBN that you could have sort of run everything for you like DARPA had. Michigan serves that role, but again, I have been away from it for a while. Well, I think people are beginning to see that there's money to be made in this, and so corporations are popping up. IBM and MCI have started this

corporation, and there are other commercial networking corporations. AT&T may finally see the light and decide that they ought to do some offerings in this area.

So it's a complex situation, and the politics continue to be awful in Washington about networks. Every agency would just as soon have their own network, and very few of them are willing to sort of cooperate in a national network.

Livermore, Los Alamos tried to grab the National Research and Education Network away from NSF during this last session of Congress. In fact, they came close to succeeding, and then the whole bill ended up dead on the floor a couple of days ago. I think I once said that networking is much more politics than it is a technical problem. Technical problems were much easier to solve than the political problems. The NSFnet that exists today could have been put on-line four or five years ago if it weren't for the politics. It's politics in Washington, politics among the common carriers, politics in the universities, politics between different disciplines (high-energy physics wants to do one thing and biologists want to do another), and interagency politics. It's a mess. I'm glad my stay with NSFnet was short-lived. I enjoyed it, but I am glad I never went back there.

ASPRAY: Before I go to closing comments let me address two other topics quickly. You noted in passing there were a number of instances where NSF provided initial support for something that was later supported at higher levels at DARPA. Time-sharing was one obvious example. Can you give me some other examples?

ADRION: Well, I think there were a lot of the projects that were underlying the ILLIAC series of machines that led the ILLIAC IV, the Holland and Burks work at Michigan that essentially was reflected in the architecture of the ILLIAC IV was funded by NSF. Again, some of the earlier networking research on packets and so-forth was done, but it's less directly traceable, I suppose. I'm trying to think of what else. AI, which has been DARPA's big claim to fame. (NSF was never into it until more recent years.) I remember I was sort of surprised, but I think it largely was in the operating system's area - you know, some of the things that predated Project MAC. The MIT time-sharing system - whichever loaded IBM TS1, or whatever it was. A lot of architecture projects that were eventually exploited; maybe computer graphics. I think some of the early computer graphics stuff was also funded at the NSF that eventually Utah expanded under heavy DARPA funding. But the AI stuff, really, NSF never had anything to do

with.

ASPRAY: And how do you explain this pattern? Is it that you expand to a point where you go just beyond the NSF support or NSF showed proof of principle that attracted DARPA somehow, perhaps for military reasons?

ADRION: Well, I think there's a case back in the 1970s where DARPA really could throw a lot of money at some things. I mean, some of the things they threw money at didn't succeed. They also had good people at DARPA who were able to figure out what projects had a good chance of succeeding. But Project MAC actually was pretty much of a big failure, but it produced a lot of research. I mean, NSF could have never put that much money in any project in computer science. It should, but it has never had the capability of doing that. That's the big difference. There was always a sort of partnership between DARPA and NSF and ONR. ONR and NSF collaborated a lot, especially through the Marvin Dennicoff period and less so recently. But ONR still takes the same sort of attitude of trying to build on top of what NSF does, and sort of filter it recently towards Navy interests. But back in those days ONR was as free and open as NSF. But ONR and NSF would fund small science, and the DARPA program managers would pick out the best of that small science and throw a lot of money at it.

ASPRAY: Tell me about the end of your career in the Foundation. That is, your last position; I understand, you became a deputy division director and you were appointed to a position of chief scientist for CISE. Tell me about what your duties were and why you left.

ADRION: Okay, I went on sabbatical. When I left I was running CER and I went on sabbatical and I came back, and while I was gone on sabbatical I became deputy division director. That was largely an administrative role, essentially managing all the program managers in computer research - helping Kent develop budgets and so forth. It wasn't what I thought it might be. Every other job at NSF they say is a good one and that was not one of the good ones. I think Kent was very fair about it, but he unloaded all the crap that he didn't want to do on me, dealing with secretaries, dealing with recalcitrant and non-performing program managers and what-not, writing a lot of budget stuff. So it wasn't a lot of fun. When the directorate was being born I guess I had not been back long. I came back in

September, so six months, I guess, I was deputy division director for six months.

There was a lot going on. They were trying to find someone to come in and be the new assistant director for CISE. Back then, I don't think it even had a name. Whatever was going to be this conglomeration. They were negotiating with Herb Shore, I think, pretty heavily at IBM, but that fell through because they couldn't work out a deal where IBM could let him come. So then there was this sort of space in which they had to get things to begin to be organized and they had no assistant director yet. Somehow Brownstein and I managed to wrangle our way in to that position as sort of the people who began to put things together until the directorate was formed. Chuck seems to really like the bureaucratic end of things. He enjoys federal politics, etc. He's a political scientist. He should. So we sort of split it up, and the two of us decided how to do it. Meanwhile Gordon got hired, and Gordon's a crazy person, anyway. So Chuck would basically handle the infighting with the internal management structure of NSF. I got to sort of structure the technical part of that in the directorate. Gordon would lay out what he wanted to do, and then I would translate. I don't know if you ever met Gordon Bell.

ASPRAY: No.

ADRION: So I had to translate, because he never finishes sentences or anything like that. So that's where I got where I was, and then, it's strange how I ended up here. I really enjoyed that, but I guess someone had talked me into applying for a job somewhere, which I kept saying, "No, I didn't want to apply for a job." It wasn't here; I think it was in Syracuse actually. So I finally sent in my application to Syracuse. Someone there told the people here that I had applied for that job. So they called me up and they said, "Well, you applied for that job; you can apply for this job." So I applied for it and I came up here. There are people in my research area that are here that I have known for a while. One person I went to undergraduate school with and two that I was in graduate school with, that are on the faculty, so I just liked all the people. So I had a tough time, because I had only been chief scientist, senior scientist - whatever the title was (it changed from time to time) for a short period of time. The directorate was just getting started and it was all pretty exciting, and working with Gordon is always fun. So I went and had a long chat with Gordon and said, "These opportunities don't come along that often." Well, until the state started taking all our

money away it was a department on a rapid rise. So I guess I left. I stayed on and I worked both places until December of 1986 or something like that. So that was the end of my gig.

Also, I got to thinking, "What would I do after that?" You know, as someone who is a chief scientist, a staff position, I would sort of stop being the one who dealt with the community directly, but the one who was sort of structuring what the directions of the directorate were, which is fine. What science do you emphasize and what you deemphasize? I like that part. But less direct interaction with the scientific community, and then what were they going to do after Gordon left? Once you get in that position, that's the position you stay in. You get to be acting like Brownstein from time to time, but again, it's sort of every other position is not a great one. I think the deputy position (unless you're really into that stuff, deputy assistant director, which is what those two jobs replaced, a title that NSF used to call deputy assistant director) has its drawbacks. The assistant director gets to pontificate about everything everywhere and the division directors get to handle their own area, and the program managers get to handle the more refined areas. But the people in the middle have to do all the bookkeeping. I'm not a bookkeeper.

ASPRAY: Lastly, in preparing this study, are there some particular themes you think should be emphasized?

ADRION: Yes, I have this personality-based view of how things happen everywhere, and particularly in Washington. I think a lot of the reasons that computer science got where it was has to do a lot to the efforts of John Pasta and Kent Curtis, some of the other program managers there, but them most particularly. I like Bill Wolf a lot, but I think he was rather less successful because I don't think he's been able to feel the pulse of the community and translate that into programs. At NSF you have two jobs: you hand out money, but you have to get the money to hand out, and the only way you can do that is really understand the pulse of the field. I think Kent was very good at understanding that and about essentially orchestrating the community to give him the support he needed to get the funds to get back to them to do the things they wanted to do. It's tougher at the directorate, and now there is this tug between supercomputer centers and big networks and the research activity. It's a much tougher job for anyone to manage. The community is much larger. It now has all the computer engineers and it has even some communications guys supported. So it's really harder to pull together a consensus, and I think the field is now

stumbling around too. It's bigger...

END OF INTERVIEW