An Interview with
THOMAS A. KEENAN
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Abstract

After Keenan briefly discusses his education, he describes the Rosser and Pierce reports that he worked on that spurred National Science Foundation (NSF) activity in relation to computers and a networking grant that he received from NSF while at the Inter-University Communications Council (Educom). He discusses his work with Kent Curtis in the Office of Computing Activities (OCA). Keenan concludes with a brief mention of other NSF personnel who worked in OCA or related areas.
THOMAS A. KEENAN INTERVIEW
DATE: 28 September 1990
INTERVIEWER: William Aspray
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ASPRAY: It's the 28th of September, 1990, and this is an interview with Thomas Keenan at the National Science Foundation in Washington. The interviewer is William Aspray. Let me begin, first of all, as a context for this by asking you if you will tell me very briefly about your career and how you came to the National Science Foundation.

KEENAN: My education was in physics, and I did graduate work in physics at Purdue after the war, finished with a Ph.D. in 1955 working for Hubert James, and tried a couple topics as thesis material, some of which did not work. Purdue had a contract with the Signal Corp for investigation of semi-conductors at the time... germanium. And one of the things I tried was to solve the quantum mechanical wave equation for perturbed periodic potentials, which turns out to be a very difficult thing to do. So that didn't succeed. Finally, I developed a theory for the behavior of deuterated methane at very cold temperatures - liquid helium temperatures, essentially. There are some phase transitions that are observed and this theory was able to predict...with just one adjustable parameter...something similar to the observed transitions. As part of all that, I had to use desk calculators on extensive calculations, and so when I began to read in newspapers about computers, they seemed attractive. And just about that time, 1954, Purdue ordered a computer. Two of my neighbors were in the electrical engineering department, and I got involved with them, and welcomed the new computer onto campus, sat in on the first seminar that Al Perlis gave when he came to Purdue, and got to be the computer person in the physics department for a little while, as a graduate student. I went to Rochester, which was my home town, managed to talk my way into getting an appointment there. Rochester was also interested in getting computers. Bob Hopkins in the Institute of Optics did ray tracing. Together with a couple of others, he managed to get the University of Rochester to order a computer. Being involved with computers and being a young, available person, I was put in charge of the computer center. No one seemed to know anything about how to organize or manage a computer center. So I began to look around at people in other universities. What were they doing? IBM ran a lot of courses (they were sales pitches, mainly). But they had some for university computer center directors. I attended those fairly often; came to know the other people in computing centers in universities. I became a friend, on a first name basis with Phil Morse who was at MIT; and Bill Kehl, who was at Pittsburgh. When
the National Academy of Sciences decided to (mainly at Phil Morse's instigation) to set up a committee to study the uses of computers in colleges and universities, Phil picked Barclay Rosser as its chairman. They wanted to have Bill Kehl as the executive director. But Bill's wife, Jean, became ill right at that time (this was in the summer of 1962), and Bill regretfully declined the job. So they turned to me and asked me to fill in, which I was happy to do.

ASPRAY: And specifically, what were your duties on this study group?

KEENAN: Move to Washington, arrange for meetings of members of the committee (first of all, you had to get people to agree to be on the committee), arrange for consultants to come, testify before the committee, and finally, generate white papers which became chapters in a report. Different members of the committee were supposed to write these white papers, but you had to keep after them. They would send you something in the mail and then you had to rewrite it.

ASPRAY: Do I hear you saying that you ended up rewriting a good portion of this document?

KEENAN: Yes, Rosser wrote a lot of it, too. Mainly, Rosser and I wrote a good part of that document, which was then submitted through the Academy - it took a long time - and was not very effective, because I think the Bureau of the Budget at the time thought it sounded like self-pleading - self-serving to point out all of these uses for computers in universities, and end up by saying, "And we need so many hundreds of millions of dollars to pursue this further." But this was a predecessor to the Pierce report, which then did have an impact.

ASPRAY: Why do you think there was a difference in the impact of the two reports?

KEENAN: Well, the Pierce report was from PSAC, and I think the tone of it was different too.

ASPRAY: In what ways?
KEENAN: The Rosser report called for a large amount of money to be invested in various disciplines of physics and chemistry - all across the board - astronomy and whatnot, to make available mainframe computers for use in research in all of those disciplines. The Pierce report was less focused on money, I think, and instead called for the establishment of the network of knowledge and an office in the National Science Foundation.

ASPRAY: If I remember correctly, Network for Knowledge was a term that had been introduced by Lyndon Johnson.

KEENAN: Right. Well, it existed before Lyndon Johnson picked it up.

ASPRAY: What was the origin of that?

KEENAN: From the president's science advisor.

ASPRAY: It was from PSAC.

KEENAN: And obviously, they were both going to call for money - an investment. But, I think, just relying on memory right now - I haven't read them, either one, in a long time - I think the Pierce report was less on money and more on organization function, "We've got to do things right."

ASPRAY: Do you think that the assessment in the two reports was accurate of the overall need, for example? I have heard from some people that they think that these reports both underestimated the need.

KEENAN: Probably in retrospect they both underestimated what should have been done. On the other hand, they both called for large increases in what was being done and were ahead of their times in that way. If you had asked for more you probably would have gotten less.

ASPRAY: I didn't intend it to come out to put you on the defensive. I just had heard that statement and I had no
opinion about it.

KEENAN: In retrospect, the whole industry, the computer business - whatever you want to call it - has grown far more than anyone ever thought was possible, even myself.

ASPRAY: How were the people chosen for the Rosser committee?

KEENAN: By Rosser, and myself, and Phil Morse, and Monroe Martin, who was at Maryland and was the director of the Mathematics Section at the Academy at the time - sitting down and saying, "Well, we need somebody from chemistry, and we need an astronomer. Who knows a good astronomer?" Making up lists of people...

ASPRAY: To represent the various scientific disciplines that used or would expect to use computing facilities. And was the Pierce report done entirely inside PSAC, or were there advisors?

KEENAN: No, they had advisors, too.

ASPRAY: I can look at this as a matter of record.

KEENAN: I think John Kemeny was an advisor. Maybe he was on the committee; I am not sure. Tom Kurtz was an advisor, I know - not on the committee. Kemeny and Kurtz went together. I think they must have had other advisors, but I don't recall.

ASPRAY: Okay, back to your career.

KEENAN: You can see that I got involved in reports and report writing in the National Academy of Sciences and I had concluded, myself, somewhere in the early 1960s as a director of a computing center, that there had to be more than providing computer cycles to this whole development. There was an intellectual content that I found
interesting, fascinating, but the people who came to buy time on a computer weren't interested in that intellectual content. I thought there had to be an academic outlet for this intellectual content. So I tried to talk people into acknowledging that there ought to be academic appointments, there ought to be courses, and so forth. Everyone was interested in pushing their own bandwagon, of course. The mathematicians that I met, most of them, weren't interested in this applied stuff. They were abstract mathematicians, and couldn't see the point of computers.

Electrical engineering was at a stage where they had just gotten out of the power engineering mold after the war and were discovering some sort of communications engineering. But, although there were some engineers that were interested in computers, it wasn't a main part of electrical engineering curriculum at the time. So being rejected by the two most obvious places, I, and others, began to feel that there was a need to create something new. About that time, a group of us met at one of the conferences and decided to form within the Association for Computing Machinery a committee on curriculum. So Bill Atchison, I and several others became the moving force to set up this committee, began to have our meetings, and pull in consultants from all over the country, "What course are you giving?" And "Give us the outline of the course," and argue about many things such as "Was numerical analysis really necessary?" Some of my good friends are numerical analysts and we had good battles about it. We finally came out with a report in 1964, another report in 1965, and then the final one in 1968. Through much of this I was becoming well acquainted and respected by lots of people around the country, but I kept pushing for an academic activity back at home in the University of Rochester and did not make myself friends there.

ASPRAY: Was there any role for the Foundation up until this point in these discussions?

KEENAN: The Foundation had started a program of grants for computing centers. John von Neumann wrote the report for that one. 1958, I think that began, 1959. It was in the Math Division here. There were some people in math at the Foundation that began to notice and pay attention to computers, yes.

ASPRAY: But were any of those people coming, say, to the ACM meetings and participating in the curriculum development area?
KEENAN: I think I have to answer no, not that I can recall. The Curriculum Committee on computer Science, C³S, did get an NSF grant.

ASPRAY: Who was the project manager for that?

KEENAN: It was somebody I knew, had called and said, "Can you help?" My memory is that it was Lyle Philips. At any rate, so I was involved with the curriculum committee; I was involved with the Academy - Rosser Committee. And then in 1964 at another meeting I bumped into a friend at a New York hotel and said, "What are you doing this summer?" And he said, "I'm going out to California to write a book." And I said, "Oh, that's interesting." This is Elliott Organick. And I said, "That's interesting; how do you go to California to write a book?" He told me he had been invited by Ed Beagle at the School Mathematics Study Group (SMSG) at Stanford to come out and see whether they could form a group to draft materials for a SMSG book. I said, "That sounds like fun." And so, when I got home I had a phone call from Elliott saying, "Why don't you come too?" I said, "Let me see if I can arrange it," and managed to arrange it in a matter of weeks, to take eight weeks and go out to Stanford and sit down with a group of people who wanted to draft this material, not knowing at all what I was getting into. When I arrived I discovered that there had been a small group that had met the summer before and had outlined eight chapters. We looked at the stuff that they had done and decided it had to be all rewritten. But we agreed on eight chapters anyhow. And so we began turning out material. One of the people in the group was Warren Stenberg from Minnesota. He was more of a traditional mathematician. I think Ed Beagle put him in the group to keep the rest of us honest. But all of a sudden, after about a week of his being puzzled about why computers ("Why are we writing about computers and not about mathematics?"), he got the idea and suddenly was turned on and began writing some marvelous material that was full of humor. He had a lot of fun doing it. We had to tone it down later, but in eight weeks we produced enough stuff to serve as a first draft for a high school course on computing. Ed Beagle invited us back the next summer to do a second edition of it, which we did. He didn't invite all of the group - there were eight people in the original group, and he invited four of us to come back for the second summer. We revised, expanded, and produced another book for SMSG, which I understood was the best-selling book that they had.
ASPRAY: Now isn't this quite a departure from curriculum in computing? I mean, as I understand curriculum developing in computing it drifted down from the graduate level to the undergraduate level, and at that time in the mid-1960s it seems to me it was still mainly at the junior, senior level of the college curriculum. It seems like a big jump to go to the high school.

KEENAN: If you read the Curriculum 1968, it did not recommend an undergraduate curriculum. Some of the materials clearly at the undergraduate level was being taught at places at the undergraduate level. But what was being talked about there was a graduate curriculum. And you're right; we expected that the material would drift down rapidly. Then, coming at it from the other end, we thought some of this material can certainly be taught to high school kids - why not?

ASPRAY: There's also a problem of facilities to support this - how many high schools supported equipment at the time?

KEENAN: Not a lot. At any rate, we did that in the summer of 1965 - we produced the second edition of the SMSG book and sat back on our laurels. We consulted with people at NSF who told us that this was public material in the public domain, that it was not our property, that anyone could make use of it, and they advised us to wait a reasonable period of time before doing any private revisions of it. We waited two years and got together again and revised it into a commercially available book.

ASPRAY: This had been done with Foundation support originally?

KEENAN: The School Mathematics Study Group was supported by NSF grants, yes. So we waited, and the whole world had an opportunity to make use of the material. Then I was not in favor at the U of R (the University of Rochester) - after all, I was gone all these summers and moving off around the country doing my own things and coming back and saying, "We have to have an academic program in Computer Science here; we have to have it." And the other people...electrical engineers, they wanted to build up electrical engineering, physicists were doing their
physics, mathematicians couldn't be bothered with us. So I decided to leave the U of R with some dissatisfaction on both sides. There was a consortium being formed at the time, which had to do with communications and computers in multiple universities; that seemed to be a natural. The Inter-University Communications Council - Educom. So I, in leaving Rochester, decided to cast my lot with Educom for a while. First moved out to California. Educom was an unusual organization. The board of directors was made up of people from all around the country. There were several people in California that were directors. I was supposed to interface with them, keep them active in Educom, which I tried to do. I tried to generate committees of people to gather and write reports on everything - computer-assisted instruction, information retrieval - for the benefit of the officers and directors of Educom to take to others to say, "See these reports! See what we are doing for you. Become a member of Educom." Well, it was a thin organization. When I was in California there was a guy, or two guys, in Washington. There was Edison Montgomery at Pittsburgh. And communication was very, very difficult. The people who were not employed by Educom itself - that is, those who were donating their time - had lots of other things they wanted to do too. After a year Edison Montgomery, who was the president at that point, decided to consolidate, bring everyone together in one place, and he chose Washington, DC - Bethesda, actually. So I moved to Washington. And Educom continued to be a thin organization trying to get grants, trying to get some money, had a Kellogg Foundation grant, basically. I got a grant from NSF for a project.

ASPRAY: What was the nature of that?

KEENAN: We called it the Educational Information Network, which turned out to be a collection of computer programs which had been prepared in various universities. UCLA had an excellent series of statistics programs. We wrote a description of these programs and how to use them and so on and published it - a big set of three-ring binders. There were many other programs from other universities. Each university had something that they were proud of, I guess. We published this collection of write-ups of programs, because it was too early to really network in a physical sense, although DARPA was starting to. We didn't have the money for that. So we networked through publishing this material. But Educom was a hand-to-mouth existence, and after a couple of years and with a couple of fallings out, reorganizations, whatever you want to call them, I decided to look elsewhere and saw a friend, Glenn
Ingram, from NSF and said, "Any opportunities down there?" and they said, "Yes." So that's how I came to NSF.

ASPRAY: What was the state of the program at NSF when you arrived?

KEENAN: The Office of Computing Activities had just been established the summer before.

ASPRAY: So this was 1966 or 1967? Something like that?

KEENAN: Maybe it was a year before. I came here in November of 1969. So when was OCA established?

ASPRAY: I think it was officially in 1967 sometime, but I don't know.

KEENAN: I suspect it was announced in 1967 and staffed in 1968, or something like that.

ASPRAY: That's quite possible. What did you find in OCA at the time. I mean, what was the emphasis? What were the problems? What was the size of the program?

KEENAN: Well, OCA was organized essentially as a miniature NSF. It had three components. the largest piece was the computer facilities component, the outgrowth of the computing centers program. NSF at the time was involved in building "Centers of Excellence," construction, buildings.

ASPRAY: Facilities?

KEENAN: Yes, facilities, but more than in the sense of machines.

ASPRAY: Right, high capital...
KEENAN: Yes, that was one of the directorates of the Foundation. And another directorate was in education, and the third directorate was in research - all kinds of research. OCA was organized as facilities, an education component and a research component.

ASPRAY: And by this time was there a well-established pattern of research being supported by the Foundation?

KEENAN: The research component was very small. There were a number of grants that had been made even before OCA was established. MIT had a grant to support the initial time-sharing project before Project Mac, called CTSS. And the University of Michigan, Bernie Galler had a project for an operating system - the Michigan Tape System.

ASPRAY: Right, MTS.

KEENAN: MTS, with the MAD language. So there had been some things like that, and I think Saul Gorn at Pennsylvania had been supported. [tape off] Are we back on?

ASPRAY: Yes, we are. So there had been a few things.

KEENAN: There had been a few grants. I remember the first thing I worked on when I came here was a grant to the Systems Development Corporation for the selection of people from various smaller colleges, generally in math departments in smaller colleges who were going to take a year to go to SDC and learn about computers, proselytizing from the start. And the grant had already been made to SDC when I arrived, but the first thing was to process these requests or applications from... I think there were about 30 or 40 people around the country in smaller colleges, from which ten were selected.

ASPRAY: Can you tell me what was the rationale for organizing OCA as opposed to just continuing within mathematics?
KEENAN: It was before I arrived, but that was from Johnson's Networks for Knowledge speech, which really was
generated from the Pierce Report on the PSAC activity.

ASPRAY: So this was an outgrowth of the Pierce report.

KEENAN: It was an outgrowth of the Pierce report. That's my understanding. And the administrative decision to
make it more visible, rather than just using one of the existing programs such as mathematics, which had been the
choice - make it more apparent that NSF was going to support computing, largely through the computing centers.
But there were also the education and research components.

ASPRAY: Tell me about your duties as a program officer at OCA?

KEENAN: Yes, as things got settled in, Kent Curtis was named as head of the research section of OCA. I and John
Lehmann were program officers working for Kent.

ASPRAY: Was there a division of labor or did people just sort of work together in those days?

KEENAN: We worked together, but we identified programs and we had to have titles and names and things like that
for everything. And we decided that there were a minimum of three - smallest integer greater than two - things that
went to make up computing. The first was theory; the second was hardware; the third was software. So John
Lehmann became the hardware person. I became the software person. And in the beginning I think Val Tareski was
the theory person. I think Val was here at that time. Each of these programs had probably something less than a
million dollars to spend. I think the section had perhaps a $2 million budget in 1969 or 1970 - around in there. And of
course, what we did is go out to conferences and get up and give a little talk saying, "NSF is willing to accept
proposals if you're interested in these things."

ASPRAY: Did you take a fairly proactive role in prioritizing areas for research?
KEENAN: I think the answer to that is no. I think NSF's style has been, in my memory, not to be proactive; not to be wise enough to decide what should be done. The proposals and the community as a whole would determine that - in the reviews and in proposals. People would write proposals about things that interested them rather than something that was determined by a bureaucrat in Washington, and the reviewers would write good reviews about things that interested them. That's the way it went on for a long time.

ASPRAY: To follow through this, I assume that the program officer would just want to make sure that enough proposals were coming in from the right people that the set of applications reflected the overall organization of the field, or the research directions that the field was taking.

KEENAN: Of course, the field grew so rapidly and divided into so many pieces that that became more and more difficult to do. By now, you can go to conferences every week in the year, for software, at least. So we had to decide some things we weren't going to get into.

ASPRAY: And what were kinds of...? Can you give me some examples of those decisions?

KEENAN: Well, it's not a good example and I don't want to point to any individuals, but I do remember a proposal on ternary logic - a three valued logic - which I worried over and tried to get reviewers for it. I got reviewers and an occasional reviewer would say, "This is great stuff - very interesting mathematics." And another one would say, "Yes, I can see that, but it isn't computing." And I finally had to decide that that was not something that we would support as computer science.

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KEENAN: That was something we decided not to support, and there were other cases too where we had to make a choice that this was the direction not to go in. But it was always... We always tried to base it on some consensus of
people that... the reviewers or advisors... advisory committee perhaps, saying, "This is a topic that has been worried to death and we probably shouldn't encourage much more of it." When they gave us that advice we tried to not encourage much more of it.

ASPRAY: Did the advisory committees play a fairly important role in the direction of the program?

KEENAN: Yes, I think they did as advisors. Even from the early days I think that was a fairly important role of... We used to ask them, "What would you do if our budget were reduced 20%?" There was only one time it was reduced substantially, and that was about 1973, I think, when we had gone through an increase because of the Mansfield Amendment. We got a doubling of our computer science research budget at that time. Suddenly we were running around trying to find good things to spend it on and did fairly well, I guess, but then the year after that faded away very quickly.

ASPRAY: It's my understanding that, especially when you compare the NSF budgets with the DARPA budgets, say, that just because you didn't have as much money to spend, or at least spend on individual projects, that there were certain kinds of research that you couldn't support. Is that an accurate statement?

KEENAN: DARPA had the opportunity to focus more money into fewer places. Part of NSF's charge, or theme, has always been - that I can remember, it's gotten away from it some now - to support individual research projects and to support them at many different places, because the health of science is the goal. That's what NSF is charged with. You don't develop a healthy science by having just three or five or nine prime places in the country, but by having good science done many places. So the distribution geographically - although, statistically, that sort of takes care of itself. But we did have to keep an eye on it from time to time - make sure that we got some grants out into the midwest and down into the south. We were always disappointed that Florida didn't have a stronger program, so we could go down there in the winter time.

ASPRAY: But there weren't projects that involved heavy capital equipment costs that you had to turn down, or ones
that involved large teams of students to work on a particular project?

KEENAN: There were some projects that were larger than others. Jack Schwartz project at NYU on the SETL language and compiler was one that was supported over a period of about ten years, or twelve years, maybe, and probably averaged a couple hundred thousand dollars a year. So that was a substantial project. And there have been other projects right along. Don Knuth was supported from my program for many years until he retired last year. And it's hard to tell with Don, how many students and how many associates and so on were there really, but he always surrounded himself with a bunch of bright students. And he was always inviting somebody from Finland, or Germany, or France, or wherever - someplace - Japan, to come visit him. And part of that visit would be defrayed from the grant usually.

ASPRAY: Specifically in the software and programming area, what were the major things that you were supported? Who were some of the people that you can look back on and say that they had a major impact, say, on the field, that got substantial support from your program?

KEENAN: Knuth is a prime example. Jack Schwartz, I think, is another excellent example; Gary Lindstrom at Utah has done a lot of good work; Greg Andrews. I mean, there are lots of people... People at Carnegie Mellon - John Reynolds, and Zohar Manna at Stanford is... and Dick Waldinger at SRI. I am leaving people out.

ASPRAY: Of course. I don't expect you to give me a complete group, but one of the problems that we have is that we see this undifferentiated mass of grants that were awarded; we have to pick a few out as an illustration of this area or that area, and that's really what I was looking for here.

KEENAN: Liskov at MIT. A lot of these people had other support too, but NSF money was preferred, because we didn't require that they submit quarterly reports or have products to produce. We wanted them to have students and write some papers, and get them published in good journals.
ASPRAY: At the time that you became involved with the software and programming systems work, what were the current sets of problems that were being looked at? Were they questions of operating systems, or were they programming languages, or were there any software engineering questions being asked?

KEENAN: No, software engineering was a later invention. Programming languages, where something was seen as a significant question; operating systems, yes; data storage and retrieval goes back a long ways; tape systems for - how do you retrieve best from a linear tape?

ASPRAY: And that was considered part of programming systems rather than a hardware question.

KEENAN: Yes. We did support some individual language design development, but then discovered - this is one of the choices that was made, I think, consciously - we didn't have the resources to support everybody's idea of a new language. Everyone in the world could come up with a new computer language that had this feature or that feature, or a combination of features from here and there and somewhere else. And so that was consciously played down. I tried to select a few that were unique and had outstanding people behind them.

ASPRAY: Was there a sense in the programming area of trying to build up a science of computing in time?

KEENAN: Yes, I think so.

ASPRAY: And how was that manifested? Were there certain kinds of grants that were discouraged, and were there certain kinds of areas you promoted at conferences?

KEENAN: Well, computer science had achieved the title computer science without much science in it, early. And I think we - here I have to say that Kent Curtis was a prime person... I loved the man very much; he was a great guy - we decided that to be a science you had to have theory, and not just theory itself as a separate program, but everything had to have a theoretical basis. And so, whenever we had a proposal, we encouraged, as much as we
could, some kind of a theoretical background for this proposal - not just software, and not just write a program, but there should be some basis for it.

ASPRAY: Was there also, at the same time, an effort to encourage curriculum development of standardization of some of these theories into a standard course material, a standard curriculum?

KEENAN: Not in the research section. As I said, there was a facilities section, and an education section, and research. Kent was in charge of the research section and I worked for him. We were trying to develop a research program. There was an educational program, which, part of it (and you should talk to Andy Molnar about this) had a curriculum development program.

ASPRAY: Wasn't there more effort in the education portion on the use of the computer as a tool elsewhere, rather than in developing an educational program in computing?

KEENAN: Well, you have to go through the changes. I am not sure I can give you all the changes in the right order at the moment.

ASPRAY: Okay, well, why don't we leave that topic aside? I'll ask at the last moment. What happened with your own career at the Foundation? Did your position change over time? Did you take on different duties?

KEENAN: No, not really. There was a period that Kent was on leave of absence, when I filled in as section head - 1972-73, or 1973-74; I forget which. The Mansfield Amendment - that was about that time too.

ASPRAY: Right. How did the day to day job of the program officer change over time? Did it become more bureaucratic? Did people become more specialized in the programs they ran because it was larger and there were more people?
KEENAN: Yes, as time went on we began to define more programs. Essentially, there was the Applications of Computers in Research section at one stage, which was after the education section was moved out to Wisconsin Avenue. So, rather than being just a one section office, we generated other programs such as the Applications of Computers in Research. But after awhile that faded away too, and then we needed to expand our concept of theory hardware and software. We decided to add Intelligent Systems and hardware became Architecture. There were the intelligent systems, software systems, and software engineering programs. Software engineering had appeared in the mid-1970s after a couple of conferences in Germany - Garmisch. And so we thought it was a decent title and we ought to take that, make another program out of it. I thought of the difference between software engineering and software systems as, sort of, the practical and the more abstract - the nearer term and the longer term, maybe - with software engineering being hopefully more practical in the nearer term. And...

ASPRAY: Was all of the work that - it came together in the 1980s, really, rather than earlier - but work on programming verification and semantics and such, coming out of the programming area, or coming out of the theory area, or was it a mixture of the two?

KEENAN: My recollection is that most of that came out of the software area rather than out of theory. I wouldn't say 100%, but theory was... back in the 1970s was focused on complexity, and a number of program directors in theory claimed that to be a theoretician you had to go to one of two conferences.

ASPRAY: [laugh]

KEENAN: And if you went to other conferences you couldn't be a theoretician.

ASPRAY: [laugh] I see.

KEENAN: Those were the conferences that generally dealt with complexity in computing. But there was semantics work that came out of theory, too. There must have been.
ASPRAY: If you were to look at some of the... If you look at this from my perspective of trying to put together a story of NSF and its impact on computing, what themes would you think are important ones to emphasize in such a story?

KEENAN: I think NSF has supported and encouraged many more graduate students and graduate training; more than other agencies. That's my belief; I don't have any data to prove it, I am sure. I think that we were interested in taking chances on people that we didn't know, whereas DARPA, for example, tended to focus on a few institutions where they had top-notch people, of course. I remember I knew Licklider, who I think was the first, or maybe the second director of DARPA.

ASPRAY: He was the first.

KEENAN: I remember he was a consultant to the Rosser committee, by the way.

ASPRAY: I didn't know that.

KEENAN: And when I was working with the Rosser committee he invited me over there to his office. We sat around one afternoon trying to think of who were the prime people in computing that should be contacted for ARPA at the time - contracts. Made up a list of about 20 people or so. I suppose he did it with lots of other people too.

ASPRAY: Sure. Other things that one might want to emphasize in such a study.

KEENAN: In your history?

ASPRAY: In the history.
KEENAN: Of what impact NSF has had?

ASPRAY: Right.

KEENAN: People. NSF has been limited in not being able to put on a big show. There's never enough money; nowhere near. And I think it's in the support of graduate students, the encouragement of people who perhaps couldn't be at the premiere research institutions, but nevertheless have contributed a great deal over the years.

ASPRAY: I don't want to lead you, so I haven't been suggesting any names, but let me go partway there. How important a role do facilities, both bill-buying machines, first for universities and then for departments, and then networking for science... do you see that as a major part of the NSF story in computing?

KEENAN: Well, NSF got started essentially by going into facilities-supported computing centers. And that was very important at the time. Shortly after I came here (I think it was 1971, maybe) we were told to phase out rather quickly the facilities activity.

ASPRAY: And who told you and why?

KEENAN: I was never told myself. It came through the assistant director for research, I think, at the time to the head of the office, which was... Who was the head of the office? Milt Rose, I think had gone, I think.

ASPRAY: John Pasta?

KEENAN: No, Pasta hadn't arrived. My friend, Glenn Ingram, who is out at Bureau of Standards - NIST, now.

ASPRAY: But then there was another program a bit later for supporting departments who were doing research? Facilities, as I understand...
KEENAN: In the research section we were always willing to buy terminals and provide money for computer time. We didn't have enough money to really support large computers. But it might have been 1978, 1979 - somewhere in there - we decided to make a competition for equipment for each of the programs. Instead of spending a certain amount on terminals and perhaps small computers for individual projects, we decided we would get more fire power out of it by making an equipment program as a separate competition. Each of the existing programs contributed to that what they were already spending. And this was, I guess you would interpret it as, departmental equipment, but the idea was that we could buy things that were needed by more than one project - more than one research project. We intended to still support the purchase of terminals, which were a sort of a one-project type thing, but when you needed something that was larger or more expensive than could be justified by a single research activity, then to come through this equipment program. We bought a lot of VAXes. I remember one, Cornell, as a matter of fact, was quite a theoretical department, and they got money from that program for a VAX, which was the first computer that the computer science department at Cornell had had. And they liked that so well that they came around the next year and said, "We like our VAX so much we want to have another one." [laughter] It changed that department; it changed a lot of places.

ASPRAY: Do you see the Foundation's efforts in networking as also being part of this same kind of facilities effort?

KEENAN: Well, if you are going to have supercomputer centers you have to be able to communicate with them. So to be able to make these supercomputer centers accessible you have to have a network. And I guess if no one else is going to support that network NSF has to do something about it. I am not sure that I agree that supercomputer centers are good for computer research, but I guess they're good for physics and good for chemistry and good for astronomy and so on, but there isn't much... It's like going back to the old days of the computing centers where you had a lot of customers but nobody was interested in the intellectual climate.

ASPRAY: What about the CER program?
KEENAN: That was something that the advisory committee recommended we get into and it played a major role in generating that. Again, it was people on the advisory committee who were saying NSF does too many small things. "What we need is infrastructure built up in other than the three premiere departments. And to do that we have to put bigger money in and buy equipment and staff, and somehow, at the same time, I guess, keep up the individual projects." [laughter] So we went through an extended period of planning in trying to do that and finally even prior to the formal announcement, there were a number of places that were submitting proposals. And again, by taxing ourselves we got enough money for one grant the first year. Then the assistant director at that time carried the message on to Congress, saying that they think this is important enough that they tax themselves, and therefore, we were asking for an increase for the future, which worked. That's the way to go: tax yourself.

ASPRAY: And in hindsight do you think that it was a valuable program?

KEENAN: Oh, yes. I think it has perhaps outlived its original usefulness - it's become a bureaucracy. [laugh] It had a definite impact, I think, on the retention of faculty, other than the three premier departments, and on the retention of graduate students. Before that, a lot of students would be gone; with lots of job opportunities they would end with a master's degree. Or they would go for a couple of years on their doctorate and not bother with a thesis.

ASPRAY: Just going in...?

KEENAN: Going into industry, yes, got a big job, sure. Lots of well-known people quit early and were making big bucks. But then more structure provided in the universities, such as Berkeley and so on. And people began to stay to get their degree. And we now have a much higher Ph.D. production rate.

ASPRAY: We haven't really talked about industry at all - foundations, attitudes about supporting industrial research. Do you want to make any comments about that?

KEENAN: The Foundation has had a couple of programs over the years to support industrial research. There's a
small business innovation research program which I was the representative for in the computer science division for many years until a couple of years ago. There was a cooperative university-industry program that went along for a bunch of years in which we had grants. But for the most part, I don't think that the Foundation has any business supporting research to IBM or GE. The health of science comes from the universities, colleges, and from the students. That's where it should be focused. If you have that charge that determines what you are going to do.

ASPRAY: Yes. There does seem to be a pattern. There may not be a high level of support but a pattern of support over a long period of time to places like SRI, maybe occasionally to Rand and SDC, a few other places. Is there something important about that to be said?

KEENAN: I don't think so. These are non-profit organizations which are qualified to write proposals to NSF; we have to consider them. I think for the most part we were worried about what was "if we make grants to somebody at SRI, how is this going to help education? How is this going to help the health of science?" There are brilliant people at these places, but they pay a high overhead rate, too. And generally I tried, at least, to be sure that there were students involved, even if they didn't show up on the books. The people I supported at SRI generally were on the Stanford campus at least half the time with students. They were attending seminars, and giving seminars, and stuff like that.

ASPRAY: If you were to look back on the different technical areas, do you see one or two or three areas where the Foundation support has had an especially important or strong impact that makes it stand out - they support it across the board, but are there a few areas that really stand out?

KEENAN: You're making it as difficult as preparing testimony for Congress.

ASPRAY: [laugh]

KEENAN: Well, I suppose I am repeating myself, but the support of people like Knuth, helping him to be able to
produce some of his volumes, essentially; to do the research, at least - to gather the information that he has had to
gather to put into those volumes has been a valuable kind of push for me. Also, his TEK. He discovered that he
couldn't get mathematical texts composited in this country in any decent way. I worried that he was spending a year
and a half, two years of his life on something that may not be as productive as text book writing, but I guess it was
worthwhile. I went out and talked to him about it and I said, "Are you sure you're doing the right thing?" [laughter]

ASPRAY: But you don't want to say that of all the areas - graphics, or theory, or some other field - are ones that the
Foundation made an especially strong mark in.

KEENAN: I don't feel you have to.

ASPRAY: That's the kind of thing I want to know one way or the other.

KEENAN: No, I think we have supported semantics work when semantics was not popular - when people who were
interested in understanding the semantics of formal languages, couldn't get support from Energy, or DARPA, or
anywhere else.

ASPRAY: Do I understand correctly that the main contribution to the computing discipline... to the research area of
the computing discipline - from the Department of Energy - was in the area of numerical analysis? Were there many
others, or other areas that it contributed in?

KEENAN: Well, the Atomic Energy Commission had made lots of contributions early in the game. They were the
source of pushing for supercomputers in the early days.

ASPRAY: STRETCH and LARK.

KEENAN: And in retrospect, that was important to. Also, they supported language development, for example, at
Los Alamos. Nobody else uses the language, but the ideas that were developed there have sort of penetrated a lot of people, perhaps even without their even understanding where they came from. So AEC, I think, made a considerable contribution beyond just numerical analysis. I think I mentioned John Reynolds, too. He's at Carnegie Mellon now, but he came through the AEC in his early years. He's a logician.

ASPRAY: And what about the role of ONR and AFOSR? Where do you see their contributions?

KEENAN: Well, these are agencies that have a mission - a mission that they have to convince their governing bodies is being accomplished, that they're supporting computing for the benefit of the Navy or the Air Force, and so on, which means they have to take a somewhat different approach. I don't think that the Air Force supported much academic research. They certainly support research in aerospace associations, and so on.

ASPRAY: So a fair amount of neuralnets, for examples, that came through on time.

ASPRAY: ONR?

KEENAN: ONR is more in the mold of NSF, or NSF is more in the mold of ONR. Of course, NSF was formed from some people that originally came from ONR. As I was saying, ONR is more on the mold of NSF, or vice versa. That is, we often find that someone that's interested in a grant also had ONR support, or was equivalently trying to get it there. But I think it's a disadvantage that they have had in having to have a mission and to prove the utility of their research to the Navy's mission, although I understand the reason for it.

ASPRAY: Sure. Last set of questions, unless you have more things that you would like to say...

KEENAN: You want me to ramble on? [laugh]
ASPRAY: Well, that's up to you. It's your opportunity to influence this study. Of course, you can talk to me anytime. If I wanted to talk to some other people about NSF computing up through the period of the early 1970s, who would you recommend I talk to?

KEENAN: Well, Don Auffenkamp is dead; Kent Curtis is dead; John Pasta is dead. Who else was here at the time? Elmer Havens, was that it? I don't know if he's alive or dead now. Who else?

ASPRAY: There was Milt Rose.

KEENAN: Milt Rose, right.

ASPRAY: People that might have sat on advisory committees for a long period of time, or were influential on advisory committees over a period of time?

KEENAN: Oh, let's see. There was... I'm sorry I have forgotten so many names. Do you have that with many of your interviewees? [laugh]

ASPRAY: Everybody.

KEENAN: There was a guy on the advisory committee in the mid-1970s who was instrumental in talking about the need for the CER programs - Bob... He was at Xerox PARC at the time, and has now gone over to... He's still in the Bay Area somewhere. Who's the fellow at Cornell that we used? Hopcroft, of course, at Cornell.

ASPRAY: Hartmannis?

KEENAN: Hartmannis, right. That's who I was trying to think of. He was, I think, a key advisor for some periods in
there. Elmer Havens, I think it was - if you can look him up and find out whether he's alive or dead.

ASPRAY: Yes. Any one at a higher level in the Foundation administration who might have had an interest in computing and can give a different perspective on that early period?

KEENAN: Do you have Fred Weinberg?

ASPRAY: Weingarten?

KEENAN: Weingarten, yes.

ASPRAY: I spoke with him this week.

END OF INTERVIEW