An Interview with

KEITH UNCAPHER

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Abstract

The interview begins with a review of projects at RAND when Uncapher was hired in 1950. He discusses some of the projects he was involved in or had managerial responsibility for through the early 1970s, such as JOHNNIAC, JOSS, a survivable national network, GRAIL (GRAhical Interactive Language), and some work related to the ARPANET. The formation of Information Sciences Institute (ISI), funded by DARPA, is described, as well as some of the work ISI did for DARPA/IPTO. The interview ends with Uncapher's general observations on how DARPA and IPTO have changed over his years as a contractor. This interview was recorded as part of a research project on the influence of the Defense Advanced Research Projects Agency (DARPA) on the development of computer science in the United States.

KEITH UNCAPHER INTERVIEW

DATE: 10 July 1989 INTERVIEWER: Arthur L. Norberg

LOCATION: Los Angeles, CA

NORBERG: Keith, can you describe for me some of the programs that you were involved in at the Rand Corporation when you first went there?

UNCAPHER: Yes, I joined in 1950 and went to work for one of the country's premier engineers, W. F. Gunning. He has been at Xerox PARC for several years. We had a REAC analog computer, and Gunning's thrust was to make some of it digitally oriented, and to significantly increase the ease of use. There were two thrusts at the time. One was to have a removable plugboard. Analog computers at that time had large plugboards that were attached to the main machine. So if a person was through with one problem, he had to pull away all the wires and have the machine completely down while repatching for the next problem. Gunning created a removable plugboard. He also created the world's first, in my judgement, analog-to-digital readout of any measurement that anybody wanted to make on that analog computer. Also, he created higher definition servo-amplifiers with much greater stability, using as much digital lore and culture as was around then, which wasn't very much. While that machine was being finished and turned over to the scientists and engineers inside Rand Corporation, it was clear in terms of Rand's dedication to the United States Air Force that a lot more computing would have to take place than was available from the card-driven calculators that came out of IBM. And Rand approached IBM research and said, "The military needs a lot more computing power," and tried to convince IBM to proceed to build something that would be a model of Rand's image of what would be useful. IBM pictured the market apparently as being too small and was not interested. Rand then...

NORBERG: Did they actually say that, Keith, or is that something that you people concluded from the discussions?

UNCAPHER: I don't know which it was, and I was not involved in the discussions. The people that were... One is George Brown, who I think is still alive and, I hope, well at UC Irvine, and the others I think are probably deceased. The Rand decision was, in looking at the Army's building of what was called the ORDVAC, which was a von

Neumann-class machine, the first of which was at Princeton, to proceed, because IBM was not as responsive as Rand wanted them to be. And again, the Gunning influence was dominant. Bill Gunning chose two thrusts: one, to make an easy front-end based on the experience with the analog computer, and to focus on high-speed memory development. He set out to build a machine with Air Force funding, and, I think, did three things. It was the first machine to hide the complexity of the machine from the user by having a console, which was really beautifully designed, largely by Cliff Shaw and Tom Ellis, for the maintenance engineer... there were fold-down sections which revealed the whole complexity of the machine and the status, flip-flop by flip-flop, to the engineer, but all of that was hidden from the user. In addition, we took a fling, along with others, at the Williams-type CRT storage and decided we couldn't make any special contribution to that; our effort was too small. We looked at an RCA, Princeton, NJ, development of the selective electrostatic storage device, and were intrigued because of the discreteness of the storage elements. If one were willing to wait, it could build up almost an infinite signal-to-noise ratio by virtue of the design. The tube had an extremely clever selection process that was devised by George Brown, who moved from Princeton to actually be my boss at Rand, and Bill Gunning's boss. So we built a ten-bit section of a von Neumann machine, with a selective electrostatic store, commonly called the selectron store. I can remember one day when Bill and I sent a telegram to Jan Rajchman, RCA Labs, who was the primary developer of the tube and co-inventor of the magnetic core, saying that we thought that the selectron had broken all reliability records, ran error-free - this was 10 bits, 256 words, not much of a memory by today's standards - for ten hours, but in fact it was a world's record in terms of reliability - absolutely error free.

We then went on to further develop software. Newell and Simon and Shaw were around. And the IP series of software devolopment -- precursors to what now would be called AI, and precursors to AI in a more general sense. I, however, focused on hardware, and a full flow 256 word, 40-bit selectron store, which ultimately ran and served us well. I then became, while keeping JOHNNIAC running, project engineer on the world's first 4096 word, 40-bit magnetic core memory. The contractor was International Telemeter, Incorporated, which was owned by Paramount Pictures, and this was during the McCarthy era where a lot of key scientists in the country gathered, who did not have clearances, who had lost clearances -- a marvelous collection of people. The specification for the memory was put together by Gunning, Ware and myself. Actually, Ware was the principal in the development of design criteria for digital computing.

NORBERG: Willis Ware?

UNCAPHER: Willis Ware. Probably the first comprehensive specification for performance along with enough detail so that people would know how to do a pretty good job of designing for reliability. And eventually it worked, and worked quite well.

NORBERG: This was done under contract to International Telemeter?

UNCAPHER: Memory yes, specification no.

NORBERG: How many machines were built?

UNCAPHER: Just one, JOHNNIAC. By this time, and long before it as a matter of fact, IBM was producing in mass quantity by 1950 standards -- 701s. We were making our mark in the memory area without much justification for JOHNNIAC other than its utility. In fact, we decided not to try to enhance its capability in any way other than having a high-speed core memory. The next big potential undertaking for the computer science research community was ILLIAC IV, which was in existence but was running into lots of problems: a little lack of attention at Illinois, and students simply didn't like it on campus.

NORBERG: I am a little confused now. It strikes me you jumped way ahead from the International Telemeter...

UNCAPHER: I'm sorry...

NORBERG: ... to go to ILLIAC IV.

UNCAPHER: Yes. JOHNNIAC was eventually shut down.

NORBERG: Remember when?

UNCAPHER: I'll have to get that to you.

NORBERG: That's all right. I can look it up.

UNCAPHER: It just wasn't feasible to keep it running, and we proved all that we could prove. We then turned our attention to a serious national problem in the early '60s. Well, I should talk about JOSS in the context of JOHNNIAC. It was thought because of the IPL languages and other influences that a user-oriented system would make a good experiment. And Newell, Simon, and Shaw, principally Shaw, decided the target would be the Rand mathematicians and to create for them a system with marvelous ways to do mathematics. And conservative, Cliff Shaw took on a large challenge by advertising what was to be known as JOSS -- JOHNNIAC Open Shop System -- as something that would be viewed by mathematicians once it was operational as a *helpful assistant*. The concept was predicated on a smooth interaction between a mathematician and the system with very little training required by the user. In fact, JOSS allowed the user to solve problems easier and faster without doing programming. It was a teletype-based system built on JOHNNIAC. CRTs were too expensive at that time. But with a color-coded dialog, namely the user typed in black, and JOSS responded in green, so there was a record of the dialog. The users were, in fact, treated to something unlike anything I believe had ever been produced in that the problem-solving environment the mathematicians could understand right away. It required very little training. It was sealed so that none of the mathematicians and others could violate the environment. As a matter of fact, when we brought it up, people looked at it, including Richard Hamming, and said, "Gee, I don't think you could contain me in your system." And we challenged him and said, "Why don't we lock you in a room for four hours. At the end of which we'll see if you have violated the system." And at the end of four hours he said, essentially, "I give up. I could not violate it." It's a sealed local system. It was a precursor to many other similar systems in language development based on the fact that it was easy to use, narrow in scope, however, and very user-friendly.

NORBERG: What was the driving force for the development of JOSS? Why did Newell, Shaw, and Simon think that it was useful to mathematicians at Rand to have such a system?

UNCAPHER: There were two perceptions that I think drove the experiment. One was, from at least 1950, when I joined Rand, there was always a focus in the computer science part of Rand toward smooth user interaction. It was just built into us. We learned it from Gunning. ... all we thought about. The other was, seeing the mathematicians struggling with Marchant calculators, not being able to use JOHNNIAC in it raw form. It was just too hard. They had to learn to be programmers. It was just a frustrating experience for them. We wanted something that would look to them as being a natural way of thinking about an aid to the solution of their problems without very much training, and without any perception of becoming a programmer, because that would have kept them away.

NORBERG: What was the range of problems that the mathematicians were working on? Were they largely things that the Air Force was interested in at the time, or was it a range of problems?

UNCAPHER: I was not able to take the time to discover the entire range, but I will give you an example. I talked to users, mathematicians, after the system had been up for awhile and I said, "What was your initial reaction?" They said they loved it because it was easy to use. I don't know how to quantify that in terms of the days required to be productive, but it certainly wasn't hours. It was probably less than weeks, but it may have been a few days, maybe longer for others. The first reaction, and a common reaction I ran across, was, "I can now solve my missile problem in 20 minutes; it used to take me four hours." A second step generally was, "I am reformulating what I want to do, because I can now add a lot more complexity." And the third was a total surprise to me: namely, they found it easy enough to turn over to their secretaries the solving of their run-of-the-mill problems, which they didn't have to fuss with at all. And the fourth was the best of all. In any kind of computer based user aid, and what was related to me is what they liked about it, and this was after months of operation with it on the part of many of the mathematicians, they were now able to dig into their bottom drawer and pull out problems that they claimed they couldn't solve. What I think they were really saying is, "We are now able to solve them," because it wasn't too hard. They were always able to, but it wasn't worth the effort. And that really ought to be the target for user-oriented systems -- to get to those problems, and to allow people more freedom to explore to a problem space without burdening them with learning too many new skills, or requiring too much training. And that's where JOSS really shined.

NORBERG: You used the verb "ought to be". Was that recognized as the appropriate verb in 1960?

UNCAPHER: No. Well, I would say... no. Well, I didn't learn it until interviewing these users after the fact. I translate it now and I have for years because of being advantaged by that experience... The goal ought to be to create a user-oriented system that fundamentally allows them to change the way they do business in quite a dramatic way such that new classes of problems are solved.

NORBERG: Is that why JOSS stands out in your mind as a major accomplishment of that period?

UNCAPHER: Well, it's the first thing I was involved with, but I think it was the first thing I ever saw that was contained, user-friendly, and extremely helpful -- just a beautiful system. And by the way, it's recorded on tape. The next project was addressing a problem that the country had from an Air Force perspective, and this was in 1961 or 1962. One of the major microwave towers of AT&T had been bombed. I think it was located in Utah. And the Air Force came to Rand and said, "We expect a heavily manned bomber attack against this country in the next few years," and we want a survivable national network, up to the point the country is severed north to south, or some such damage. So the Computer Science Department turned to me and said, "Solve it." Well, I certainly didn't know what to do.

NORBERG: How many people did you have working for you at the time?

UNCAPHER: Oh, probably 10 or 15. So what we needed was a concept paper, an "architecture" is what we would call it now. I searched for someone that had deep digital skills, that had some history of involvement in communication. And the only person remaining on my list after a careful search was a fellow named Paul Baran, who was then at Hughes, Fullerton, California. It's now called Hughes Ground Systems. And I talked to his boss, and I said, "Is he skilled in the following areas?" And I said, "By the way, I am after him for a national need, and so I hope you don't get in the way if he turns out to be the right person." He did seem to be the right person. So I invited him to come to Rand and address this problem. And the first pass was a flakey architecture that would link all the nation's AM radio stations together by an emergency box that a disk jockey would plug in at each radio station. I said, "Paul, that just isn't going to do it. I would not want to brief that approach to the United States Air Force."

NORBERG: Why not, Keith?

UNCAPHER: It would be so error-prone under stress that a lot of the disk jockeys would probably just leave the scene, and secondly, it would be awfully hard to practice. It would be so manually-prone that it just couldn't work, and under stress is the only time that it would have to work. It would probably be one of the most failure-prone approaches I could think of.

[INTERRUPTION]

NORBERG: We finished with Baran's first proposition to have the disk jockeys plug in these boxes, and you described why that wasn't a good idea. What did the two of you go on to then?

UNCAPHER: Neither of us was happy with the solution. I certainly can't recall exactly what happened, but that plan was rejected by me. I do recall having a series of conversations and we agreed to the following: that whatever approach was taken, it would be totally digital, point one. Point two, transistors should be considered as being free, zero cost. Just to challenge us, the third thing we agreed on is that there would be no AT&T. So we had the option of building something from scratch.

NORBERG: Why did you hypothesize no AT&T?

UNCAPHER: On the premise that maybe there was no way to retrofit the existing toll plant, and this would be important enough, if we had free transistors - because T-1 had been imbedded by this time at least had been experimented with - that we could do the rest ourselves. It was just to provide some intellectual freedom in terms of the approach.

NORBERG: Is that a feasible freedom though, given that the system that you were trying to replace or cause to survive was an AT&T system?

UNCAPHER: It wasn't terribly practical, but it was terribly useful as a concept to free our minds, so as not to be bound by their switches, and the antiquated technology that was in place, and low band-widths, and everything else. It was extremely helpful in that context. What came out of this exercise was a design, an architecture headed by Paul Baran called On-Distributed Communications. There were about 11 documents in the form of Rand reports. The documents depict an architecture and a system for the transportation of both voice and data, but with focus on data, in a completely digital context, understanding the value of asynchronous transmission, that is, people that want to send data want to send one bit, or a million, on their own schedule in a reliable way. And unlike voice where hundreds of bits, in terms of digital encoding, can be missing and it doesn't matter, one bit does matter in terms of filetransfer, which could be millions of bits. The concept of a distributed system was chosen as opposed to Star connected and other traditional kinds of plans, with a node being one or more computers, and with any communication medium being acceptable between any two nodes. This could be a satellite, or hardwire, or T-1, microwave, whatever was there. And the design recognized that the sending of digitally encoded information would have to probably go over some existing lines, which were designed for voice, whether they were government-owned or AT&T, and that some accommodation would have to be made to the error-prone nature of voice circuit from a digital standpoint, and that there would have to be what I would now call not only error-detection, but error-avoidance mechanisms that are inherent in Baran's design. The series of documents involved marvelous, marvelous concepts, which are too lengthy to discuss here. Paul Baran and colleagues invented what's called the "hot potato" algorithm; namely, if one thinks about packets flowing, get a packet, get it right, and send it on its way as quickly as possible to avoid having too much storage and too much processing capability consumed at a node because of the nature of thousands or hundreds of millions of packets flowing through the network. It was the first document to put forth the notion of an equivalent of an electronic envelope, a packet, with information inside the envelope, and addresses on the outside, along with parity detection capability, and then a mechanism for error avoidance, i.e. retransmit, not correction within the line because that would not be possible.

NORBERG: What date was this development going on?

UNCAPHER: 1961, 1962. The documents were, I think, published in 1964.

NORBERG: Okay, so this network is the communications network. It's not the network that we think of when we think of the ARPANET.

UNCAPHER: No. This is a totally documented approach to a national survivable network. By the time the work was finished, not surprisingly, the threat had gone away and I was unable to get the Air Force interested in developing a test bed to prove some of the principles. The documents are well worth scanning because of the marvelous notions, some of which are still not in place in this country. One of which I think Paul called poor-boy microwave. The notion was to buy dirt cheap plastic dishes from Japan, nail two of them out in rural areas to each telephone post, have redundant capability, and have them serviced by the Maid-of-Honor Division of Sears.

NORBERG: Maid of Honor. What is that?

UNCAPHER: That was, I think at that time, the dishwasher, or I can't remember... Maybe it was the clothes washer product line of Sears, and whenever the repairman was out in the rural area he could also check to see if the microwave dish needed to be replaced. These were nailed in place on telephone poles.

TAPE 1/SIDE 2

UNCAPHER: There didn't seem to be a chance, at least within the Rand context, of creating an exploratory network. In fact, nothing happened until Larry Roberts, probably in late 1968, decided to pick up the ideas in distributed communication series and those of his own, and some slightly later work by Davies at the National Physical Laboratory in the UK, who independently, I think, invented packet switching using a different set of thoughts, and out of it came the ARPANET.

NORBERG: Yes. Was there any attempt that you remember at Rand, or in the greater L.A. area, to develop what would be a prototype of a wide area network?

UNCAPHER: No. The only work that I recall is that when Kleinrock and others at the UCLA Computer Science Department certainly were aware of Baran's work and that of others on the Rand project. And because of that, and because of their own thoughts, were very helpful in responding appropriately to Larry Roberts' challenge, "Is it time for" what we now know as "an ARPANET?" In fact, this August will be the 20th anniversary of the start of the ARPANET, the initial planning for which was done at UCLA, and is therefore hosting the 20th anniversary of the event.

NORBERG: Can I take you back again to the early 1960s? What other work were you aware of at Rand?

UNCAPHER: In...?

NORBERG: Any area at all that you remember now as having been aware of in computing.

UNCAPHER: There was some work in graphics, some work in what has historically been called man-machine communications. I am sure there was much more, including bringing in a PDP-10, and in parallel with that, continuing the thrust of man-machine communication advantage. Effort was started in the use of graphics as an aid to a narrow slice of the programming problem. Ultimately, a project called GRAIL, for Graphical Interactive Language. The concept was based on a Rand development of a computer-based tablet, which was a grid and a stylus used in conjunction on a rather large CRT where the "ink" from the pen, or the stylus, if you will, was on the face of the CRT. One could imagine just below the CRT a tablet surface. If one got the stylus close enough to the surface of the tablet with the pen point, the dot of "ink" would show up on the display. There was a microswitch in the tip of the pen. One could press the microswitch and draw a line and the line would appear on the face of the CRT. The level of feedback was extreme, which proved the following concept that had been highly contested; namely, that a stylus should be used to write on the CRT, a user would know where he was via the feedback produced by the "ink." The early experiments didn't have enough feedback. And so, what the Rand tablet proved is that the feedback, namely, the "ink," or the dot, is good enough that rather instantly, the user can train himself to write on the CRT and know where he or she is using a tablet at the base of the CRT. Having developed the tablet to an acceptable state of the art, an application was needed. The one that was chosen was a very ambitious one to use the tablet and the CRT to allow a programmer to provide in flow-chart form the definition of any level of complexity for the program, provided it all remained in flow-chart form. So the GRAIL system evolved into one in which a programmer could in fact sit at console with the tablet, and nothing else, and describe a process in flow-chart form with kind of any level of depth, could easily create a box, easily connect a box to another, easily erase a line, hand print the contents of a box, and correct errors later. If he misspelled a word or a letter, there was no need to correct it instantly, just go back, erase lines, shrink boxes, extend boxes, connect them to all the things associated with flow charting. The computing required at this time was really extensive to interpret the tablet and to keep in easily accessible form a whole series of frames and pictures which had to be recalled and to provide memory and processing capability to allow any depth of definition of the language in flow chart form.

NORBERG: Which machine was being used for this?

UNCAPHER: We knew at the outset there was no storage device that was dynamic enough to service this. So there was the development of the language. There was the development of a storage device, which turned out to be about a six foot magnetic disk (analog.)

NORBERG: Six foot diameter or radius?

UNCAPHER: Six foot diameter. It may have been smaller, but it was huge. And no one had one. So at that time Ivan Sutherland was the director of IPTO, and we convinced him that this was a good investment. And he took us to a senior vice president of IBM and convinced IBM to produce the buffer storage.

NORBERG: Which would be this drum.

UNCAPHER: It was a disk.

NORBERG: A disk.

UNCAPHER: A drum, we felt, couldn't handle it. With lots of heads, it was a major development project. It was marvelous in the sense that IBM was excited about it, and Rand was sensitive enough not to tell IBM how to do it, just what the specs were. And IBM assigned Hal Martin of San Jose Laboratory to be project manager, and he and his crew had a marvelous time and produced a successful system, which produced an operating system. The system required 360/40 dedicated to one user and an IBM 1800 and the massive disk storage device. That was a lot of hardware per user (laugh) for one programmer doing flow charts, but it proved the concept. And incidentally, there is a marvelous film, fortunately, showing how the GRAIL system worked.

NORBERG: Can I get a couple questions in here?

UNCAPHER: Yes.

NORBERG: Who do you remember was responsible for the development of the tablet?

UNCAPHER: That was Tom Ellis. There was some independent work, I think it was at MIT but it may have been at Boston College by Herb Teager. But the first really successful tablet, and a very expensive one, was a Rand tablet.

NORBERG: And do you recall when that was available? Not going onto the GRAIL system now, but...

UNCAPHER: The tablet was probably around -- I'm guessing; I'll have to go back and look at the notes... but around 1966.

NORBERG: Now, who was responsible for the GRAIL system?

UNCAPHER: One thinks about project management responsibility. JOSS finally went under my direction, as did the tablet, as did the GRAIL system, and earlier, the work on distributed communication. I did not have the skills at the time to be the architect of these marvelous achievements, but my responsibility was to make sure they had it. In the case of distributed communications, Paul Baran, along with a lot of other people. The tablet, essentially Tom Ellis. The GRAIL system, Tom Ellis, and Bill Sibley, and a fellow named Gabe Groner. Did I answer that one?

NORBERG: Yes, you did. When was the system now available? When did IBM deliver the 360/40, and the disk, and so on, so that everything was operating effectively?

UNCAPHER: In the late '60s and the very early 1970s. By that time Rand was focused on helping the network, development of the ARPA network, and the PDP-10, and the operating system Tenex.

NORBERG: While you were there in the 1950s, do you remember any involvement with SAGE?

UNCAPHER: No, other than there was... Yes, there was a training requirement. And that work under Newell started and was transferred and ultimately became System Development Corporation.

NORBERG: Okay, but you were not aware of all that...

UNCAPHER: But I only was supporting some of the technical aspects of the training rooms which were established and some of the supporting technology, but I was not a major player in that at all. Except Rand was.

NORBERG: Yes. Can we shift to the founding of ISI?

UNCAPHER: All right.

NORBERG: Which must be around this time, now, isn't it?

UNCAPHER: In the late winter of 1971 I had decided to depart from Rand. I had been there a long time and felt that a change would probably be good. And I also felt constrained by the fact that by that time the company management, corporate management had switched from engineering to being managed by five economists. And the technologist couldn't possibly fare as well as in the early days, and also the priorities at Rand had changed. Although I did fight any demise of information processing, information sciences, and information technology, but it was very hard for me to get my way. Rand, at the time, was a federal contract research center, and everybody I brought on board meant somebody had to give up somebody. And there was a built-in natural conflict. It was a battle I was not prepared to go fight.

NORBERG: Yes.

UNCAPHER: But more important was the following, I think, from my personal perspective. When it was known by a few people that I was going to leave, I had agreed with Bolt, Beranek and Newman that I would open a West Coast laboratory for them and try a fling at private enterprise to the extent that BBN was private enterprise at the time. It was sort of a half-way house, really, halfway between a pure non-profit and profit.

NORBERG: Would that laboratory have been a mirror image of what they were doing on the East Coast, or did it have some specific focus?

UNCAPHER: Well, it had a focus as a major customer being NOSC at San Diego --Naval Ocean Systems Center, and some other work, and ARPANET-related activities, in the San Fernando Valley. When it was known by some of my colleagues I was leaving, a few of them gathered around me and said they would be interested if I could set up a non-profit, or university-based center, R&D center. And they were competent enough to really get my attention.

NORBERG: To do what? Anything?

UNCAPHER: That wasn't really the key thing. I guess the assumption was on my part, and I would guess on theirs, we would get together and do something useful. By this time, I hoped, I had a track record of successes in projects, and helping guide projects, or recognizing opportunities and getting the right people together and getting support for projects. I wasn't worried about what we would be doing.

NORBERG: What was the advantage to that? Why not go into commercial business?

UNCAPHER: It was my bent then, and I think it still is, that I like dealing openly. I like research; it's been my life. And I don't like conflict of interest, and I was worried, although I had not tried living in a closed environment where proprietary information was generated and protected. And at that time I thought it would be counterproductive for what the country needed. In addition to having a presumption that there would be some good researchers available, I was intrigued with the following. I had spent the previous year as president of AFIPS, and spent more time on major campuses, primarily at computer science departments, than I had had an opportunity to do previously. What I observed was a bit disturbing to me. Here, in the beginning of 1972, the major computer science departments were still largely involved in traditional forms of computer science, and I thought, as a matter of fact, 1966 or 1967 computer science was being produced, and no intent to be application-oriented, or try to discover science required for major applications, and no interest, from the university standpoint, in getting involved in exploratory systems or proof-of-concept kind of systems. So the combination of leaving Rand and having the high probability of some very good people available, I went to DARPA and said, "I would like to start a new venture. It makes sense to me. It would be off-campus, but would be totally a part of the university, live under university rules, but would have largely a full-time research staff, because in the L.A.-basin I don't think I can get the attention of faculty members. They are too busy, and too tied to lucrative consulting. I proposed 40% basic research and 60% applications focused on problems important and opportunities important to the Department of Defense. I needed a full-time staff for that, which I thought I could augment with graduate students, but I was less sure about the faculty. I didn't have anything specific in mind in terms of opportunities, but I know there were plenty. What I would like to do is make it successful enough and quickly such as to influence the thinking of mainstream (campus) computer science departments, to get them involved in applications and helping discover the scientific issues underlying applications and be relevant to national need." That was the essence of the proposal. And DARPA said, "We love it." I then went to Chauncy Starr, who was then the Dean of Engineering of UCLA, and said, "I'd like to propose an off-campus new organization, which I would like to head. I think I can get some good people. I'd like to start pretty big for a university, and at the end of the first year there will probably be a million dollar contingent liability. I'd like you to know that up front. The long term problems I would like to solve is convince computer science departments to be more applications-oriented because the country needs it, and to help discover the underlying science. If we are going to use DOD money, we ought to pay more attention to DOD problems." There was relevancy to this point at the time, because just prior to this due to the Vietnam event, students at Harvard and Stanford, in particular, had really burned the bridges between themselves and DOD. And I felt that what I had in mind would help re-establish that bridge to all universities and DOD. Chauncy Starr said, "That's a marvelous set of ideas and the time is right. And we have got a place off campus. The contingent liability is a Board of Regents issue." And I said, "What does that mean?" And he said, "That probably takes 15 months." And I said, "Thank you, Chauncy; I don't even have three weeks." So I called my first boss at Rand Corporation, George Brown, who was then Dean of the Business School at UC Irvine and said, "Here's what I'd like to do. Do you have a home for me?" And he said, "Marvelous idea! And we have got the space off campus. I would love it. It's just right, and the timing is perfect, but don't come near the University of California. The regents will kill you. You wouldn't have flexibility that you need." So I remembered that a friend of mine named George Bekey, who had moved from UCLA to USC, and was professor of EE and computer science at USC. So I called him and said, "Here's sort of a short proposal. I'm really serious about it. Could there be any interest at USC?" This was on a Monday, and he said, "Let me check." And he called me later and he said, "Could you meet with myself and the director of Grants and Contracts, Clark McCartney, the next day, Tuesday?" At the time it was a fellow named Clark McCartney, and a fellow named Jack Munushian, who was a professor of EE. And I said, "Okay." We met on campus for about two hours, the next day (Tuesday). In 20 minutes I think I had said all I had to say and the remaining time was answering questions. The following day, Wednesday, I received a call from Dr. Bekey and he said, "Could you come down and have lunch with Dr. Kaprielian?" And I said, "Yes, I don't know who he is." And he said, "He is the Executive Vice President of USC for Academics/Research. He is also a Dean of the School of Engineering." So we had lunch and Dr. Kaprielian (I had not met him before) said, "Could you give me a half hour briefing?" I said, "I think I can do it in 15 minutes." I did, and he said, "I like the idea. The timing is right, and your perception of the need is really paramount to this university and for other universities." I said, "Well, what can we do?" He said, "Well, the only disagreement I have is I want it on campus." And I said, "I can understand that, but it won't work. At least, I couldn't make it work. So if that's a rigid rule, then I suggest you take the ideas and find somebody to start it, because I don't think I can make it a success on campus. I have got to move too fast. I need space; I need to hire people, full-time researchers. And the university culture, although I'm a novice at it, I think I just couldn't make it work. And I want a very nice environment, supportive environment, which I know how to build. And I just wouldn't... I don't think I have the freedom to do it at USC, or UCLA, or UC-Irvine, or any other university. I could do it off campus." That was Wednesday noon. I gave them a list of personal references. And I said, "You better check with DARPA and see if they can support this, because I don't have any money." On Friday Dr. Kaprielian called me and he said, "The Board of Trustees at USC meets at Annenberg's Rancho Mirage tomorrow for a semi-annual board meeting and I am going to tell them *about their new institute - go*." So I proceeded to borrow space out at Marina Del Rey and Tom Ellis joined me instantly, as did Bob Balzer and a fellow named Rod Fredrickson, who was running the computer center at Rand at the time. And within 30 days we had a three-year contract with DOD (DARPA). I think it broke all records. To write the proposal, we first had to sit on the floor. We had no chairs, no desks. Actually, we borrowed one desk and one chair for a newly-hired secretary. It was necessary under California law to provide her a typewriter, a desk and a chair, but we didn't have the funds to do anything else. I did borrow $3000, or maybe it was 6, from Dr. Kaprielian -- which I paid him back in 30 days -- to launch the venture, because I did not want it to be a drain on USC or any university to which it might have been attached. We wrote a marvelous proposal, got lots of help from the community and sat on a shag rug and did it. Sent it in and 30 days later we had a contract.

NORBERG: Can we back up a minute. I have two things to ask you. Why DARPA? Did you not consider other possible funders?

UNCAPHER: After my experience with DARPA, which had been a transition largely away from Project Rand funding, which was Air Force, I was totally captivated by the freedom that DARPA had, the excellence of the people, and their ability to commit to a good idea based on the back of an envelope drawing or a telephone conversation. It was clear to me that the key computer scientists in the country viewed DARPA - this was IPTO, of course - as the best game in the country. In addition, it was my belief that I could not look great to more than one major client. I had witnessed failure at RAND, Aerospace Corp., and others trying to look great to more than one major client.

NORBERG: Which interaction had you had with them? On which project? You mentioned going to IBM...

UNCAPHER: They supplied a PDP-10 to Rand, and I was getting more and more involved with them on the ARPANET. We had a contract to help bring up a demonstration of the ARPANET, which Bob Kahn orchestrated in the exhibit halls of the Washington Hilton, which was a smashing success. So I was really getting caught up in this new love and culture called DARPA, and I loved it.

NORBERG: My second question has to do with something you told me earlier before we started the recording. That is this departure from computer science to information science, and that this concept of information science was part of the driving image behind ISI.

UNCAPHER: Yes.

NORBERG: And you haven't mentioned that in the last few minutes.

UNCAPHER: I think that once the ARPANET really came up, started to come up in 1969, and that...

TAPE 2/SIDE 1

NORBERG: I'm sorry, Keith, could you repeat a couple of those things about the ARPANET coming up that you were just saying?

UNCAPHER: Well, it's in the context of your question...

NORBERG: Yes.

UNCAPHER: ... moving away from traditional computer science to information sciences including computer-based communications. The transition that took place in my mind was driven by the importance of computer science to a very broad set of applications, particularly in the military. And it was not my intent, and I think it would have been foolish, to try to create another computer science department after the successes that MIT, Carnegie Mellon, Stanford, and others had, including Utah and many, many others. It was my view, almost a responsibility, that we provide an existence proof with respect to impact in some application areas, and what I perceived to be excellent career growth for ISI researchers that could take place by getting involved, and that, by the way, we hoped to be good enough to provide existence proof to computer science departments by virtue of professional growth, the successes, and satisfied users, that others would find this an exciting path. And I admit to my bias at the time. My number one target to influence was Carnegie Mellon; that was my objective.

NORBERG: Target in what sense?

UNCAPHER: In influencing their thinking towards applications, and even applications and exploratory systems.

NORBERG: Why? Did you perceive some sort of lack in their approach?

UNCAPHER: Like others, they were focused largely on mainstream computer science activity, and I felt because of the successes and the influence earlier of Newell and Simon, and the fact that, in particular, I had hired over the years so many of Newell's students, that if I could provide existence proof of the fun and the impact of applications, that it would probably be recognized by others. And besides, I had been chiding Newell to do something practical for a change -- a long period of time -- but not successfully. And there was no reason for anybody to believe that I was trying to create a competitor of any university computer science department. That wasn't the intent at all, because we had no students. Even though ISI still supports probably 30 graduate students, in fact, it doesn't have students. It doesn't give degrees.

NORBERG: What was contained in that first proposal?

UNCAPHER: I had better review that before commenting on it, because I have actually... It was done so quickly and immediately thereafter, I decided that we should grow, and concentrated on extensive refurbishment of the proposal. And by the way, I should say...

NORBERG: Let me skip the proposal then and say, "What happened after this first 30 days and you had some money from DARPA? What were the early projects that were developed in ISI?

UNCAPHER: There were two drivers that I had. Namely, the basic research in a kind of traditional form. That was largely in the software area under Bob Balser. And very quickly learned that although we were connected to the ARPANET, and we had a PDP-10, I was unhappy with the cost of the PDP-10. It was costing about $380,000 a year to maintain in parts and people. And I didn't like the level of service we were providing to ourselves or to people that were on the ARPANET. It was just too expensive. There was nothing unique about it. So I went to DARPA fairly soon after the start of ISI and recommended that they take one of two paths: either take the machine back, because I thought it was too costly and I would buy cycles for ISI around the network, or to provide for ISI and ARPANET users the best cycles that anybody had ever seen on the ARPANET at the lowest possible cost with a high level of user support service. In order to do this, I said I would have to have at least four additional machines and all the economy of scale-saving would be distributed (shared by all) to all users.

NORBERG: Four PDP-10s?

UNCAPHER: Four PDP-10s.

NORBERG: So we are going to get rid of something or demonstrate economy of scale operation via having four machines.

UNCAPHER: Right. But then these will be machines that you might otherwise be supplying to other universities one at a time, and it will also cost you $380,000 each. In less than a year we were up with four machines and the cost went from $380,000 down to $186,000. And we had the best cycle delivery on the ARPANET along with the best user services, primarily with Chloe Holg of ISI who was *the* interface to the ARPANET for hundreds of users. So there was an experiment that I thought was a good precursor to show the industry, the time-sharing industry, what's important about cycle delivery in terms of quality, in terms of user support, and in terms of sort of standards that would be useful. We and other contractors made sure that all the TENEX systems were exactly alike. We played a major hand in the enhancement of TENEX and then turned the improvements over to BBN under contract to DARPA to sort of - I won't call production engineer, but improve the stability of the changes and also do some documentation. And that was something that we did really well. Then it wasn't too long after that we were involved in transfer programs for the military. We also started immediately to provide for the research staff a marvelous support environment within the university setting. And I ought to say that at the outset, thinking about ISI, we chose DARPA. DARPA did not choose us. My view was that we would have to earn our way, but we were fortunate enough to have DARPA agree with the concept of an ISI and support us vigorously up front. And just to make sure that we were not institutionally funded, we did two things. I declined tenure at USC. That was just a personal choice to make sure I could continue to compete. But I asked that early on, when ISI was very young, in the first year of its three-year contract, the then director of IPTO, J.C.R. Licklider, that I wanted him to consider the following: to help review the projects and to help grow young ISI people to be project leaders. What I wanted as a goal within the first year was to be responsible for everything we did, but to grow project leaders at ISI that would deal directly with the DARPA technical program managers, and that I would not be in that loop. I contrasted this with the traditional way in which principal investigators had interacted with DARPA. They did most of the interaction. And although I wanted to continue the interaction, I wanted it in many other dimensions. I wanted to grow project leaders to be PIs on a project-by-project basis. In order to do that I asked DARPA to participate in getting these people trained. And in that process they would get a straighter look at what was going on and what was not with interactions with ISI project leaders and not just with a centralized PI. I didn't feel competent in all the areas. In effect, although we had a three year contract, I zeroed funds available within ISI each year with the cooperation from DARPA. Each ISI project leader had to convince a DARPA Program Manager to reinitiate funding each year. That was a drain on DARPA, but that was instituted relative to ISI. It hurt us but mostly it helped us. We had some projects canceled and delayed, got some "A's" and some "F's." But it helped people grow and be able to on their own defend their projects year after year. My role was to sort of zero-fund a three-year contract each year, and establish the umbrella level of funding for all of ISI with DARPA and let the various elements within ISI compete for the dollars. I think it's a good thing but it would be too much of a burden if everybody asked DARPA to engage in this procedure. But after all, there aren't many ISIs.

NORBERG: What was Licklider's reaction to that? Obviously he supported it, from what you just said.

UNCAPHER: Well, he said after the first one, which was over a Labor Day weekend, he wasn't sure he wanted to do it again. It was a terrible drain. I could understand all of that because we had some very young and inexperienced people dealing with Washington. But I thought that was okay, and he supported it beyond; it was less painful the second time for all of us.

NORBERG: Were these people attending PI meetings that DARPA was holding at the time?

UNCAPHER: No, the attendance was severely restricted at that era to PI meetings. And later, some of these people certainly did. And they certainly attended project meetings, because as is well known, many of the projects which ISI was involved with, involved several contractors in specific areas.

NORBERG: I don't know that. You will have to explain it to me.

UNCAPHER: Like program verification, for example, was set up as a project involving four, maybe five universities. So the program manager out of IPTO would hold a meeting once or twice a year and invite the contractors -- mini PIs in ISI's case -- along with people from National Science Foundation and ONR, if there was a relevant program, or if ONR was part of a contracting arm of the project. Back to the single client focus of ISI. So my major job was initially to convince DARPA that this would be a good thing, and that I guaranteed them what I thought was an advantage, namely, keeping ISI with a single contractor focus, and that there would be advantages for both of us, and they could make it turn out that way. My presumption as to why this would be attractive was based on remembering at the time, that I had seen Mitre, Rand, Aerospace Corporation, and others -- pure non-profits -- try to look good to two or more major clients and none of them had been successful. I didn't think I was smart enough to know how to make that right, so that I thought it was time that an organization like an ISI ought to tie itself it to a single contract source, see how it would turn out. It turned out very well in my judgment.

NORBERG: For 15 years now, you have been observing DARPA from a fairly close position...

UNCAPHER: Yes.

NORBERG: ... as a contractor. What changes have you observed over the years, both in terms of the kind of programs that DARPA involves itself in, or IPTO and its successor, involves itself in, and also in terms of how they go about interacting with their community?

UNCAPHER: Probably at the end of Licklider's first term and later, it was a small organization with three or four people - I don't know how much money was available - and the general conduct of business was to show up with a good idea and see if it could be funded.

NORBERG: Now, did you ever do that in the 1960s?

UNCAPHER: Yes, when I was at Rand. The nature of the organization then was, I think, probably appropriately but not adequately depicted in the following sense. A small number of extremely bright people who were unencumbered by the rest of DARPA, and they were helped immensely by both the management and the administrative structure of DARPA itself, and just free to respond to what they thought was important to the country, and to computer science, and to the military. They moved quickly, and I do remember making telephone calls and saying, "I think this is a good idea." And in one case I remember, "Proceed. Send me a proposal. But proceed, because the proposal is a detail." It was fascinating; it was useful. The level of innovation just soared, and it was easy to attract very good people both at IPTO and at contractor sites. There was stability of funding as long as there was delivery. There were checks and balances, and people had to compete for ideas and results. A bit later it became a little tougher for two reasons. One, the size of IPTO grew relative to the other directorates, and that caused the director of DARPA to pay more attention in a management way, but probably no other. And the second thing is the contracting became a little more complex, and at some point in time - I can't remember when - the Mansfield Amendment hit, and that was pretty dramatic in terms of the requirement for justification in the context of military relevance. Probably it had some advantage, but mostly I think it disadvantaged the system including DOD. The next change that I saw was...

NORBERG: Can I ask a question here? When you say the IPTO office grew in size, did you mean staff, or did you mean money, or both?

UNCAPHER: Mostly dollars, but minor extensions of the staff, because there was a limit as to what people could do. In the early days, they could handle quite a bit because the contracting was so easy. A fellow named Al Blue took care of contracting. He was the corporate history of IPTO and he was an enabler. If the director of IPTO said, "Is there some way to make the following happen?", if it was legitimate, Al Blue found a way. He was just absolutely marvelous.

NORBERG: In the early 1970s, approaching the time when you founded ISI, when you went to IPTO, did you always talk to the program manager, or were there occasions when you talked to, what I will call scientific officers within the office?

UNCAPHER: First, the program managers were the scientific officers. I had a sustained involvement with whoever the director was. But I also had somewhat frequent meetings with every program manager, because it wasn't very long before ISI was large enough, such that virtually every program manager had some involvement with ISI, and I wanted to make sure things would always go reasonably well, if not better.

NORBERG: And who were these people? You mentioned Blue, and, of course, we talked about Licklider.

UNCAPHER: Well, Blue was on the contract side. The crew has changed over the years dramatically, so that I am not sure that's terribly relevant. But at one point in time there were only a couple of such people. And when Ivan Sutherland was there, Larry Roberts was one, and Bob Taylor was one, and Barry Wessler was one. That changed out over time. Several military people migrated into the office as scientific officers from time to time. You ask about changes. I think the next dramatic change that I wanted to see take place was not, in fact, in place, but concerned me. And it was when Lukasik was a director of all of DARPA. It was my view... Larry Roberts was then head of DARPA/IPTO at the time... that the impact of information processing had really grown in actual ways inside this country and inside the Department of Defense, and that, somehow, the growth in IPTO ought to reflect that, I'd agreed to help Larry Roberts help you [referring to Lukasik] make sure that it is the case and that IPTO, which I often depicted as ARPA at the time, is in a unique position to create technology, to transfer technology, once it finds out about more military needs. But that will require more money, and that was a principle that was imbedded in my mind along with the importance of information processing to DARPA itself. At that particular point in time it seemed important that the director of all of DARPA know a lot more about IPTO than had been true in the past. In my realm of experience it started with Lukasik. I happened to be talking with Larry Roberts on a Monday night about 7:00 p.m. when Dr. Lukasik showed up. I had not met him before. I said, "Gee, things are really exciting here. I think that this agency and you personally in the Department of Defense would benefit enormously by your knowing much more about this technology, because it's going to be the premiere technology for the country and for DOD for a long period of time." I said, "It seems a bit strange to me that you're not on the ARPANET because it's absolutely marvelous." And he said, "Well, I'll try to get an account." And I said, "You've got one. I'll go and make it happen." So sort of instantly he was on the network. I began to spend time with him and convince the director of IPTO of the importance of educating the Director of ARPA - I think that's an absolute responsible term in this context - about the special nature of information processing, its relevancy to the country and to DOD. It was my judgement at the time, that we are not apt to have a director who is a computer type. So we had a responsibility to help train these people to get them a feel for the special nature of what it means to the rest of DARPA, to the country and to the military. And that was the first. By the time Steve Lukasik left, I felt in some ways he was one of us, because he really got excited and he spent a lot of time... and people in IPTO spent a lot of time with him, educating him, selling him programs. By this time there was enough growth in the office that it required his support of new initiatives and going to Congress and getting additional money. And he had to really understand something about the value of this enterprise, the specialness of what it could do and help defend programs. So it meant that the director had to have more knowledge. And that path followed. And so in my judgement it is a key part of the responsibility of the director of IPTO. You asked about changes; that was a new requirement that had not existed before. Driven by required growth to track the impact of information processing, the office continued to grow. Bob Kahn became director. New programs were initiated, and extensions of communications, extensions of the program in terms of DOD potential and requirements, both. The first transfer program; a military message experiment, followed by one at Strategic Air Command Headquarters and later Air Force Logistics Command.

NORBERG: What is the message program?

UNCAPHER: The military message experiment. That was the one that took place at the commander-in-chief's command center on Oahu - commander-in-chief for the Pacific.

NORBERG: Oh, you mean the project you were describing to me earlier, that you were one of the principal people involved in.

UNCAPHER: That was the first, as I recall, of a real transfer of an experimental system, not production engineering. And others followed, and to logistics, and to the Army, the Simnet project.

NORBERG: Now, how were these transferred? Did these things become requirements that ISI worked on, or someone else worked on, and the actual job got done then through DARPA's support of each of these projects?

UNCAPHER: Well, in the early days, I think they were largely suggested to IPTO as ways to get closer to the military to create technology that would be helpful, but realizing that it wasn't possible, and it was probably inappropriate then and now for DARPA to do production engineering, in fact, to do traditional kinds of prototyping because it's too expensive. But in the context of information processing, there's a lot of prototyping that's not all that expensive. Contrast an E-Mail system versus prototyping a swept wing aircraft. There's a huge difference. The former still fits IPTO and ISTO. So that was a change. With growth grew problems of recruiting and getting enough program managers, and competition in contracting hit years later, and that's really taking its toll on the entire enterprise. Contract time went from probably under three months now to probably closer to 16 months. And now the resulting administration is a factor of ten more in many cases.

NORBERG: I should think this would seriously impede the work of an office like IPTO, now ISTO.

UNCAPHER: It does in a very serious way. And there are times -- I don't remember specific examples clearly enough to relate them -- where the contract cycle is longer than the half-life of the technology that was proposed. And that's serious. That is truly serious for the country.

NORBERG: Now, that's not just a question of money either, is it?

UNCAPHER: It has nothing to do with money. I think it has to do with a bent in Washington that's existed for a few years now, which I describe in a general way the following. There seems to be a bent towards reducing virtually everything to the lowest common denominator. And what that means in terms of competition and contracting is, place money where there's political advantage, and spread the wealth. That's not how DARPA created its successes. It went to the best people independent of what they were. And that's at odds with what is an often practiced lowest common denominator approach today, if that's an accurate assessment of the situation. But porkbarreling is still increasing, and that's a lowest common denominator of a different form. That's a more selfish lowest common denominator...

TAPE 2/SIDE 2

NORBERG: Did you just say it took its toll on the kind of people you can get into these positions?

UNCAPHER: Yes, because they do not have the freedom any longer to do what previously was so exc iting.

NORBERG: Is it just a question of freedom thought, or is it also a question of salary? Because if you think about what some of the computer scientists can make outside of the academic world, or in some cases, now, even in the academic world, it can be almost double for young people what it would be if they went to work for the federal government these days.

UNCAPHER: In terms of recruiting, I think there are three issues. Salary generally is raised as one. The second issue: is it my turn? And the third issue is, what sort of a career step is it for me? Maybe there are four here, and then what could I do as a candidate for program managership? Not me, but I'm generalizing. It's probably still true, in spite of the pain and anguish of dealing with the world of competition and contracting, that a program manager that's really bright and takes advantage of two years at DARPA will still find that it is equivalent to the best new doctorate that a person can have, and along with it, influence the future in a programmatic area in a very substantial way, while dealing largely with the cream of the crop of the research community in this country.

NORBERG: Yes. But people clearly don't see it that way.

UNCAPHER: Well, that is on-faith value up front, although I must say that a few years ago, I was interested in getting Bob Englemore from Stanford into a program manager slot. And his boss, Ed Feigenbaum, called me one afternoon and said, "He's decided against it." I said, "Don't accept an answer. Can you make an appointment? I need an hour with him this afternoon. I'm going to be there." And in 30 minutes he changed his mind, principally on the basis that he accepted my judgment, I think, that it would be equivalent to getting the best Ph.D. in the world. After he spent two years he wrote a marvelous letter to the editor [*ACM Communications*] saying that's exactly what it was. "Precisely right. That's what I was told, and that's what I got." So the money element is generally there, but not so much at the program manager level, but certainly at the director level. There is the issue of the return investment and the influence. Those are really positive. And they are all conditioned by the nature of the director and what's perceived as the opportunity. I think the perception of opportunity has been declining lately, probably even substantially.

NORBERG: Due to what, Keith, do you think?

UNCAPHER: Well, if one looks at it today, July 1989, the entire federal system is closer to a zero-sum game than I have ever known in my life. Two, competition in contracting makes it more difficult. Three, porkbarreling is, in spite of the short-term benefits for the recipient, a really bad thing, and that scares a lot of people, and it diverts money away from the real competition. And that's perceived as a recent thing. Quite recently, there have been no new programs launched by ISTO of the strategic computing level. And that's a worrisome thing to a lot of people -- against the national need. And I think the other thing is that virtually everything that is funded now that's brand new is well below the critical threshold level. HDTV is an example. So far, I would say that includes the national very high bandwidth research network. There is almost no money allocated. And the other thing that probably worries people is major programs in the past, strategic computing is probably the last one, as something generated within IPTO, sold by DARPA to Congress, and launched with the real structure influenced by key researchers in the country identifying the key problems and the research agenda all very appropriately. That has not happened in a long time.

That's a worrisome thing.

NORBERG: Where is the community? Are they not saying anything publicly that would try to reverse this trend?

UNCAPHER: Some of my colleagues have the following view -- and I guess, in part, it's mine -- that in a zero-sum game, or in dealing with the Congress and an executive branch in a total government scene that has no national priorities, the computer science community is poorly equipped to present its case, as opposed to the premiere case which is cited to me every time a physicist can get what they need, and we don't. And we're still either too young, or too naive, or silly. We can't agree on two or three key thrusts with enough programmatic focus to get the attention of people, and then do all the lobbying or whatever is required that the physicists do to get their priorities injected even into a zero-sum game. It's a field that, for any number of probable reasons, isn't yet organized to be a competitor in this contest. This is a somewhat widely-held view.

NORBERG: I see, because if you think about the structure of the professional physics community, it's very tight. It isn't dispersed among 20 organizations. It's built right in there in AIP, and everything is tightly held. That certainly would explain some of their success. I would think another element might explain some of that success. And that is the fact that the physicists have been strong in the National Academy of Sciences and the National Research Council for a very long time, and the computer science people have not been, as far as I can tell. And that may say something about the nature of computer science more than it says anything about the nature of physics.

UNCAPHER: That may well be true. It's an interesting thought.

NORBERG: Let me ask you one more question, and it will give you a chance to end on a note of exultation rather than on the down side. Keith, what would you cite are IPTO's successes?

UNCAPHER: This is not an ordered list, but I think it has been the major reason in the outstanding computer science presence in this country. And the key elements of that are Carnegie Mellon, MIT, Stanford, Berkeley, Cornell; it goes on and on. IPTO created for the United States world class computer science departments. It has created the excellence, the presence across a whole spectrum of really key levels of innovation and programmatic issues of in this country that would not have existed otherwise. It has probably been responsible for attracting a lot of people because over most of the years of IPTO, funding was stable. There were those that were responsible and had good ideas. Ideas were easy to translate into contracts and grants relative to now so that really helped. IPTO had enough freedom to explore areas which could legitimately be justified as being militarily relevant, while knowing at the same time if there wasn't a transfer path to the military, there surely would be to industry, and therefore indirectly to the military. And that's been a big help to the country. From time to time, there has been a vector produced within IPTO itself. The ARPANET, time-sharing, support of Tenex, and ultimately, Tops-20 (the follow-on to Tops-10, by DEC, which IPTO orchestrated), the move towards standard LISP, transitioning appropriate parts of the research community very rapidly to a LISP-base environment where they could be much more productive, and doing it quickly and well, and being innovative in the acquisition of hardware at an awfully low cost, and delivered in a form that was useful to the research community that didn't deter them in terms of debugging forever. Researchers were given a new environment and it helped to shape it; something that hadn't been done before. Networking, the ARPANET itself, the Internet, exploration with low cost satellite concepts, packet-radio concepts -- still not looming large, but will really be very big technology transfer in the long haul. Software, making computers easier to use, the graphics area, technology injection into the military, MOSIS, as the first instance that I know of as a kind of contemporary component, and a very important one, of what I would call a new level of computerized commerce directed towards a technology that will allow the wafer producing industry to restructure itself when it wants to in an advantaged way. But long before that capability was recognized, it gave graduate students starting at 60 universities hopes of seeing their designs in chip form while they were still students, and at low cost and ultimately very high quality. And that was DARPA initiated. As a result of that, there is a cadre of custom, semi-custom designers. The importance of that is that most of everything around us has a major electronic component no matter what it is as a consumer product with an ever-shortening half life, product life. So the value to quick design... And by the way, the short half life means, in most cases, low volume too, and still there is a requirement for profitability. Changing the culture through a LSI design has been a marvelous advantage for this country and MOSIS is the enabler. To get people to think about the consequences, or more importantly, the advantages of living in an international community, and the consumer market product especially, with very short half marketing lives yet having very substantial customer appeal, which I define as excellent, form, fit, and function, and making money at low volume. All dramatic changes for the U.S. industrial design culture, manufacturing culture, assembly culture, and marketing culture. And I think DARPA, in particular ISTO and IPTO, have really provided a technology base and a set of aspirations to make all of this happen.

NORBERG: Do you think this is true in other areas besides computer science and technology through other DARPA programs? I think about materials; I think about different kinds of university programs to develop the inter-disciplinary activities and so on.

UNCAPHER: Probably not in terms of university programs because -- I'm guessing at this, but across the last 15 years IPTO, and later ISTO, probably funded 90% of all the university-based research supported by all of DARPA. So there probably can't be that much influence because of lack of investment. Potential? It could be just as great in other areas, but it has not been exercised the way it has been with IPTO and ISTO.

NORBERG: How about other military organizations? Has the Air Force continued its strong support of basic research and development since DARPA came into existence?

UNCAPHER: Yes, probably even grown significantly in terms of investment in what we call computer science, but not at universities in the computer science areas, as AFOSR, which has a small budget in this domain relative to DARPA. That's true for ONR also. There are centers, like RADC, which has a pretty big budget. It has to do things that are considerably more relevant and shorter term in impact. And there are counterparts of this in the Navy and the Army.

NORBERG: Because it seems that the United States is still recognized as the world leader in aviation, in the production of both military and commercial planes, and that even the Japanese at least publicly state that that is not going to change. But yet, no one agrees today that it's not going to change for computer science, that we are at a disadvantage to the Japanese, and that we may continue to be at a disadvantage to the Japanese.

UNCAPHER: I don't know if we are in terms of computer science and technology, or more broadly, information science and technology. I think we have a big edge in terms of research investment, although that's probably dwindling. And they're still better at implementing a lot of stuff faster. In terms of the transfer process, I think that there is some evidence that Japanese industry has a better appreciation for the R&D investment in our area than U.S. industry does -- maybe not a better appreciation, but a more effective means of taking advantage of the investment in science and technology faster and better than some components of the U.S. industrial base.

NORBERG: Yes. Considering what you said about ISTO, that doesn't seem like a process they could help these days in improving that transfer mechanism.

UNCAPHER: Now which? I'm sorry.

NORBERG: I'm not saying the question well here, Keith, is the problem. Considering what you said about DARPA these days, the changing character of contracting, the insufficient funds and programs, the type of people who are there, they are not likely to reverse what appears to be a trend. They're not likely to be able to improve the transfer mechanism at the moment to get the basic science and technology into industry and into products and out onto the customer's shelves.

UNCAPHER: ... Certainly can, but it requires a level of vision and trust, and within DARPA's resources, it probably is going to have to be more astute in terms of areas of focus to make sure that the probability of a big win has some chance of occurring, because the country needs it. And ISTO is still a major source of technology. I think there are many people that agree with me that the minimum energy path to dramatic change, which many of us would like to see, is only possible through a crisis. We don't have one. I said minimum energy, not anything that anybody would like, but if we had a crisis, we'd do much, much better in making a dramatic change. In fact, this country is just beautiful in adapting to a crisis. But a creeping change is something that we too easily ignore, personally, and corporately and nationally.

NORBERG: But when IPTO had its great successes there was no crisis, in the same sense that you're suggesting that it would help today.

UNCAPHER: Yes, but it didn't need one. A crisis had created DARPA. And IPTO was created and protected by the crisis, and sensing a need, was able to gather really bright people both as directors, program managers, instant contracts, support by the director, and freedom to explore new areas. And all of that had a very high payoff. And that's what we would all like to see again. I'm sure everybody on the hill would like to see this, and in the executive branch, and certainly within DARPA. None of us can find a way to create that again. And it may be that the only way that those freedoms that formally existed can exist again in a minimum energy way is a crisis, saying, "Hey, we have to reorganize our priorities, forget competition and contracting, and get on with the job against some priorities which generally come out of a crisis." We don't have that advantage. What we have got to do is contemplate our navels and say what is right for the country. And right now, it seems to me that compromise is the major thrust, and it's not getting us where we need to go. I suspect we are all unhappy with it, but...

NORBERG: Well, thank you very much, Keith.

END OF INTERVIEW