An Interview with

RONALD B. OHLANDER

OH 175

Conducted by Arthur L. Norberg

on

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Marina Del Rey, CA

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Abstract

The interview begins with a discussion of Ohlander’s background in the Navy and his education in computer science in the mid-1970s. From 1981 through 1985 Ohlander was a program manager in ISTO. He describes the programmatic, administrative, and technical management of the artificial intelligence program while he was there. Topics covered include the AI contractors and programs, the Strategic Computing Initiative, and the Competition Bill. In the last part of the interview, Ohlander talks about the necessity of the long-term funding of AI provided by DARPA and the AI influence on computer science, industry, and the military. 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.
RONALD B. OHLANDER INTERVIEW

DATE: 25 September 1989
INTERVIEWER: Arthur L. Norberg
LOCATION: Marina Del Rey, CA

NORBERG: Can we begin, Ron, by your telling me something about your educational background? It's my understanding that when you were at DARPA you were still in the Navy. Is that correct?

OHLANDER: That's correct, yes.

NORBERG: How does that play in the educational background and the time spent in the years, say, late 1960s through the 1970s?

OHLANDER: Well, I don't know that there's much point going into my career before I got my degree in computer science. It might be interesting, but it doesn't seem germane. I went to Carnegie-Mellon starting in early 1971. And I went there for a little over four years, basically getting my degree in computer science, AI, in... It was actually awarded in the summer of 1975. I really finished everything earlier in the year. Then I went not so happily off to Japan.

NORBERG: Had you had degrees in computer science before you went to Carnegie-Mellon?

OHLANDER: Before I went to Carnegie-Mellon I had gone to the Naval Postgraduate school, and that's where I started in computer science. I was there about a year and a half, I think, but I never did get the master's degree. I just went to work at Carnegie-Mellon, and then went into a direct doctoral program. So I did have some education, but, you know, not a degree from the Naval Postgraduate School. So then I went to Japan for about three years. Then I came back, and while I was there... Well, it's a curious thing. At that time they were having all kinds of trouble with Navy ships and engineering, and somebody had decided... I was in a group they called "engineering duty." Now this was not ship's engineering. This corresponded to the more regular type engineering - naval engineering, electrical
engineering, electronic engineering, what have you. And I was in that group of the Navy, engineering duty. And it was on the basis of going to the Naval Postgraduate School and getting a degree that was going to be computer science, that they reckoned I should fall in the engineering field. So anyway, while I was busy getting my degree at Carnegie-Mellon, they decided that they needed people with greater technical skills on the ships as ship's engineers. So I got tapped to go to Japan and serve as a chief engineer on a cruiser. It didn't make much sense, but the military does that fairly often. So I served there for three years. And while there, they started reviewing that whole issue of whether engineering duty people should be ship's engineers and they decided, "No, they should not be," that regular line officers who have careers on the ships should do that. That was too late for me, but what did result was that one of the admirals who was in charge of looking at the situation had also determined to look at the employment of engineering duty people within the whole Navy and what they were doing. One thing he was concerned about was that people got used properly, depending on their education. So I came back and worked for him and I worked in mostly software engineering for the next three years at what was then called NAVELEX, Naval Electronic Systems Command. Now it's called SPAWAR. I should mention, before I went off to Japan, and then, also, before I went into NAVELEX, DARPA tried to get me both times. It wasn't in the cards at those times. I needed some ticket punches and things to make my next rank and that kind of thing. But Admiral Fowler, who was the person I worked for at Naval Electronic Systems Command, worked a deal with Fossum at DARPA that I would go over there for at least two years. And so, in 1981 I went over to DARPA where I worked for just about... almost exactly four years - a little over, perhaps... at DARPA, and worked at IPTO the whole time. So that's how I wound up at DARPA.

NORBERG: Yes. Can we go back for a moment? What contributed to the choice of going to CMU? How did you come to decide that?

OHLANDER: Well, I was at the Naval Postgraduate School and I knew I wanted to go get a Ph.D. I was looking at several places. I knew I wanted to work in artificial intelligence, and a person came there - I can't recall his name right now - who had just gotten his degree from Carnegie-Mellon and joined the faculty at the Naval Postgraduate School, and he said I ought to go there. And I said, "Well, I did want to go there and I tried to, but when I sent in my application they turned it down because it was not in the right cycle. Everybody starts in September and I was trying
to get in there in January." And he said, "Don't pay any attention to that. Call Alan Perlis because he makes his own rules, and all you did was talk to some graduate office back there." So I called Alan Perlis and he said, "Sure, come on ahead." And that's how I wound up at CMU. I knew I wanted to work there because they had quite a reputation. They were one of the few schools that had a reputation in the area of AI at the time. I had also looked at Stanford, but I kind of ran into the same thing there. I talked to Don Knuth. He wasn't the chairman, but he said, "Well, we've got a regular program..." He said almost the same thing I had heard from CMU, and I probably could have turned that around if I had gone back to the chairman when he came back but actually I decided to go to CMU and certainly did not regret it. I wound up at the best place, I think, even compared to Stanford and MIT.

NORBERG: What sort of program did you find when you arrived? What sorts of courses did you have to take? What was your research group like?

OHLANDER: Well actually, the thing I found was that they were extremely flexible, which was fortunate. Almost the first person I was introduced to after I met Alan Perlis was Raj Reddy, which was fortunate because he turned out to be my advisor. And so I got started in a program fairly quickly in the vision area, which was fortunate because the Navy only gave me four years, and it's unusual to get a degree from any of those schools in four years, or anything, you know, under five. Five or six is more the norm, but I didn't have much choice. It was fortunate that I got Raj Reddy because we were able to focus quickly and get on with the work fairly soon. And he was very helpful in getting me out on time. So basically, as I said, I got introduced to Raj Reddy right away. And he introduced me to looking at a number of things and suggested work to get into. In the meantime, I didn't have to take any courses; all I had to do was pass qualifiers. But it turned out that each school was a bit different so you really had to take the courses. And I went through that procedure and passed the qualifiers and got into, you know, substantive Ph.D. work and by that time I knew what I wanted to work on.

NORBERG: What was the general problem that Reddy was working on and how was it divided up among the graduate students?
OHLANDER: I have no idea... Well, that's not quite right. He was working on two things that I would characterize under one topic and that is machine perception. And he was working on speech understanding and image understanding. The image understanding program was much less substantial than the speech program. The speech program involved a number of people, strong funding, quite a few graduate students, etc. In regard to the vision program, I was the only one in it at first, and we had some funding under NSF, as I recall, on the Lobster Project. I started in that area and then Keith Price came a little bit after I did, six months after. He also started working in vision. We were the only two in the vision program. I actually got the first Ph.D. from Carnegie-Mellon in image understanding. And somebody else had joined CMU, Ruben, just as I was leaving. John Kender was working for me a bit when I needed more help in processing, and he went on to get his degree in image understanding. Now it’s a very strong research area, but I came out with the first degree in that area.

NORBERG: When you say first degree, you mean from Carnegie-Mellon. Or was that...?

OHLANDER: In image understanding, yes.

NORBERG: Period. Of any school...

OHLANDER: Oh, no.

NORBERG: ... or Carnegie-Mellon?

OHLANDER: No.

NORBERG: Who else was doing work at the time in image understanding?

OHLANDER: What other schools?
NORBERG: Yes.

OHLANDER: What other research places?

NORBERG: Yes.

OHLANDER: Well, Rochester was; MIT was; Stanford was doing some things in robotic vision. Well, there were others. SRI certainly was; USC was. Yes, USC was doing work on image processing when I started. By the time I ended up, UMASS had come along and they were doing work on image understanding. And there were others. There was work going on in the area. There was healthy work going on.

NORBERG: You mentioned that it was funded by NSF.

OHLANDER: Well, the Lobster Project was. Actually I worked under something else. [interruption] Let's see, where were we? We were talking...

NORBERG: I asked you about the NSF support. You had just mentioned the Lobster Project.

OHLANDER: Oh, yes. Actually, it turned out that I did not work much on that effort. I started to, but then I got into something else that was more of interest to me, and so I didn't work on that really for any length of time. Actually, I co-authored a paper on it, but my main work was not under that project. Since the Navy was paying my way I never...

Well, they were getting some funding from DARPA. I don't know precisely how much. But since the Navy was paying my way, they didn't have to support me. So all they had to do was give me the machine resources. And in fact at that time they may not have actually had funding from DARPA; they may just have been letting me use DARPA machines.

NORBERG: Yes. The 1978 publication on picture segmentation quotes DARPA as being the supporter in part.
OHLANDER: Yes, so that might have been machines. I don't know precisely what. You know, in your graduate student days you don't pay much attention to funding as long as you had the resources.

NORBERG: Indeed not. (laugh) Right. All right, so we now have you getting your degree in the middle of 1975 and leaving for a three-year tour of duty with the Navy, partly in Japan and partly in...

OHLANDER: Yes. That was actually in the spring of 1975 I left for Japan.


OHLANDER: 1978...

NORBERG: If you showed up at DARPA in 1981.

OHLANDER: Yes, that was when I was at NAVELEX; I mentioned that earlier.

NORBERG: Okay, so I have it straight now. All right. Now, you went to IPTO then in 1981.

OHLANDER: Yes.

NORBERG: Which part of 1981?

OHLANDER: May of 1981.

NORBERG: What was the structure of the IPTO program like when you arrived? What is it you were assigned to do?
OHLANDER: When I arrived there, I actually took over two programs. There was a strong AI effort going, which Bob Englemore was the program manager for. And then there was the ACCAT effort, which was a kind of an applications program that had been under Floyd Hollister to start with and then taken over by Bob Dietzler. He had left to go to ONR, so I took over both those projects. Now, the ACCAT was winding down. There were some things salvaged out of ACCAT that were AI and moved into the AI program, and the rest was just wound down. In fact, when I came on board I put that one to bed. And then I was in the AI program the whole time I was there - four years. Some time after about a year, I started helping Bob Kahn build the AI portion of the Strategic Computing Program. And that came into being about 1983, and I initiated that whole effort in the strategic computing. Actually, if you look at it, what's going on in AI today at DARPA, I don't know which percentage, but a very significant percentage of it, I started and inherited some from some others too. Some things I did not start. Most of the SC effort I started and it continues today. Some of the things I inherited from others are still continuing, because they involve very good researchers.

NORBERG: Can you be specific about some of those programs? What they were designed to do; who the contractors were as you remember them, and what sort of interaction you had with the contractors.

OHLANDER: Well, that's kind of a tall order.

NORBERG: Yes, it is.

OHLANDER: When I first took over the AI program I would characterize it... It included efforts for the vision program, the same one I had been under in years before. There was a natural language effort, which was pretty strong also. Those involved multiple contractors. Let me go see if I can recall the natural language... The natural language effort included SRI, BBN. In fact, let me recall... Why don't you turn that off?

[INTERRUPTION]
OHLANDER: SDC, I started. NYU. Now, who else? Oh, Roger Shank, yes. Roger Shank at Yale was also participating. And that was a pretty strong effort. There may have been one or two others I don't recall right now, but those were the main players, certainly. Okay, now, in addition to that... I'm not sure. How do you want to do this? Do you want me to take those...? Do you want to take each program out into the future, or do you want me to...?

NORBERG: Go ahead. You just say what you remember first, and then I'll come back to the individuals.

OHLANDER: Well, let me cover each main category, how it was when I started, and how we expanded it, because that's the easiest way for me to do it. Then when the strategic computing came around, there was some determination that natural language had pay-off in the near-term and that the program would be beefed up to give that pay-off. So at that time we added the University of Massachusetts at Amherst. We added System Development Corporation. We added NYU, Ralph Grishman.

NORBERG: Didn't you mention these before as part of the natural language program?

OHLANDER: No.

NORBERG: When you were looking at the screen I thought you were...

OHLANDER: No, I was just mumbling to myself. No, when I was looking at the screen I was determining who came when.

NORBERG: So who in the natural language area besides Yale?

OHLANDER: To recapitulate, when we started, the main players were SRI, BBN, and Yale University. When the Strategic Computing Program started, we added the University of Massachusetts, the System Development
Corporation, Paoli, NYU - Ralph Grishman doing that work, the University of Pennsylvania - Arvin Yoshi, and ISI. Of course, at that time I had not the slightest idea that I was going there.

NORBERG: Yes, of course.

OHLANDER: And that was pretty much the natural language program. After that... Excuse me, other than that we had the vision program. Now the vision program... Give me a moment here... was originally started with Carnegie Mellon University, Rochester University, MIT, SRI, University of Massachusetts at Amherst, USC, and Columbia University. That program was expanded under the Strategic Computing Program to include Hughes. These are in addition. The others were still participating. Hughes, AI and DS - Advanced Information and Development Systems, I think it is. They actually changed their name for obvious reasons. Their moniker was AIDS, and...

NORBERG: Yes.

OHLANDER: General Electric, Honeywell. Okay. One thing I should point out about both these programs is that there was a basic program which included the basic research. And I mention those organizations that were included in that. Generally they continued that work, and some in addition got some additional SC work. But the basic work was meant to be that: very basic, 6.1 research. And the SC work was meant to be applied, 6.2. Now I think since then that this whole thing has gotten merged so they don't make that kind of distinction. You know, people just have a program. But when they first started, some of them had basic research in SC and some of them just had an SC program. And some of them probably just had basic research also. Then there was a number of efforts. [At this point, I am referring back to the initial AI program as it was when I came in 1981.] There were some other efforts. There were some efforts in distributed AI. These were not as well-focused and clear-cut as the speech and the... I mean, not the speech, the natural language and the vision, but they were areas that were thought to be important also. So for example, in distributed AI there was University of Massachusetts, Amherst. There was USC-ISI, and Rand Corporation doing some work on distributed AI.
NORBERG: Ron, what is the difference between AI and distributed AI in this context?

OHLANDER: Well, it's just that distributed AI involved multiple processors, that's all.

NORBERG: I see.

OHLANDER: And you were looking at massive numbers of operations that required distributed types of processing. Then there was other work at some major universities that were getting into a number of things. At Stanford, for example, there was a good program with Ed Feigenbaum, a healthy program with Feigenbaum, and he was working on mostly expert systems at that time. In addition, at Stanford there was John McCarthy. We funded John McCarthy, who was doing work in formal reasoning, logic. At CMU, there was some speech work that was actually funded by Duane Adams at the time. And then there was work that Allen Newell was doing in SOAR, general problem solving at the time. And we were also funding some chess work there. At MIT there were a number of things going on in addition to the vision work. There was learning work funded with Patrick Winston. And there was some planning work funded with... I can't think of his name right now... Hewitt... Carl Hewitt, and other kinds of things that might come up with graduate students, smaller efforts. CMU, MIT and Carnegie-Mellon tended to be the big three.

NORBERG: And Stanford?

OHLANDER: I'm sorry...

NORBERG: You said Carnegie-Mellon and CMU twice.

OHLANDER: Oh, I'm sorry. Yes, Stanford. CMU, MIT and Stanford. Those happened to be the big three. And then at that time we were also looking at Columbia and Berkeley to make them centers of excellence. And that wasn't just AI but it included other things also. In about the 1982 time frame, I funded Bob Wilensky at Berkeley in the area of natural language and cognitive reasoning, knowledge representation, that kind of thing. We also funded some
natural language work at Columbia - Kathy McKeown. And we had some work going on with Drew McDermott at Yale in spatial reasoning. And we did some work with... Let me think. You might want to.... I am missing someone else, and I can't think who it was.

NORBERG: Can I ask you an incidental question here that just came up?

OHLANDER: Yes.

NORBERG: Was the concept of centers of excellence still very strong in IPTO when you were there?

OHLANDER: Yes.

NORBERG: Because I haven't seen much about it after the early years.

OHLANDER: I think...

NORBERG: As an explicit idea.

OHLANDER: My idea of it now is it kind of happens by... I mean, it's still there. They still have the centers of excellence, but I don't know that there's a directed program on centers of excellence, as there was when I was there. It was a very conscious program then. Now I think it's just the residue of, you know, what was there before. I think certainly under Jack Schwartz he did not tend to have centers of excellence.

NORBERG: One of the things that I saw in the early documents was a determination that the centers of excellence would have a certain focus. For example, when Licklider wrote a memo on the idea initially in 1963 he wanted a place like MIT to concentrate on information technology and wanted Carnegie-Mellon to concentrate on what he was calling at the time "theoretical aspects" which we would not see as the same definition as we use now for the phrase.
But what I heard from you just a few moments ago in the attempt to improve activities at Berkeley and at Columbia, a broader-based program...

OHLANDER: Yes, that would all be called information technology. In fact, we wanted them stronger across the board, not just in one particular area. Although, you know, it's kind of ironic what you mentioned about Carnegie-Mellon, because Carnegie-Mellon actually was not strong on theory.

NORBERG: Well, I don't think he meant the same thing as we now mean by theory.

OHLANDER: I see. Because Carnegie-Mellon was very applied, and I think when... Actually, I think when Nico Habermann came in, he made a very conscious effort to increase the formal, theoretical aspects of their work, and I think he's been quite successful in doing it. At that time, I think that places like Berkeley were more known for the theoretical aspects and perhaps Stanford with Knuth and other people.

NORBERG: Why do you think there was such a strong emphasis on AI in IPTO this time?

OHLANDER: Well, it wasn't just then. AI had been funded from 1958 or something like that with guys like McCarthy and had grown in the 1960s. From its very earliest days, AI was a strong program at IPTO.

NORBERG: How do you define strong for the early days?

OHLANDER: I mean they got a significant portion of money. For a long time DARPA kept AI on the map. It put it on the map and kept it on the map. [Interruption] So it had been a program from its earliest day. Before we get into it though, I'd like to finish off on the structure of the program so that we can get that out of the way. I think I mentioned... Let me recapitulate. When I came in there was a program of about 14 million dollars in AI. There was a very strong vision program and a very strong natural language effort; a pretty good distributed AI program, and then pieces of other things of various sorts around the place. When the SC program came in, it got kind of organized
around four major areas: the natural language, the vision, expert systems, which are now called, I think, knowledge-based systems, and the... What is the fourth one? Let me take a look here...

[INTERUPTION]

OHLANDER: And speech. And what happened with speech was, DARPA had had a strong program in speech, you know, for five years, and then they cut it way back and Duane Adams managed that. Duane Adams left and he gave it to me, basically, to manage. And so we added that as a fourth cornerstone of the Strategic Computing Program. In the areas of expert systems, I can tell you who was doing work there. There was, of course, Stanford, who has always been doing work there...

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OHLANDER: The other name I was trying to recall before that - I knew I was missing someone - was Saul Amarel at Rutgers. [This refers to page 14.] He had been funded right along also in the area of expert systems and learning with Tom Mitchell. Well, expert systems, Tom Mitchell was doing research in learning. In the SC program, we also added Teknowledge Corporation, Intelllicorp, BBN, General Electric, University of Massachusetts at Amherst, and Ohio State University. Ohio State was Chandrasekeran, and they were doing work on expert systems per se. General Electric, University of Massachusetts, were doing work on reasoning with uncertainty, which we viewed as being important, you know, for future efforts. And in the area of speech: CMU, who had really been doing work all along - Raj Reddy, of course. We also had MIT with Victor Zue, who had been doing work, and BBN, John McKoul, who had been doing work all along. When we got the SC program, we expanded that. Those groups were strengthened. In addition to that, we added FLAIR, which was the... That was... I'm trying to remember what the group was before that got incorporated under Schlumberger Labs. It was the company they took over. I can't remember the name of it right now. [It was Fairchild Labs.] NBS was added, National Bureau of Standards. Lincoln Labs. TI was added. A company called Dragon was also added, and SRI was added. Each of them working on different things I could tell you what those were...
NORBERG: That's all right. I can look that up.

OHLANDER: I should say one thing else about the strategic computing effort. In addition to being somewhat applied, and I don’t mean... I should clarify what I mean by "applied". What I meant by "applied research" was that they would deliver a product. Anybody who worked in SC had to deliver a product. It could very well be a research product, but it could not just be papers. It had to be a system. And it could be delivered to other people in the community; it could be delivered to industry; it could be delivered to whomever. In fact, it could be more or less closer to actual production; that was not a criteria. What was the absolute criterion was that they had to develop some system, and you couldn’t just deliver papers.

NORBERG: So that means that one could do AI research under contract for DARPA for basic research and papers, and one could also do it under the strategic computing aspect of the IPTO program as well.

OHLANDER: Yes. That's absolutely right. Oh, there's one other thing I should mention that I haven't mentioned, which is Explanable Expert Systems. [This was one of the efforts conducted under the original program.] This was at ISI, under Bill Swartout (?), and it was funded under the basic effort and it continues to be funded under the basic effort, although I don't know they make that distinction anymore. But that was under the basic program. I'm sort of cleaning up here, because other things are coming to me. And also Bob Balzer and Cordell Green were funded under the basic program, in programs like automatic programming, program synthesis, knowledge-based programming, whatever you want to call it. They were both funded under the basic programs and continued to be funded by DARPA. And in addition, Bob had some SC money. That was another area under the area of research, automatic programming under the basic effort that had slipped my mind. So anyway, they were applied in the sense that they had to deliver a system. This was to try to get them focused more on a little bit nearer term results, but not to cut out research; there were still research programs. This is applied research I am talking about. One other thing I should mention about the SC program is that there was in each one of the major areas something called a "next generation system". This was to get somebody working on something in the large scale, so he wasn't working on a piece of
speech feature; he was trying to deliver a speech system. He wasn't working on pieces of vision; he was trying to
deliver a vision system. And it turns out that in vision Carnegie-Mellon was chosen to develop the next generation
system. But this focused on a whole system to do vision in some context. In this particular case, it was an
autonomous vehicle. In speech, Carnegie-Mellon was also given responsibility for the next generation speech
system. They were focused on delivering a connected speech, independent speech system of two to three
thousand-word vocabulary. I might mention for all these things, it might look like, since I was from CMU, there might
have been favoritism, but actually they were all competitively selected, fully competitive, all the SC work. [This gets
back to next-generation systems.] In the area of natural language generation, there was a combination effort with ISI
doing the generation part of it and BBN doing the understanding part of it. They were supposed to deliver a system
that would do both speech and understanding. That was going to be the heavy focus on leveraging research for that
area. And then in the area of expert systems was Teknowledge. They were supposed to develop and deliver the next
generation system in expert systems. Now, as it turned out, those things have worked out more or less well. CMU
actually does have a system that I think one could say constitutes a next generation system. BBN, also in the area of
speech, had delivered something along those lines. In the area of vision, again, CMU has delivered a system that I
think I would call a "next generation vision system" that really stretches the state of the art. In the area of natural
language it turned out that it was hard for BBN and ISI to work together because they just have different thrusts.
After a couple years of trying to do that DARPA said, "Hey, each of you go off and try to do the next generation
system." And I could say that in those areas, at least in the case of ISI, the next generation system is coming
probably within the year. There will be both understanding and generation. And, in fact, we'll probably also do
machine translation, although that's a couple more years out. In the area of expert systems, Teknowledge, in fact, has
developed an architecture that constitutes a next-generation expert system, and that kind of focuses on writing an
overall system in which you can imbed things. So it really gives you a centralized system with various hooks to
incorporate other systems. For example, we have a good knowledge representation system here, and Teknowledge is
built such that it can incorporate that without too much effort. They can also incorporate Intellicorp's key system,
which is actually a rival product. The other major thing I think that's happened in the knowledge-based systems area,
and really turns out to be where expert systems are at these days, I think, is in the area of knowledge representation.
It really is turning out that the biggest need in expert systems is for much more powerful and portable standardized
knowledge representation systems. And I think that's where that effort is moving now. Okay, in addition, going back to the Strategic Computing Program, because that really came along about mid-term when I was there and really beefed up the AI effort, you know, extended it in a major way, I had drafted up some plans for some other work in the area of planning data bases and those have... and there's a third, which I can't recall right off hand. And those, basically, were fleshed out by Saul Amarel, and Bob Simpson and Allen Sears after I left. I mean, I scoped out a general plan and put a little bit of verbiage on it. And I did actually start... Oh, learning is the other one. Learning was the other effort. And I actually did start some AI and database work with Alan Sears before I left and he expanded that substantially. Planning work, I didn't really do much other than lay out a plan and Bob Simpson expanded and put in place a real program in that area. And in the area of learning I had gotten a few things started with Bob, like Pat Langley and a few others, to get some learning building on the stuff we already had. And Bob Simpson expanded that much more substantially after I left. That still seems to constitute most of the program at ISI as I understand it. I think right now you'll need to talk to the people involved, but now they're starting to look into some other things, like Steve Cross has some ideas about pushing planning in certain directions and perhaps getting an automated software documentation effort going, and a few other things. Charles Wayne is, I think, predominantly interested in speech, but he also manages the natural languages now. Natural language is much weaker than it was when I left. After I left they built some additional work, but it has been substantially reduced by Jack Schwartz. There are some areas he didn't believe in and he cut back substantially.

NORBERG: Has somebody else picked up the funding for those, like National Science Foundation, or not?

OHLANDER: No. Well, not per se, but certainly we have gone out and looked for others. For example, we got hit in a budget cut. When I say "we," I mean ISI, and we went out and got additional funding from RADC, so some of it has been picked up, of course, by people going to other places like NSF. I don't know how it's fallen out and it would be pretty hard to find out, but I would guess that, you know, some of that funding that was dropped by DARPA got picked up by NSF, ONR and other places. But certainly some of it did not also. So anyway, I think that kind of outlines the programs, kind of from the programmatic standpoint.
NORBERG: I would like to ask two questions about that.

OHLANDER: Before I drop that, let me say that when I left I had about 55 million dollars I was managing. That included not only the AI work but also the other computer science work, which I was handing over to Steve Squires because he was the obvious person to take that program. He took it and expanded it substantially so now it's a separate program. And some of the programming synthesis or automatic programming work got carved out of the AI and went with him. So for that 55 million, it is a little hard to say how much that went with the other computer software work, how much of it stayed in AI work. But AI work was in the 40s, like 44, something like that. You could probably find out from Simpson what it was.

NORBERG: Yes, so it's about a three-fold increase over the time you were there.

OHLANDER: Yes, it was about a three-fold increase. And actually I could have done more. I had more scope under the SC program to spend the money, but I didn't want to do that. I wanted Allen Sears and Bob Simpson to have money for their work, you know, and not just to have me put everything in place and then inherit it. I wanted them... I left them something on the order of 10 or 12 million bucks, I believe.

[INTERRUPTION]

OHLANDER: As I recall, it was 10-15 million. Actually, I think it was 15 million.

NORBERG: When you came in?

OHLANDER: No, no, when I left. When I left I left 15 million of flex there for additional scope that was on the books to fund.

NORBERG: Okay, let me get this straight now. You said when you came in it was about 14 million in 1981 and it rose
to 55 million.

OHLANDER: Yes, but the 55 million included computer software, so let's say for AI it was probably about 45 million, okay?

NORBERG: Fair enough.

OHLANDER: But in addition, there was money on the books that hadn't been spent, on the order of 15 million. And I left that for Sears and Simpson to, you know, do whatever they wanted to do with it. And I think they grew some programs in the planning, the learning, and the database stuff after I left. And then part of it got swept, too, because Tegnalia came in and he wanted money for some of his things.

NORBERG: Who came in?

OHLANDER: Tony Tegnalia. He was a deputy director and he wanted some money. So any money that hadn't been spent, he grabbed it. So I am not quite sure how it all fell out; I just wasn't there at that time. By the way, if you want, you know, more precise figures, I can probably dig that up. I will verify... Let's put it that way. What I've told you I will verify.

NORBERG: Yes. I also have some program plans from that period for IPTO that Louise sent me, so I can check them against that as well.

OHLANDER: So anyway, that covers the waterfront pretty much, I think, from a programmatic standpoint and an administrative standpoint. It doesn't talk so much about the technical thrust of those things.

NORBERG: Yes, okay, I want to come back to that if I can. But I have two other questions that I want to ask you that were generated by what you said. When you were talking about the Strategic Computing Initiative, did this focus on
the necessity to produce systems as part of the SC program constitute a change in emphasis in IPTO?

OHLANDER: I'm not sure if I would say it constituted an emphasis. It certainly... I won't say it constituted an emphasis because that implies that, "Hey, before you were doing basic. Now we're going to shift to..."

NORBERG: I'm thinking of just the AI program, though.

OHLANDER: No, I know. I'm thinking about AI. I won't say it constituted a shift in emphasis. It constituted the opportunity to put emphasis on that effort and that sort of thing in addition to the basic research with the infusion of money. I mean, if that infusion of money had not come, it probably would have continued to be quite basic. I mean, when you say "basic" there are people all the way to the theoretical side to the more applied research side. So there were things going on in the basic program that were akin to that going on in the SC program; they weren't that much different. On the other hand there was a real difference in money. The basic program at that time was 6.1 and the SC program was 6.2. Now what happened was - and the thing that happened was a shame - was that Tony Baptista... Tony Baptista on the House... [It should be noted that Tony Baptista was a HASC staffer, not a Congressman.] the HASC looked at this effort and he said, "Hey, these two things are redundant," and he cut the basic programs to zero. So we went in and reclaimed that. In addition, we talked to people in the community and found out what was happening, and they got to their senators, because it was a place where he... Tony wasn't interested in talking to us. He wouldn't talk to even the head of DARPA at the time. And so what we did was we worked on the Senate side. They were very cooperative and they understood the situation, and so when it went to resolution, you know, the worst that could have happened was.... The Senate restored it all or kept it in. The worst that could have happened was they could have split it down the middle, but Tony Baptista did become cooperative and agreed to put the money back in. But the one stipulation he had was it all came under one line. So at that time all the AI money was turned into 6.2. And I consider that a great shame. You know, I was happy we were able to preserve the total amount of money, but I felt bad at the time that we weren't able to keep that 6.1. And as things turned out my concern has been borne out. Things could have been a lot better if that money had been kept as 6.1 money. When Cooper was there, I talked about the situation. He said, "You will have no restrictions on spending the money on the way that
you want and we'll recognize that as 6.1 money.” But after he left, and I left, and other people came in, and Kahn left and, you know, now it's all regarded as 6.2. I think somewhere along the line DARPA's emphasis has shifted more to the applied side and near term results, but that was not the case when I left.

NORBERG: Do you have any idea how the increase in money came to IPTO for things like strategic computing? It was a substantial increase.

OHLANDER: Well, Kahn is certainly the best person to talk to about this, because I didn't understand all the front office machinations. Essentially what happened was Congress agreed to support a new effort called the Strategic Computing effort. And that constituted a sizable difference in funds used for information processing, information technology, information sciences. And when I say "difference," I mean this wasn't all new money. I don't know how much new money came in for that. Part of it was carved out of existing programs at DARPA. Cooper felt very strongly that information sciences was an area where he could get very strong leverage, an area where DARPA made a huge difference over the years, an area that they had been extremely successful in. He just thought more of the money should go to that. Now what happened was some of the money was reprogrammed from other programs at DARPA. And some of it, I think, was new, but I really don't know; I wasn't privy to that. Kahn is the person that could tell you about that; maybe even Cooper. But I give Bob Cooper a great deal of credit for taking that stand, and I think it has made a big difference. And if you look at DARPA and its history, I think, you know, most of its big results have come out of information science, IPTO, you know. If you look at the things that have pervaded, that have made big differences in the world, not just in the military but as well in industry and academia, I think IPTO and the basic kind of research they do has had more impact. And the same is true of the Defense Sciences Office. If you look at, you know, some of the things they have done on materials, they have had a big impact. Personally, I think that DARPA should do more of that - the kinds of focused programs on major systems, I think, gets in competition with the services. And it's my belief that if DARPA was only doing the Stealth kinds of things and those other kinds of projects in the technology offices, it would have disappeared by now, because they're just in competition with the services. I think it's things like Defense Sciences Office, IPTO, the Defense Manufacturing Office, that really make DARPA distinct and, you know, put them on the map.
NORBERG: Yes, let's go back to something else you said as well, Ron, if you don't mind, and that is that all of these programs were run competitively. How did that process occur during your time?

OHLANDER: All those strategic computing additional efforts were run competitively. The reason that happened was because Cooper was starting to feel the pressure of people concerned about competition. The competition bill was in the wings although I didn't even know it at the time. He asked some people to do something about it and I thought there was enough time that I could do it competitively. So they weren't even BAAs. They were all full competition.

NORBERG: What is a BAA?

OHLANDER: A Broad Agency Announcement. The way you do it now is you do it under a Broad Agency Announcement. Well actually, I should not say that they were all full competition because... Excuse me, they were all full competitions because even Broad Agency Announcements are full competition. At that time there was no concept of the broad agency announcement, at least that I knew of. And it actually turned out that some of the efforts, for example, the one I had with NAVELEX at that time were full competition - a big announcement, all the baggage and everything else that went along with it, and they took a long time. Things that I did with ONR and RADC tended to be more towards what a BAA turned out to be. It was an announcement in the Commerce and Business Daily under Sources Sought. But we didn't go out with a full RFP. It was a Broad Agency Announcement concept. But everything was announced and all sources saw it. Plenty of time was given for everybody to compete. And so it was fully competitive from that standpoint. Where we did cut a few corners from the full rigmarole was that we didn't put out the full RFP. And the reason was for the very reason that you have Broad Agency Announcements today, that you can't put out an RFP on research when you're looking for good ideas in an area. All you can do is talk about the area of research you want, the kind of results that you want, and then you let people come in with their great ideas and you select the best ideas out of it. And that's the way the Broad Agency Announcements run today. And that's the way you have to run research. But anyway, there was pressure from
Congress and it was time to do it, and so we did it.

NORBERG: Turning to what I’ll call the basic program then, what sort of criteria were you using for evaluation of those proposals that were coming in from the various centers of excellence?

OHLANDER: Well, at that time everything was handled... Everything was done under selected-source, i.e., non-solicited proposals. But, you know, I maintained, and I still maintain, that it turned out to be competitive, although it was sole-source. Although it was selected-source, DARPA took the best there was. So that means everybody had the freedom to come in under a sole-source any time they wanted to. DARPA’s obligation was to always read and review and report on these things, and we did, and we selected the best. So...

NORBERG: How did you know you were selecting the best?

OHLANDER: We knew we were selecting the best for a number of reasons: because of interactions with the community, because of attrition of those who didn't turn out to be the best - they fell by the wayside, because the program managers were pretty darn knowledgeable of what they were doing, and because we also put together groups from other agencies, etc., who were in the area and, you know, came to a consensus. We cooperated with other agencies who were doing research. What the competition bill has done is formalized a number of these things now. The real harm the competition bill has done... Well, put it this way, in terms of the reviews that are done by DARPA, etc., they haven't probably done much, because, as I said, they tend to be competitive in the first place. There's a few more formal aspects, such as you can convene a board, and you review, and you report scores. Of course, before we didn't report scores. We just said, "This guy was the best," and we did it. It has formalized the process a bit more, but it is not too unbearable, I would say, in terms of DARPA. I'm imagining if I were there doing it it wouldn't be too unbearable. Where the real harm has been caused is in the agents that put these contracts into place for DARPA. Before, with the sole-source, they could do it in 30 to 60 days. Now with all the formality and stuff, and competition advocates they have just added a whole bunch of crap onto it. So now it takes six months or more to do anything, without accomplishing very much at all... really hamstrung DARPA in terms of its flexibility.
NORBERG: Was the competition bill aimed directly at DARPA, or the Department of Defense in general, or the government in general?

OHLANDER: No, it was aimed at DOD in general. And in fact, I think that the research area and the people like DARPA who did this kind of work were not considered very much at all. In fact, I know they weren't. In fact, after they put the bill in place they went back and looked at DARPA and ONR and places like that and put a number of bandaids in place like the Broad Agency Announcement and things like that. And in fact the people who put that thing in place were not really looking at the research really at all. And if they had, they would have done well to exempt them in the first place, because all they did was, as I said, really hamper agencies in terms of their flexibility. That was one of the reasons DARPA was so good in its early years, and had such great repute because they were able to be very flexible. That has been reduced somewhat; I don't know how much. When I was at DARPA I could get in there and put a contract in place in 30 days, and certainly within 60 days. I had agreements with agents that as long as it was selected-source and that they would give a start to a contractor as of the date they received the proposal from me. Then they would take, you know, 60 days to get all the formality in place. When the competition bill came in all that stuff went out the window. I mean, you supposedly can still do sole-source, but it's so onerous that, you know, you just generally don't try. And of course, that's what Congress was trying to do was make it onerous so you didn't try, but on the other hand what they have done is they have destroyed a flexible mechanism, and I don't think they have any idea of what the impacts are.

NORBERG: Can you give an example, though, during, say, toward the end of your tenure where the lack of flexibility caused you to lose an opportunity?

OHLANDER: No, I can't say that... Yes, well, I can actually. I didn't lose a dollar when I was at DARPA because I paid a lot of attention to getting my dollars spent. But I certainly know of people afterwards who lost money because of these competition bills, actually lost money. But the real place it hurt, the very serious impact is it took away so much of the program manager's time to do technical management and put it into garbage administration. That's where...
it really hurts. I don't know the actual affect on time, but you could probably talk to somebody like Bob Simpson, and I would say they'd probably spend another 30% of time or more on administration that they didn't use to. And that's where it's really hurt. Now you're losing the gifted peoples' opportunities to do close technical management and you've dumped it into the administrative side. That's really hurt DARPA, in my belief.

TAPE 2/SIDE 1

NORBERG: Can we turn to your technical management of the program? Can you tell me something about the way you ran the program, and what your interaction with the various contractors was in order to monitor the activity that was going on to keep abreast of the important developments in the field?

OHLANDER: I spent a great deal of time working with the various organizations, and, as I said, at that time because DARPA was so flexible I had the opportunity to do so. Let me just say, parenthetically, one of the things...

(interruption) As I said, I spent a great deal of time with the different organizations performing research through visits to DARPA and through visits out to the field. So one was able to keep on top of things technically. Oh, I remember what I was going to say parenthetically. What I was going to say was that it now takes more staff to manage the same amount of work that I used to manage because of that shift in administration. Even then, people don't get enough time to spend on their technical line. But, anyway, with the administration we had, DARPA people were able to spend a great deal of time with contractors, monitor the research very closely, hold conferences or workshops within the organization inviting people not only that were working on DARPA programs, but from outside to come in and talk about areas. In addition, go to lots of other relevant technical symposia of various kinds. So, one could keep on top of the broad areas fairly well, quite well. In fact, this is a technique that NSF uses also, where they invite a lot of people out from the field to come in and have a big conference and try to find out what's going on, do an assessment, see where you are, and that kind of thing. But in addition to that, we relied on papers that were published, journals, things were coming out, where they were going, and an assessment by the world at large as to the quality of the work. Through those means, one was able to essentially fund the cream.
NORBERG: How did all this filter into your daily activity though? You talked about reading journals, site visits, symposia, and getting some sort of quality assessment from the field. How were you doing all this? As I remember at NSF, I couldn't do all that. There just simply wasn't enough time.

OHLANDER: Well, you fit in what you had to. But, in fact, we tried to have workshops in the various areas of emphasis, for example, natural language, vision, distributed AI, at least once a year. At that time, we would get all the DARPA people certainly, and depending on the effort and how much widespread interest there was, we would invite others from the field. For example, the vision effort was open fairly substantially; certainly to all of DOD.

NORBERG: How many people might show up at a meeting of that kind?

OHLANDER: A hundred. That would be in vision; you wouldn't get that many at the others. So we tried to run those regularly and we did run them. That is becoming more institutionalized now at DARPA where they have regular workshops in almost all the areas, every year, at least every year. And then you set up a rotating itinerary of visiting people, and then you set up your administration, and whatever you had left you used for reading and inviting people in. I had a rotation of visits I made to various places. I certainly visited every place, I would guess, twice a year. Now, when I got to the end of my time there, when I was having to put SC into place, I had to spend all my time on that. While I did everything competitively on SC, it was a big administrative burden, so I pretty much roosted at home while that was going on and taking care of all that. In fact, one of the things I did was, when Allen Sears and Simpson came on, I turned the basic program over to them completely and I spent my time getting SC into place.

NORBERG: Had you had help before they came? Or were you running this program alone?

OHLANDER: No. I ran it alone.

NORBERG: When you visited a place like Carnegie-Mellon for a regular site visit, one of these roughly two a year,
what would go on at that point?

OHLANDER: Well, generally I would say I want to visit you and I want to review your programs. I would let them make up the schedule, and if they had something missing I would say I want to see this. Sometimes, I would say I only want to cover one particular program. Most of the time I was covering all of the AI work. In fact, one of the things that existed at DARPA, we tended to hear things about other people's programs too, to get a little broader perspective. So I would go into Carnegie-Mellon and I would get briefings not only on what I was doing but also briefing on some of the other work in speech and interfaces. Oh. Let me mention here: That's one of the things I left off the basic program as another area, intelligent interfaces.

NORBERG: What sort of work was going on in there?

OHLANDER: Well, it had to do with, in fact, this was another program that Duane was managing at first that I took on when he left. But what was really dealt with was man/machine interaction. How you improve that man/machine interaction, not just through natural language or other menus and visual means, but also through anticipating context, tracking context giving people the feedback they need at any particular time. So a lot of the stuff that has grown up, such as help programs and interactive programs that tried to help the user operating the system, grew out of things like that. The work has now taken off in the direction of user interface management systems where you try to provide a system that manages all the requirements of interfacing. Through that you can build tools for constructing interfaces. That work is very much on the wane at DARPA; it's been cut back very substantially.

NORBERG: Why do you think that's the case?

AMAREL: Well, because Craig Fields thought that the exciting things weren't happening in interface areas these days. I think Craig's conception of interfaces tends to be a little different than mine. He tends to think of interfaces, I think, as more the immediate front end of graphics, like Negreponti is doing: better graphics, better means of manipulating graphic systems. Whereas I tend to think of interface as the intelligent work that goes on behind that,
to give the right graphics at the right time and that kind of thing. Craig, I think, believes that to be more of software engineering rather than what he calls integrated interfaces. He just felt the program wasn’t doing what it should in the interface area and maybe some of it will be revamped into the software engineering area. Certainly you can legitimately call that work software engineering. But the problem is that the work has been cut back very substantially. Now it may get reborn again under software engineering. So, anyway, going back onto how we dealt with people, we basically would say we were coming and they would set up an agenda. Sometimes I would perturb it one way or the other, sometimes I wouldn’t. We would go into all day sessions, one or two day sessions. Sometimes it would be just review; other times we would schedule it for a particular program because that program had problems and it needed redirection or because it had run into major technical issues and we had to decide how we were going to go in the future, etc. That’s basically the way we ran it.

NORBERG: How let me ask you, can you tell me some of the important or significant developments that occurred in the program you were running, while you were running it? Some specific examples please, Ron. Then, of course, I’m going to ask you which ones didn’t work.

OHLANDER: Some specific examples of things that worked?

NORBERG: Yes, advances that occurred in the field as a result of this funding.

OHLANDER: Well, of course, there was LISP under John McCarthy.

NORBERG: When, though? Are you talking about the ’60s? I’m talking about when you were managing the program.

OHLANDER: Well, LISP came about as a concept during the ’60s, but it really took off when I was there.

NORBERG: I see. Could you describe that to me?
OHLANDER: Let me say, it really took off in between and we really got it focused on coming out with a standard when I was there. In fact, just before I got there, Bob Englemore had had a meeting with a number of people who were proponents of various LISP dialects and said, can't there be something that's common among all these efforts rather than everybody doing their own? Now it turned out at that time, that InterLISP was a standard. It was agreed upon by everybody; in the MACLISP area, everybody was doing their own. As a result of that, some people got together, five people got together and decided maybe we can come up with something. Let's see, it was Dick Gabriel, Dan Weinreb, Don Moon, Scott Fallman, and Guy Steele. So they just got the notion, and when I took over they wanted to, you know, actually come out with the standard, so I supported them to do this. Then when they actually came up with something, I worked with them to get it published and copyrighted. I got all the manufacturers that were starting to think LISP might be something they wanted to put on their machines together and they underwrote the CommonLISP standard. As a result of that, you see, basically, CommonLISP on all the machines today. I think that was one significant thing that happened. In the area of speech, certainly... well, I guess I don't want to take any credit for speech, because a good part of it was done under a different program then, Duane Adams managed it and I mainly put the new SC effort into place. The peaks probably happened on either side of me. Certainly in the vision area things were continuing to happen all the time. The work that we had been funding saw some of the various products going to the commercial world in robotics, certainly in parts recognition for registering, you know, drilling, etc., like recognizing engine block and those positions, etc., all came in to being about this time. There was no real breakthrough in the sense that here was this major thing achieved that was very visible like CommonLISP was. It was just a steady flow of technology from the research community to the industry.

NORBERG: Can we go back to something else you said? I'm just curious, what were the peaks in speech recognition that occurred on either side of your tenure at DARPA?

OHLANDER: Well, there was the first speech program. It really resulted in practical speech systems, speech recognition systems, you know. They weren't really speech understanding systems; they recognized certain portions of speech for retrieval and look up for commanding wheelchairs and various machines. All of that came from
the work that DARPA had done. I should say stimulated by the work that DARPA had done. Then after that, there
have been some real achievements in connected speech understanding and substantial increases of vocabulary, etc.

NORBERG: Now to hit those two peaks.... Well, we'll leave the first one. To hit the second peak, there would have
to be some sort of continuing development going on during the time you were there. Were there aspects of the
program that you stimulated and others that you sort of cut back on in order to get that second peak in phase?

OHLANDER: No, no. I think the second peak really resulted from the work we put into the SC effort. So I could say
I started it and made the choices of who would do it. The results were from the selections I made, but I did not
actually manage those efforts substantially.

NORBERG: But what I'm looking for here is to try to be able to get some measure of how much it takes in terms of
funding, in terms of personnel, in terms of time, in order to go from one peak to another. Is it longer in AI research
than it is, say, in distributed information systems or networks or whatever?

OHLANDER: Certainly not more than distributed information systems, because that's a very tough area. What do
you have in the way of...? Not much. Certainly distributed processing in general is a very tough problem and it
certainly is as tough as most of the AI problems. There have not been many breakthroughs, no breakthroughs in
that. Basically there's some efforts that people...there are some systems that have come out, but there are no big
breakthroughs in distributed processing. It's a very, very tough problem. You keep working it and things come out.
So in comparison, certainly one is not tougher than the other. It's very hard to give specifics in this arena, because
you have to really say what you're comparing. For example, operating system work is much more straightforward
than most of AI. On the other hand, it also has a great deal of inertia because of the investment people have in
existing systems. So unless you're IBM, you don't put new operating systems out. You know, what's the last big
operating system - UNIX - and that's old, old, old! But for a lot of people it's the newest thing on the horizon; they
just found out about it. DARPA's now funding a system called MACH. Carnegie-Mellon's been working on it;
DARPA's been working on it for years and years, and it'll still probably be a number of years before it goes out and
becomes the world's new operating system, if it does. But it's very hard to put out something in that area because of people's investment in software, not because technology is that tough. Although there are areas of operating systems like a secure operating system that's very, very tough. So, it's really hard to compare these things as one being more difficult than the other and the peaks getting farther apart. Now one of the reasons you've had the peaks in the speech program is because there was a strong level of funding, then it went down to a minimum, then it came up again. So that's why you tend to get that thing, probably, rather than breakthroughs coming along. If it had been steadily funded like the vision work, you probably would've had a flow of technology coming out. In the area of speech, certainly during the time I was there, some of the first real commercial systems came out for natural language understanding and DARPA certainly did most of the basic, funded a good part of the basic work there that led to some of these systems coming out. One was... What was his name? Cullingford (?), I believe it was? I really didn't fund that effort; I'm not ever sure DARPA did. But he built a system that IBM marketed. Gary Hendricks has come out with a system for PCs. He was at SRI, and it certainly is a result of work that DARPA funded. I mean, initially, he did a lot more work on it; I don't mean to say that he took the DARPA work and just went out and spent a few months on it. He did very significant, subsequent work. But certainly he was funded by DARPA for a number of years and his system, which I can't think of right now, is quite successful.

NORBERG: Who is it marketed by?

OHLANDER: He sells it himself, but I can't think of the name of it. I don't really pay much attention to those things. Expert systems, of course, had major impacts. A lot of that had to do with work that Feigenbaum did under DARPA funding and under NIH funding. The MYCIN and EMYCIN system, certainly the MYCIN system came out before I started to fund him. But the EMYCIN system came out under my funding. I wouldn't claim to have placed any major direction on that work for the first round of expert systems. I mean, that work was already well under way and had a good deal of form and structure before I got involved. The subsequent stuff was the result of funding that I put into place. Part of the work in knowledge representation has to do with that. Work at BBN and work at ISI in knowledge representation, based on KL1, certainly had significant impact during my tenure in the funding effort provided.

When these programs take shape you generally work in partnership with the organizations. You go in there and they
present plans on the way they want to do things and you tend to perturb them a little bit. It is a really cooperative type of effort, which is important in research, because you just can't specify what you want up front and say now deliver this. Whereas in building major weapon systems, you kind of work closer from the standpoint of requirements. You don't get very much mixed up in the how at all. Whereas in the research, you get quite a bit involved in how it's going to take place. You have to be flexible because if something leads to a blind alley you've got to go off in a new direction.

NORBERG: One of the things that DARPA seems to be very proud of is the fact that once a basic research problem reaches a certain stage where you can begin to apply it, that they have been influential in transferring technology to the industrial setting. Time-sharing is one of the best examples of that, where companies began to develop time-shared systems that they sold commercially. Is the same thing happening in expert systems, by any chance?

OHLANDER: Technology is flowing, but, see, this business of transferring is very non-specific. There is no specific program that says, hey we're going to transfer these things and we're putting these many dollars up, etc.

NORBERG: True.

OHLANDER: It happens in many ways, and generally they tend to be diffuse ways. Once in a while DARPA will pay for an actual transition, because they need to make a mark and justify what you're doing. For example, when we started the strategic computing program there was a number of exemplars that were put in place also, in addition to the research programs, because we figured we needed to prove to Congress that the technology had a payoff. Now, for example, the FRESH system was built and the CASES systems were built at... I think CINCPACFLT. I didn't have much to do with CASES, but I had a lot to do with helping evaluate and select the FRESH system and to try to provide technology that would feed into it later. Well, those have been very successful. So that was where an effort was made to specifically transition it, to show a success to Congress, to give some proof of a concept. You have to do that once in a while. DARPA shouldn't do it in general, because it's too costly, and they would substantially reduce their impact if they did. But they have to, every once in a while, select a few of these to do that. Most often,
transition comes because of writing a paper, publishing, and showing prototype systems. Certainly that's what happened in time-sharing. DARPA gave money to some people and said let's show this in existence. It did and the industry caught right on to it. Berkeley's UNIX, for example, DARPA paid specifically for it. They said, hey, we like UNIX but it's got to get better. We need things in it, for example, to handle mail protocol, etc. They paid Berkeley to do it. Because of that, that's probably the standard in UNIX today. Some of these things you have to do. They're going to be doing it for the MACH operating system also. In the case of AI, other than the previous cases I talked about, there hasn't been too much of that. Mostly it's been to fund the research and then depend on industry to get interested. Now you look at the success of AI and how many places have really solid AI programs. What I mean by solid is everybody got on the bandwagon a few years ago, and some of those have dropped out, but most of the major firms still have solid AI programs going on. Some of the AI companies have gotten marginal, but that doesn't surprise me because I always felt they were on the fringe and that really the AI stuff would be undertaken by your mainstream companies. You wouldn't just have separate companies for AI, but you would have companies that would incorporate AI, and that's certainly what has happened. Places like Teknowledge and Intellicorp still exist, and I think they're solid, but they basically have gone into.... Well, Teknowledge has merged with some robotics firm, and there seems to be a really nice fit there. Intellicorp is in a business where they build custom systems for people, but this idea of providing a framework like a LOTUS and selling zillions of them and making billions of dollars is just not coming true. There are people who want to have AI systems, and if you can build an AI system for them, you can continue to be successful, but you don't have that strong leverage that you get a few software companies that made it big, like LOTUS with their product, DB3, and Microsoft with DOS. So, there has been a lot of, I think, transfer of AI in general, and this is the right kind of transfer that DARPA should do. Not that there's a specific system that transfers, but there's a whole field that transfers. That's where DARPA really makes its big impact. So if you look at the whole broad field of AI and at one time realize that DARPA was funding probably 80% of it or something like that, in the country, there's a very strong claim to success there.

NORBERG: Success, though, based on what, Ron?

OHLANDER: Success based on AI going into various systems in the military and commercial firms. You may want
to talk to Bob Simpson. He actually started collecting data on this when he had to defend the program and he collected enough that Craig Fields no longer questions whether AI is valuable. He can give you lots of instances, but they're all over the place, in commercial and military systems. So if you look at it from the standpoint that there's a good chance that most of those wouldn't exist now, if it wasn't for DARPA funding, it's been a very successful transition of technology. Now, if you wanted to point to specific products, well then you can say that DARPA funded MYCIN and EMYCIN that went to Teknowledge and became S1. Other people have built their own based on that technology, I mean they followed the blueprint and built their own because they didn't want to be dependent on any licenses but, you know, it's really not important. The point is that the models for all expert systems that exist all derive from Feigenbaum's work. Other people would argue that there are other kinds of expert systems such as Dendral, Moses' work at MIT, the symbolic MACSYMA, and well, if you want to call those expert systems, fine, but the standard expert system all comes from Feigenbaum's work funded by DARPA and NIH. That's the model; the MYCIN and the EMYCIN are the model, and that's a very substantial transfer of technology. Most of the natural language systems that exist are based on the kinds of models that DARPA funded.

NORBERG: Such as?

OHLANDER: Well, work at SRI for example, (in natural language) such as LADDER, and then followed by a subsequent system that I can't remember right now. I'll have to dig that up. [The subsequent system was TEAM.] But anyway, that system served as a blueprint for most people who took up natural language after that in command types of interfaces, that is, man/machine rapid interchange. Work in text understanding has really not hit the commercial market much, but I predict that when it does it will be based on technology funded by DARPA. I don't mean some specific system that transitioned, but I mean the general work.

TAPE 2/SIDE 2

NORBERG: That's what you meant by the speech, when it had its big success?
OHLANDER: Yes. DARPA work came; people improved commercial systems very clearly based on the DARPA work from the standpoint of the technology approaches that were used and the technological results. Now to say one specific system transferred to another, there were a few but not many because one of the things is that generally people go out and build their own so they'll have a proprietary system. I think that's how it will happen in knowledge representation today. I predict there's going to be some standardization of knowledge representation and it'll grow out of the work that DARPA's funded, work that I funded and beefed up, work that other people continued to fund, and most importantly the emphasis that DARPA will put on it in saying, hey world we need a standard, such as they're doing in MACH right now. That's another thing that DARPA can do that very few other places can do. In fact, I think only DARPA can do it. It's hard for even NSF to do it.

NORBERG: Can do what? Can you be explicit?

OHLANDER: That is, to put into place major new systems and languages such as an ADA, such as a MACH, such as a new knowledge representation system, such as a CommonLISP, because people will listen to them and they have impact. Otherwise you just have this broad base of voices kind of crying in the wilderness, because some of these things are very hard to do. It's not just technological issues; it also involves standards and other kinds of things.

NORBERG: How is MACH spelled?

OHLANDER: MACH. This is not AI, by the way. MACH is not an AI system; it's an operating system. I use it for purposes of illustration because it gives an example of something that's hard to do from a political standpoint and something that DARPA's putting their prestige behind and something that I think will be successful.

NORBERG: I sense in your description of the program and its activities, since it takes such a long time to achieve some results in various areas of AI, that you would probably say - and I'm going to try to put words in your mouth here to see if you'll challenge me on the question - there are no failures, programs that lack success, programs that seem to be too premature to continue to fund them. Is that correct?
OHLANDER: Well, I guess I believe in long-term sustained funding in areas. You may change them, you may have to switch them, but you cannot fund areas for a few years, four or five years, drop it and then come back. Even in the case of speech where you have the speech program sacrificed to some degree, there was still some level of effort that kept going. If you're going to produce major results, you've got to put in sustained funding. Where would you be in physics today if people just went up and down all the time. And that's where DARPA can make a difference. They can pick something like AI and go in for the long haul. It may go up or it may go down, depending on where else they want to put emphasis, but I think that's very important. In terms of any of the programs in DARPA, in AI let's say, sub-programs or sub-projects, projects in AI, I would say that none has been a failure from the standpoint of progress. They've all contributed; they've all made some difference. Some have been more productive than others. There has been some shift in emphasis perhaps, because some of them seem to be more productive than others. In that sense, there are no failures of the projects. Certainly people have dropped by the wayside because they didn't perform well enough, so that's been the kind of failure that has occurred. For example, people change in a place, and they don't get good enough replacements, so that organization is dropped. They're not usually dropped, by the way at least in my experience, I don't know of anybody who was dropped just because people changed. It was dropped because people changed and they couldn't hack it later and they were given time to show that. Areas that have turned out to be quite successful may get cut back a bit, throttled back a bit, because it's more important to put money in some other area that also needs it. It's always the case that there's not enough money to do all that you want to do. So I guess from that standpoint I'm agreeing with you.

NORBERG: I'm trying to find specific examples where one would decide that...

Well, let me give you an example that Larry Roberts gave me. When he came in as IPTO director in the late '60s, early '70s, he decided that the improvements in operating systems that were coming down the line at that time were roughly 10%. He threw out the number of 10% improvement, and he didn't see that IPTO should be involved in 10% improvements of anything. They ought to be greater than that or else don't fund it. So he just cut out the operating system activity in the program and put his money somewhere else.
OHLANDER: Well, it happens from time to time. As I said, Craig thought that the improvements in interfaces weren't significant enough, so that program was cut. It wasn't that everything dropped completely but it was being cut over time so that people could get out gracefully and have a little time, but the contracts aren't being renewed in that area. The aspects of it that I feel are important, Craig is willing to consider under software engineering, so if we can fight through the software engineering program and propose some of those things and be successful, then he is not going to disapprove. There's an example of someone who's made a decision that we're not getting the leverage for the dollars we're putting in and so we should put it in some other areas that have more payoff. So certainly that happens from time to time, but that's not because the program is a failure. That's because there are better ways to spend money or at least someone perceives there are better ways to spend money. Anybody who is in there has the right to make that judgment, I feel, because these are mainly individual judgments.

NORBERG: When you were in the program office....

OHLANDER: I was growing all the time, so there was nothing that I killed that way. I had a very happy tenure there, because I had a program that was healthy to begin with; I had an office director, Bob Kahn, who thought AI was important and anything I really felt needed to get done that was additional, he somehow found a way to do it, and then the SC came along so there was substantial growth. I was never in the position that I had to make really hard decisions because of not having enough programmatic funds. I certainly terminated some efforts, no question about that.

NORBERG: Did you ever make decisions about aspects of AI that you didn't want to include in your program, for whatever reason?

OHLANDER: I didn't make any decisions about aspects of AI I did not want to include in the program. I certainly had some people I didn't want included in my program. I am certain, I can't recall offhand, but I am certain there were some efforts that were proposed to me that involved a new aspect of AI that I just didn't want to do because I either thought the people weren't strong enough, or because I thought there were better ways to spend the money. I'm sure
there were, but I can't think of an example right now. I certainly made, especially in the beginning of the year, decisions about who would and who wouldn't get funded. Some of those had to do with the nature of the work and the quality of the work.

[INTERRUPTION]

NORBERG: Since we seem to be steaming right along without interruption for your other meeting, maybe we can just turn to the third topic. I would like to hear more specifically from you what you think we should do in this project with respect to an AI study. Now what I'm interested in, in doing any of these case studies - let me mention about AI specifically rather than talk about the others - is that there are two aspects to a case study that I think are important. First of all, one has to describe the growth of this sub-field itself; one has to talk about AI in terms of its development from 1962 to, say, 1982, or whatever we choose as the endpoints of the study. We want to show the growth of the field and how that developed and who was responsible, what sort of funding came to this, and what sort of principles came out of it. The second aspect of it, and where the case study then feeds into a general history, is the impact of AI both on computer science as the much larger field and AI elsewhere, whether it's in industry, whether it's in the educational field, and so on. So, if you could help me to choose a few topics within AI that would be relatively easy, I don't want to say that this is a simple task to do, but relatively easy to describe the influence on computer science and speculate, at least, about subsequent influence on computing generally outside of computer science. That's the kind of case study I would like to do.

OHLANDER: Let me start off by saying, if you want to look at growth, there's certainly a number of parameters of growth and maybe some of which you can get from AAAI - American Association for Artificial Intelligence. Certainly the number of people in the field can be tracked over time, grossly, from just a few practitioners in the early '60s to large numbers of people now. Certainly that's an indication of success. That also can be correlated probably with some of DARPA's funding, because DARPA couldn't possibly fund all of these people, so it's not just a question of something that DARPA is nurturing. For example, if one looks at parallel computing now and some of these companies, they may be only successful because DARPA is funding the area and keeping some people going
and producing research. That was true of AI at one time, but certainly it's gone well beyond that now. So it has life of its own, whereas before it only had life within DARPA, essentially. That's one major success. One can probably somehow grossly measure the number of dollars that are going into it. One can probably grossly measure the number of systems that are coming out incorporating AI. I would say that anything you see there is probably only the tip of the iceberg, because these things are tending to become more integrated these days. There are certain kinds of successes you can track; some have had commercial impact, some have had .... Oh, another thing I should say you can track also is the scope of AI. If you look at the early scope of AI and to what it covered and to what it covers now, it's vastly different.

NORBERG: That's what I said at the Advisory Committee Meeting.

OHLANDER: When AI started, we had a few things; vision and some natural language generation and a few things like that. Now there's so many more things - manufacturing - much, much, much more to impact. Then one could look at certain kinds of subfields to see what is the impact. For example, one could look at expert systems and see a very heavy commercial impact without stimulating so much continuing research though. The research in, I think, expert systems tends to go into things like explanations, knowledge representation, and things like that that are important to it but not so much building the next great expert system. Industry is carrying that along pretty well. In terms of what's impacting computer science in general, certainly that kind of model is a tool. It is a system where you have a shell and you put things into it. I think that has a substantial impact. It's had that impact, whether it will continue to have it remains to be seen.

NORBERG: How was that used in another area of computer science?

OHLANDER: Well, I think that programming environments have been influenced by that, although one can argue that the AI programming environment has had an impact of its own. The kinds of systems for interleaving now are finding their way into other kinds of general systems use. AI has had an impact on database technology. People are looking at AI trying to find techniques for better updating and other kinds of technical problems. AI has had an
impact on languages, certainly with CommonLISP, PROLOG, and SETL. Well, some of the others are so problematic one doesn't know yet. I think AI will have an impact with knowledge representation. Certainly it's already had some influence with knowledge representation and semantics. They're using it in database systems now and in other kinds of areas where they find it useful to have semantics around to help them through problems. I don't know for sure, but I would suspect they were used in compilers and other kinds of things where having semantics around is good for you. AI certainly had a broad impact on object-orient coding and object databases. They use them extensively. LISP, for example, used Flavors, an object-oriented system coupled with the LISP environment. That kind of thing is emerging now. I think you'll get some debate on some of these things, perhaps from people like Scherlis and Squires, but I think there's no question there's been some influence. Another area that's proper to consider is something like learning, for example. When I started in AI, there was like a handful of people doing it; now it's all over the place. On the other hand, one probably has to perhaps wait and see what impacts are with some of the systems that are coming out. Certainly it's stimulated a lot of endeavor with a lot people working on the area. I'm not sure that one can point physically to some system that's been fielded commercially and is making a difference. So it's a little hard to measure that one all the way, but one can certainly see a great burgeoning. SOAR, for example, is a system in general problem solving that DARPA funded under Newell where there is a big, strong learning component that is having a major research impact. I mean, they've got sixty people in research really working on this thing. DARPA is funding a little bit of work here; a little bit of work at CMU; and maybe a little bit of work at Michigan - I'm not even sure of that. But people found it so interesting you have got 60 people working out there, the vast majority of which is not funded by DARPA. So this is an example of stimulus, intellectual stimulus, and fascination with an idea and a concept. From the commercial side, I mentioned expert systems. There's some commercial exploitation of natural language, some commercial exploitation of vision.

NORBERG: Are they very effective yet?

OHLANDER: To be honest with you, I'm not sure. The natural language systems are out and doing well. People like them, but it hasn't taken over the world. In the area of vision, some of those technologies being used on some defense systems I'm just not up to speed on them.
NORBERG: Can you give me one example, though, of the military system using these?

OHLANDER: I'm not sure I can, but you ought to ask Bob Simpson. I'm kind of decoupled from that now. Bob Simpson's got a big collection of systems-user technology. As a way of suggesting going about doing a study, one thing I would do is ask AAAI to help me out first. What they can give you is the kind of major progress in the AI field, etc. I would go to some people that have been connected with the field over a number of years, such as Raj Reddy, people you've probably already interviewed or thinking about interviewing, Allen Newell, for example. Allen Newell would be a good one, Ed Feigenbaum.

NORBERG: All of these people have agreed, so there's no problem with that.

OHLANDER: I think that would give you a great perspective. All these people are very intimate with the history of it. I think they could really help. What I would suggest is getting some framework around which you want to measure. I suggested the dollars, the people, the scope of things. In terms of successful systems, then the other aspect of how many systems have become successful. Feigenbaum's book, this new book on the expert systems, will help in that regard. There's places like TI putting these things out all the time; you just stop tracking it. At one time I was tracking every one of them, because I was looking for success and now I don't even bother because there's so many. There's DEC's experiences with R-1 and XCON and XCEL that have been successful, and you ought to talk to John McDermott at DEC. You ought to put him on your list, because there's a company that has a very big AI group and who are continuing to put out things. Some of them, I think, are beginning to go commercial. They're using them internally, and at the same time they're starting to say, hey, maybe we can sell these things outside. So, John McDermott is a guy you ought to talk to. Herb Schorr is a guy you ought to talk to because he did a study on this very recently on expert systems and their success and he's got a lot of data.

NORBERG: I see.
OHLANDER: He can tell you about IBM and the successes they found.

NORBERG: Did he publish that, or is it going to be published?

OHLANDER: Yes. It is going to be published, I believe. I think he's going to publish a book. Where it stands I don't know. I don't know if it would be possible to talk to him. Probably within this timeframe it's probably too short a notice, but there might be some opportunity.

NORBERG: Where is he now?

OHLANDER: Right there.

NORBERG: Right here? Next door. Yes.

OHLANDER: I think if you look at that and start that way, especially talking to Feigenbaum and Schorr who have looked at the commercial aspects of these things, you can uncover a thread that will lead you to others. So commercial success would be a good one. Be sure to talk to Simpson about uses in the military. NASA's got a big program. I just went to the NASA conference down there, and they're talking about a number of endeavors. They've got a big program to incorporate it in the space platform.

NORBERG: I'm surprised. I hadn't heard of that before.

OHLANDER: Yes. You might talk to Swietek. Greg Swietek, I think it is; he's manager of advanced automation and robotics. I'll tell you, when I was at that conference, the thing that impressed me was not so much the work that goes on, but they're talking about software engineering for AI and how you support it over its lifetime and that kind of thing, and you don't get into that until you're institutionalizing. You get into that when you're starting to deliver systems and you're having problems managing them and dealing with them. So, they're talking about engineering
standards. Oh, in fact I've got a guy's call I'm supposed to call him back on.

NORBERG: Can I just ask you one more question, then we can stop?

OHLANDER: Yes.

NORBERG: How much interaction did you have with other agencies during your tenure at IPTO, such as NASA, that might have been funding other programs or developing their own systems?

OHLANDER: Well, when I was at DARPA, there weren't that many agencies funding AI. So in addition to the expansion of AI in companies, expansion in the government, NSF was doing just a little, AFOSR was doing some work in vision they had a vision program, ONR had always been doing some work, but it tended to be a much, much smaller program than DARPA's. I interchanged with anybody who was doing it. But there weren't very many professional counterparts out there. Anybody that tended to do it happened to pick it up as a kind of a sideline. They either got interested or they said, hey somebody needs to do this here, here it is. Marv Denicoff already started to fund... he was a believer, he always funded something. He was an agent for DARPA programs; he added some things too, and funded some of his own things. In fact, that natural language system, I think it was done by Cullingford at Connecticut, I think it was, that later got developed into a commercial project and used by IBM, was partially funded by him. NIH was a believer in it, through Feigenbaum. I didn't talk much to them, because there wasn't a strong AI person and they did everything from peer review and there wasn't much to discuss to tell you the truth. I talked to Y. T. Chien at NSF, dealt with him; we got a few joint programs going. I kept in touch with the AI Lab that was started at the Navy at NRL. I kept in touch with the AFOSR people until Price left. I was pretty much in touch with anybody who really had any kind of strong program.

NORBERG: This helps to justify the comment that you made earlier that AI support from DARPA covered about 80% of the field, roughly.
OHLANDER: I just used that as an estimate. But, let me tell you, DARPA was funding far more than all the rest of them put together.

NORBERG: Others have made the same claim, so you're not the first one to do so.

OHLANDER: I mean you could talk to people like Feigenbaum and they'll tell you because they were out looking for funding, right? They can tell you where the funding came from. I would say it was on the order of 80%, certainly more than all the rest of them put together by a large margin. And when I was at DARPA, I started to see that percentage erode, which was a good thing. It certainly is not the case that DARPA funds more than all the rest of the government put together today. I think they still fund more than any other agency. I'm sure they do, but it's not so predominant.

NORBERG: I'm of the opinion it is somewhere close to 50% still, though.

OHLANDER: Yes. I'm not sure of the right number. It depends on what you're talking about, if you're talking about research... See, DARPA is still a very, very big player in the research and that's why it's important for DARPA to continue funding AI, because of that research. Most of the other people funding, I mean other than NSF... If you look at the research places fundamentally, you've got DARPA, you've got NSF, you've got AFOSR, and you've got ONR, and ARO ....

NORBERG: How about NIH?

OHLANDER: Okay and NIH. ARO and AFOSR tend to be small. ONR, they're very small, too. NSF, they've got a lot of money in the aggregate, but I don't know how much is going to AI funding, probably not that much.

NORBERG: It's not that much, that's right.
OHLANDER: And NASA probably has more going in than any of the other places. They've got a very good group now up at NASA Ames under Peter Friedland. They're certainly not near the level that DARPA is, but they're certainly bigger than any of the others, I think. But if you still put it all together, if DARPA were to drop out, it would be a very severe handicap. It would be a very severe impact on the program, because while DARPA is not the major funder of all AI anymore, it's still by far the major funder of research. If you want these commercial exploitations to still come and be used by the DOD, then progress has to continue so that the technology flows. A lot of the funding, most of the funding today, is in the application side. If you look at the money DEC and others are putting in, it's all going to applications, some technology too.

NORBERG: Very good. This has been very helpful. Thank you very much.

OHLANDER: You're welcome.

[INTERRUPTION]

NORBERG: There's still some room on the tape.

OHLANDER: What I was talking about was DARPA's ability to get focused on critical mass, to do things on a larger scale even though they're funding individual efforts, than anybody else can do. NSF can't do it, they're too diffuse and they have the peer review type of evaluation. They get much less direction on their efforts. There is no other organization that can or does do it the same way. Maybe if they get a civilian DARPA, as they're talking about, that might happen, but I think that's an area where DARPA makes a contribution that far exceeds their work to just the DOD. They can focus on, for example, network protocols and put a system into place, and now the whole world has DARPA to thank for network standards and not only that, but showing the technology can work and getting industry to put in large amounts of money to put in their own networks. I think it'll happen in MACH again. As I said, it's very hard to put an operating system into effect these days because unless you're IBM you haven't got the resources and the horsepower to do it and to make people conform to it. But, if IBM does it, it's not good, because
they're doing it just for their own products....

END OF INTERVIEW