

An Interview with
DABBALA RAJAGOPAL REDDY

OH 231

Conducted by Arthur L. Norberg

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Dabbala Rajagopal Reddy Interview
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Abstract

Reddy discusses his work in artificial intelligence (AI), especially speech recognition, from his graduate work at Stanford University through his research as a principle investigator on Defense Advanced Research Projects Agency (DARPA) grants at Carnegie-Mellon University. Other topics include: the interaction of researchers at the Stanford Artificial Intelligence Laboratory, DARPA funding of AI research, the expansion of the principle investigator community over time, and the various directions of AI research from the 1960s to the 1980s.

DABBALA RAJAGOPAL REDDY INTERVIEW

DATE: 12 June 1991

INTERVIEWER: Arthur L. Norberg

LOCATION: Pittsburgh, PA

NORBERG: Can I begin by asking you to comment a little on how you got interested in the problem of speech understanding in the early 1960s?

REDDY: I was a graduate student at Stanford. I came there in 1963, and frankly, other than broadly being interested in artificial intelligence, I did not specifically think about speech, per se. And that was about the time DARPA was initiating some of the earlier funding, and John McCarthy, who was the key person in AI at Stanford at that time, and still is, received the first DARPA grant, I believe, at that time, which was then partly used to buy the first PDP-1 that came to Stanford in 1963. A lot of the work on it was more on time-sharing and LISP and other things. But we were able to add an A-to-D converter to the PDP-1 and then it became obvious that with ADT converter you could begin to look at things like speech and image input signals of various kinds and see what could be done. And thus I started a class project in early 1964, which was part of the broader DARPA-funded research in speech recognition.

NORBERG: Actually a class project.

REDDY: It was a class project.

NORBERG: How many people involved in the project?

REDDY: Just me. John McCarthy put up in the class saying, "I want everybody to pick a project and do something. Here is a possible list." And I picked speech recognition.

NORBERG: Why did you pick that one? Maybe it would be useful to say what some of the others were if you can remember them.

REDDY: They were kind of broadly in symbolic processing and optimization of compilers and projects related to LISP of one kind or another. And so it was a broad spectrum, and speech was one of the ones that became possible because of the availability of the A-to-D converter.

NORBERG: So why did you choose that one?

REDDY: I chose that particular one because I was both interested in languages, having come from India at that time, and in having had to learn three or four languages, and I thought it might be interesting to understand and study what it is that we could learn from use of the computers. And also, speech is something that is ubiquitous to humankind. And so you think in studying something as fundamental as that might be an interesting endeavor for somebody. What I didn't know was that it was going to be a lifetime problem. I thought it was a class project, to do something. After spending close to 30 years on that problem, we are probably less than 10% there. There are kind of a large number of still unresolved issues. Later on, I will show you a video tape of some of the things that we are able to do now, but that's where we are.

NORBERG: All right. Let's go back to about 1966 when you have your degree and you now are embarking on a research program of your own. How did you define the problem for yourself? McCarthy would have defined the class project, but how did you define the problem for yourself?

REDDY: It was kind of a natural extension. I had just finished the thesis, and Stanford, in spite of their restrictions on hiring their own Ph.D.s, hired two of us, Bill McKimmon and me as assistant professors. And so I didn't just pick speech. It turns out I had some students working on image with computer vision. There were two kinds of vision that were going on at that time. One is hand-eye manipulation and robotic control, which Jerry Feldman and a number of other people were doing. Then I was kind of interested in more natural interpretation of images. So Mike Kelly, one of my students, started working on what it would take to recognize human faces, which is still an unsolved problem even today. It is solvable to some extent, but it is not uniquely done yet. My expectation at that point was I

would be working at the interfaces of speech, vision, and robotics - the kind of perception manipulation end of AI. There were other people working on certain kind of coordination issues and hand-eye systems, and Shakey the Robot and so on. But nobody was kind of looking at it in the perspective I was looking at. But a significant part, because of my background and experience, was on speech. You know, I had two or three students - Lee Erman and Gary Goodman and Rich Neely at Stanford, who then came to Carnegie-Mellon when I came here in 1969 to continue some of the work. Mike Kelly, who was in vision work, finished at Stanford and graduated and went on to Georgia Tech.

NORBERG: What was known about the problem of speech recognition at the time? What sort of work had been done before yours?

REDDY: Most of the work before that was kind of a straightforward attempt at matching that was done out of Bell Laboratories by Peter Denish and Ed David. There's a paper by both of them on the subject around 1961 or so. There was some work at Lincoln Labs by Ben Gold and Jim Porgee [?]. They were not doing recognition, per se. They were looking at specific phenomena, like pitch detection, or something.

NORBERG: What do you mean by pitch detection?

REDDY: If you look at sound...

NORBERG: Pitches in human sound?

REDDY: Whenever you say r or something there's a vocal code vibration, so you see a pseudoperiodic wave form. And if you can locate the periodicity that tells you the pitch of the speaker. It is also useful for further signal processing after that. So the net result of all of that was, what we thought, and if it proved to be partially successful at that time, and now, it's actually an amalgam of a number of the different techniques. It was, in fact, that you may not need to go and resort to full spectrum analysis. There may be other features that can be directly extracted from

the time domain signal that might be adequate for the purpose of recognition. And it did prove to be, up to some point, right. One of the first things I did as a class project was to build a vowel recognizer. I remember Ivan Sutherland had just taken over from "Lick" Licklider in 1964 and came for a visit to Stanford. And so he spoke to the computer and he said, "Ah," and it said, "Ah," and he said, "E," and it recognized all the different vowels. And then he kind of whistled into it and you said, "E," because even though he whistled. That was the first realization of what it would take to build robot systems, not just systems that operated in an envelope.

NORBERG: What was Sutherland's reaction?

REDDY: He was kind of happy to see the results and the interesting work that was happening. They continued funding that work at Stanford from 1964 through 1969, and afterward also a little bit even after I left. When I came to Carnegie Mellon in 1969, there was no activity here in the perceptual motor end of the spectrum. That was one of the reasons both Simon and Newell, I think, desired that I join them here.

NORBERG: Okay, we can come to that in a moment. I would like to continue with Stanford, if you don't mind. And that is, I'll come back to the question of the research that you were doing, but I would like to fit it into a context here before I ask my next question about the research. And that is, who else was at Stanford at the time in AI and what sorts of other projects were going on - 1966 through 1969?

REDDY: I think, in my view, that was the golden period of AI at Stanford. The next golden period will come probably when the heuristic programming project... the expert systems that came to be kind of a rage. But then it was one area. So from 1963 to 1969, there were six different things that have happened at Stanford which have had significant impact on AI. Let me kind of go through each of them.

NORBERG: Yes, please.

REDDY: So there is the speech project that I was working on. There was different kinds of image input and image

processing and computer vision projects that Jerry Feldman was working on. The manipulator hand-eye coordination stuff. And some of the early work in edge detection, like Dupel-operator [?] and Sobel-operator [?] came out of that. But that's just part of it. Then Ken Colby and Roger Shank and Horace and Larry... he's at Apple now; that will come to me in a minute. All of them were working on natural language and using psychiatry as an application domain - natural language understanding, including verbal language rather than grammatical. So that whole area was kind of beginning. And Ed Feigenbaum was part of that group at that time. He was associate investigator. He started the work on DENDRAL. That was in about 1965, 1967. Then Pat Hayes was there. McCarthy and Pat worked on epistemology. Then people like Barbara Liskov, who is now at MIT, did her thesis on chess, end-games in chess. So there were about six or seven activities that kind of got started at Stanford at that time where all the people from that core have kind of gone on to different places and have been able to establish the satellite centers, if you will, except here it was already established. At Yale, until Roger Shank went there, there was nothing significant... And after all those people left around 1970, 1971, the whole Stanford AI scene, from my point of view, was much less interesting and much less dramatic. And John also, I think, became not dispirited, but basically he was able to kind of collect and let people do their thing. Oh, there's one other thing I forgot to tell you. The computer music was initiated about that time, computer-generated music, music synthesis. John Chowning, who is now a professor at Stanford, used to be a student. He used to come and look at me kind of accusingly, because he was a smoocher. I was the only one that would work all night, every night. So anytime he needed to use it I would be sitting there and so he would feel... because he was not part of the DARPA group, right. It was all kind of on the side. So that's where the DARPA impact on things they didn't even fund can be profound. So the whole computer music, the music synthesis and computer music, came out of Stanford. And some work came out of Bell Labs, but I think by far the more dramatic stuff of the 1960s and early 1970s came out of Stanford. And at that time, computer-generated music was not even academically respectable, and it took them 15 more years before they could make John Chowning a professor at Stanford.

NORBERG: So when all these people left McCarthy got dispirited.

REDDY: He didn't get dispirited, I don't think, but he basically did his thing, you know, which was Advice Taker,

epistemology, semantics of programming languages. He did whatever he was doing by himself, continued to do. But I may be being unnecessarily pessimistic, but my view is that was the golden period. Afterwards, things came out, but nothing dramatic. The only next big thing to happen out of Stanford in AI was the work of Feigenbaum's expert systems, Shortliff and Buchanan and all of that team of work. That group also kind of split off and became a heuristic programming project. And that was kind of exciting. Now there's a lot of work on logic and so on. Other than polishing the representational framework underneath, I am not sure where it's going to go, or when we will learn, or what the impact will be. There are a lot of people that are working on those kinds of things, like Nils Nilsson, Genneseith [?], McCarthy. Everybody thinks logic as a formalism or a framework may be the way to deal with it. It may be true, or it may not be true, but...

NORBERG: Incidentally, are there alternate positions being taken by people in AI to provide an answer to the learning problem?

REDDY: Absolutely. There are any number of representations, logic being, from a mathematical point of view, more rigorous. But if you talk to Simon - I don't know if you have talked to him or not - if you have raise this issue he can give you a much more cogent viewpoint of why there are three or four other ways of representing information, which are equally formal in his mind. And so it's an interesting debate, but from my point of view it's going to be a long time before that becomes the preeminent representational framework.

NORBERG: Okay, back to the late 1960s. How did you...?

REDDY: Did I convince you that, in fact, it was a golden period?

NORBERG: Oh, you certainly did. How did the people in the laboratory interact? What sort of interaction did you have with Feigenbaum, with McCarthy, with Kelly, with whoever else was there?

REDDY: We had a very interesting, you know, executive director, Les Ernest. Les was there since about 1964,

because I think people realized John McCarthy is not a manager and he needed somebody who could deal with all the bureaucracy that DARPA and other people need, kind of helped him to recruit Les Ernest from Lincoln Labs. Les is still there. In fact, by the way, there is a thing here I can print out. SAIL, Stanford AI Lab computer, the PDP-6, PDP-10, is being disconnected this month... this week actually, after 25 years or something. So there is a little party going on at... so let me print this out.

[INTERRUPTION]

REDDY: They are actually having a closing-down ceremony for that computer that was actually being there and operational, and just now, I think, it became too expensive to keep it maintained. It was taking up too much space.

NORBERG: Okay, we started that by my question about how did you interact with these other people.

REDDY: So, here was a good, high caliber, quality time-sharing system for that day and age. And we were all using a PDP-6 and trying to do different kinds of research, and we found there were limitations of language and limitations of display. So McCarthy got Ed Fredkin to build this special set-up of graphic displays at that time. That's another direction that came out of the SAIL labs. Before that, nobody was seriously looking at using graphics or video terminals. But video terminals came after the fact. But first, random graphics terminals, which were, of course, originally used at Lincoln Labs, but as a routine way of interacting in a time-sharing environment, first came out of Stanford.

NORBERG: Did Fredkin do this through his company?

REDDY: Yes, we used to call them quadruple Is. Triple I was the name of the company. The video camera also was built in, so it was called the Triple-I I. And so the way of interaction was kind of one of between colleagues.

NORBERG: Well, I guess I misled you by the way I asked the question, because what I am really interested in is,

which research areas and research results contributed to your work, and what of your work contributed to the work of the others? I am looking to build a statement about what everyone was doing and how they were contributing to some core.

REDDY: In this particular case, the contributions to each other were primarily in the tools, and systems software, and editors, and LISP, and all the other things that we were all commonly using. A lot of the work I did at that time had to be done in FORTRAN, because good LISP was not available; even if it was, it was too slow, so there was a concern about how we actually did this thing. Other than that, the degree of direct impact on each others' research was mostly, I would say, by osmosis, or subconscious, rather than any deliberate... There's only thing where we actually did a project. It's a paper in the Fall Joint Computer Conference in 1968, called "A Computer with Hands, Eyes, and Ears" or something, where we tried to integrate the speech research and the vision research and the robotics work into a single system. We tried to say pick up the red block on the bottom corner or something, and the vision system would look at it and decide which one it was and just pick it up and stack it, or do something. And that's the first time we discovered the complexities of systems integration, taking all this different software written by different people and putting it into a single working unit. And what we thought would take one month took about a year. Why? Because the people were using the same variables in different places, so the blocking structure didn't exist, and all kinds of stupid, irrelevant things. But nevertheless, they became a major problem. If it was today we would have things like Remote Procedure Call and Interprocess Communication; they could compile them as separate processes, and then simply shift information so that they wouldn't have to worry about sharing the variable space. There, in order to integrate them you had to make a single compiling of all the different programs, which then blew up, and by the time you tracked down each one and fixed it, and the software engineering practices were not so hot at that time, there are still not even now in research. An interesting thing is how little research environments follow the practice of software engineering, even today.

NORBERG: This seems like a period, in fact, Feigenbaum said this to us during an interview some weeks ago, this seems to be a period in which DARPA money was provided, and maybe other money from NIH and places like that, was provided for the investigation of research problems as defined by the investigator at the time the problem arose,

like the integration problem you were just describing. And therefore, there was, in his mind, at least, no necessity to worry about justification of these projects. You had the money; if some interesting problem came along you investigated that problem until you were satisfied you had a solution or the problem wasn't very interesting anymore.

REDDY: No, that's not completely true. I think I might even have still that 1968 proposal we wrote for something, where we did outline what areas we would study. And we might have said about three pages about speech, and three pages about... I'm sure you will find these in DARPA archives also, but if you can't I think I have it somewhere. I can find it.

NORBERG: It would be nice if you could find a copy, because we don't have a complete record from DARPA at all.

REDDY: In the proposal, we did actually say what we were going to study, but Feigenbaum is correct in the following way. Along the way, if we decided that was the wrong way of doing it and we wanted to do something else, there was a substantial freedom. I believe that freedom even now exists; we just have to talk to their program managers a little bit. Secondly, the degree of specificity to which the proposals were written were much lower, saying, "Here is an interesting problem; here are some broad outlines of the direction in which we are going to study it." That was it. Then the expectation was that we would discover a number of unanticipated things, which we did. And what we thought were two-year projects or one-year projects became thirty-year projects. And so the role of clarifying the nature of the problem, and clarified it... So it is indeed true that there was a substantially greater degree of freedom, and we could have probably proposed something else and still got funding for that too. But that's true even today. I could propose something else, and maybe have not as high a degree of success, but some degree of success in finding money.

NORBERG: Well, that sort of is in a bit of disagreement with my perspective on what happened over the years. And that is that in the, let's contrast the 1960s with the 1980s, rather than try to get a continuous history here. In the 1960s the interaction between principle investigators and people in the IPTO office was relatively easy-going, let me say, in that it was a somewhat tighter community than it is now. It was also remarkably similar to the description you

gave of the people inside Stanford AI lab. Whereas in the 1980s there is... Well, I didn't finish my remark. And everybody was, for the most part, convinced that there were certain fundamental problems that needed to be worked on before one could ever get to application of these principles. So there's a considerable amount of process technology that is investigated during the 1960s. And that leads to all sorts of spin-offs, as well.

REDDY: Right.

NORBERG: Now, in the 1980s, things have changed remarkably in the Department of Defense. There's much more focus on applications, much more concern about meeting the DoD mission, and much less interest, as far as I can tell, in supporting university research for the sake of supporting university research.

REDDY: Yes and no.

NORBERG: Therefore, the proposals that you would make now would have to be considerably different than back in the 1960s.

REDDY: That's probably right, but let me kind of contrast it on a different perspective. I don't quite remember exactly the number. I believe the total funding around 1968 for information technology was around \$20 to \$25 million.

NORBERG: That's correct.

REDDY: And I believe in the start of the strategic computing program in the 1980s the funding was about \$250 million, and I believe in the 1990s the funding will be more like 1/2 a billion dollars. Now, you can take the 1968 dollars and then compute the amount in constant dollars in 1984 and I believe that would be more like 60 million or 70 million.

NORBERG: Fair.

REDDY: Three times. And if you go back and look at the total pot and see what fraction of that money is still available for what you might call basic research, because at CMU we have a certain level of basic AI funding, which has kind of continued. We kept shifting things away from it, like speech is no longer in the basic AI, because there was a speech understanding program. And the only way we could kind of keep basic AI funding for other things was to move it out. So I think there is still that kind of money being spent on those fundamental investigations. And the rest of the money is going into what you might call, "Uses and Applications and Productization of the Basic Research." That's one viewpoint you can take. Now, even if you simply limited yourself to that \$60, \$70 million and said, "Is it really... That \$70 million - is there the same laissez-faire attitude about how it is spent?" That's probably not the case, and I think you would be correct in saying there's a little bit more. The same bureaucracy that's getting applied to the rest of it has also crept into this, right? But nevertheless, at least in the old centers where the funding has been kind of continuing, there is a certain level of acceptance that that funding ought to happen. But what did happen was, now this basic funding had to come out of somebody's pocket, in the program managers. And the program managers were getting more acquisitive, and they are getting saying, "Why should I put the money into some basic thing which is kind of vague and anything could come out of it? Instead, if I could put it on something here a gadget will come out, which would make me look better."

TAPE 1/SIDE 2

REDDY: So that's what I believe happened; namely there is indeed... My view is the amount of money being spent on those kinds of endeavors is still substantial.

NORBERG: Well, I certainly don't disagree with that.

REDDY: And the proposals you would have to write may be similar or slightly different. You still outline your basic ideas, but the amount of money you would get is of the same order of magnitude. In those days it might have been 200K; today it may be a half a million. But if you want to get a full million for a project, then it has to be more application-like.

NORBERG: Fair enough. Okay, again, go back to the 1960s, because I want to make another characterization here, but I am going to try and elicit it from you first rather than you hearing my viewpoint. How would you characterize the nature of artificial intelligence research in the late 1960s? What were the principal paradigms that people were operating under to explore the various research problems in the areas that you described earlier that were going on at Stanford?

REDDY: Right. So there were kind of multiple paradigms. There was a paradigm that was happening at Carnegie-Mellon at that time, where if only you could kind of understand the nature of search and problem-solving... the nature of problem-solving and search as a mechanism for doing it, then you would understand intelligence. That was the main conceptual theme of the 1960s. Within Stanford at that time, even though there was an appreciation, the view was, "People at CMU are doing that; there's no reason for us to do the same thing." Nobody articulated it that way. And then they were kind of looking at other ways of exploring the same thing. If we wanted to understand intelligence, what would we study? Here there were kind of protocol analysis and the study of problem-solving behavior. And a lot of the work that happened at Stanford then, you can see, it was all saying, "Can we understand intelligence by studying how computers listen, and see, and understand language, and play chess or whatever. So it was more of a subarea or task-related research with no underlying theory about what is the right framework that would lead to progress in the field. In the 1970s, then, the knowledge framework became dominant, and that came out of Stanford. And then, now if you go back in the 1980s and keep looking at where the 1980s were, I think it's more of the same, until, at some point, now there's a significant realization that systems that can learn from experience, and learn from example, and learn from observation, is the structure that will teach us what is the fundamental underlying nature of intelligence. And Newell has a list of half a dozen things, like what is an intelligent behavior. And each of them are fairly interesting, including real-time behavior. Nobody within AI that I know is studying the notion of hurry-up. I can come to you and say, "Hurry up, we are running short of time. We have got to go do something." Whatever problem you are solving you will do something - either delay it, or do it partially and get it out of the way, or ignore it and forget it forever. We do various things. And the intelligent computers of today don't have that notion in their system. You know, the old issue of resources and how we proceed. On the other hand, there is this

major preoccupation with logic and logic programming and non-monotonic logic, and a whole series of other things. And then there's a set of another very interesting direction on uncertainty and dealing with uncertainty within AI. And so, you can see these themes kind of come up and go down. And the main theme of the 1960s at Stanford was, if you could study speech, vision, robotics, languages, then the sum totality of understanding those things would lead us to understanding the nature of intelligence, if you will.

NORBERG: Would you say that what was going on at MIT was similar to Stanford, then?

REDDY: Yes, to some extent. They were not doing speech. As far as I know they were not also doing natural language; maybe Marvin [Minsky] was doing some work. But there was a lot of work on image and computer vision. Yes, language work came out with SHRDLU with Terry Winograd. And there was some robotics work.

NORBERG: Is there some basic underlying principle there similar to the one that was going on here at Carnegie Mellon, in terms of trying to understand human problem solving?

REDDY: No, I don't think there was any explicit or implicit tilt toward saying, "Whatever we do here would also help in human problem solving." However, some of the models, like the blackboard model and so on, were adopted by psychology after the fact, even though they were kind of invented as a mechanism with no deep understanding of their relevance or role in psychological ? ? . Sometimes there is transfer and some knowledge back and forth. But it's kind of...

NORBERG: Back to your own research area now, what sorts of accomplishments did you have over the three years from 1966 to 1969 before you came here?

REDDY: So, the main things we did there, I think, were, we built a system for isolated word recognition, which was the largest that I knew of at that time - about 560 words or something - with a respectable performance of like about 92%. It didn't do spectral analysis or any of those things, and it still looked pretty respectable. And at the same time

we started dealing with connected speech. And we, you know, had a system in which you had a primitive grammar so that you could actually map each of the words and then see how far you are and then go from that. So, from that perspective, the beginnings of isolated word recognition and some of the connected speech work that we did there, I think, were still worthwhile. What I will do is I will give you a copy of a review paper I wrote around 1974 or 1975.

NORBERG: I think I have that here, the one on speech...

REDDY: On machine recognition of speech...

NORBERG: Yes.

REDDY: ... because it was an attempt by me to kind of summarize what were all the things that were known, and I believe even today people kind of keep referring back to it. The same problems are with us, and we have maybe three more bits of new insight that we didn't have at that time. And so after we came here the two things that we kind of contributed from CMU as part of the DARPA project: one is the role of syntax, and the other was the role of semantics in speech recognition. We did some of the low-level acoustic work also, but we essentially ignored it, saying, "Let other people do it," because we were trying to prove the point that AI, or higher level knowledge can help equally well, or it can be very powerful and important in solving the recognition problem. And I believe the original ? chess system that we did hear on the Hearsay I was one of the first uses of semantics in recognition. And when you come back, and now after 15, or 18 years, and look at what is the role of semantics, it's the same: it's an attempt to constrain the alternatives in the search space. And for chess there was a clear, obvious way of doing it. For other natural language semantics it was not clear, but now that we are able quantify and qualify some of those alternatives, now their role is exactly the same. And that was one of the first things we did. So there's a wide range of different issues that came to the fore at that time, which are still the classic problems of today. And if you look at the problems we are working on, we are working on robots, and what happens in different noisy environments. We are working on more detailed models. When we built the Harpy/Hearsay systems we used to use one spectral slice for a whole segment for a vowel, or something. Now we use many, much more detailed model of a vowel, or detailed

model of a word, or something. And the more detail we are able to keep the better the accuracy is.

NORBERG: This must take a considerable amount of computing capability to do that.

REDDY: It does. And that was one of the problems. Our thinking was limited by the computing available almost. The models we were willing to study... Today, there are 20 megabytes in a workstation, so I can actually look at various sets of complex things. The PDP-6 we were working on only had 256K words or something.

[INTERRUPTION]

NORBERG: All right. Your mention of the 1968 proposal leads me to ask you what sort of interaction you had with DARPA people while you were at Stanford. You mentioned Sutherland, but what happened after that when you were an investigator?

REDDY: The nice thing about both at Stanford and here is the amount and degree of interaction I have to have with DARPA people was mostly professional and occasional when they came to visit or something. They knew me and I knew them, but I didn't have to do any negotiations or any of the worrying about the budgets and so on. And that blissful state of attitude continued until about 1980. When I came here, Newell was here. He was talking to them and taking care of things, and I was delighted not to have to do it. I would write my little portion of the thing, if needed, but simply attend to research and not worry about all the rest of the thing. And that's how we kind of...

NORBERG: Did you ever attend principle investigators' meetings?

REDDY: Yes.

NORBERG: When do you remember the first one?

REDDY: When Lukasik was still at DARPA there was a meeting in L.A.; that was probably the first one.

NORBERG: Okay, so that would have to be somewhere after you came here then. Is that right?

REDDY: Yes, probably. I think, yes. I would say that. There was a meeting in L.A.; there was a meeting in San Diego the following year, I think.

NORBERG: Yes, that sounds like 1972, 1973 to me, if I remember correctly.

REDDY: Right, so those were the first meetings that I remember attending. And, you know, it was kind of an interesting period in some sense.

NORBERG: In what way?

REDDY: Because in the principle investigators' meetings you met all the other people in the DARPA community; it was still small - like I think 40 people, or something. So you kind of knew everybody, but as time kind of went on, you know, the last PI meeting that was in Dallas or something, there were, like, 300 people. It was no longer the communality; it was too big. And I remember sitting there and saying, "I wish we could kind of find a representative for each \$5 million of funding or \$10 million of funding, so that all the random 100K proposals, or the PI, doesn't show up." They also need a way of getting together, but that could be a different meeting of a given area or a community, which can most benefit by getting together. But if you are kind of looking at DARPA as a whole, or ISTO office as a whole, then I thought we ought to be able to find a formula where you can bring 20, 30, 50 people together, who would be able to articulate a vision and use and share ideas. And the new ISAT meetings that are held serve that role a little bit, but not quite the same.

NORBERG: What does ISAT stand for?

REDDY: Information, Science and Technology Study Group. They meet every year at Woods Hole in August. So one of the things you might want to do is see if you should come there and sit in the background and listen to what goes on. How far does your history go?

NORBERG: Well, we will probably stop with the introduction of the Strategic Computing Initiative, the definition of it, because there are too many political problems in dealing with it at the moment.

REDDY: Right, so if that is the case then you don't need to be there. So the current thing that happens is, there are about 20 members of an advisory group, 25 members, who are kind of from various disciplines, who serve the role of contributing to planning for the new ideas and new areas of interest.

NORBERG: How long has that been going on?

REDDY: For six years.

NORBERG: Okay, and were there predecessor groups that you know of?

REDDY: Yes, there was a predecessor group that was not meeting for a summer study, but Cooper had an advisory group, whom we met at Washington two or three times, once a year or something like that, or a couple of times a year. And this ISAT group is a kind of follow-on to that.

NORBERG: Do you remember the meetings that were held under Cooper?

REDDY: Yes.

NORBERG: You were present.

REDDY: I was there.

NORBERG: Can you recall some of the topics that were discussed?

REDDY: Let's see...

NORBERG: Maybe it's easier to ask you, were minutes kept?

REDDY: I am sure there are some minutes...

NORBERG: But you didn't see them.

REDDY: I don't... They may not even be minutes in DARPA office, but there are some people who are good notetakers who take notes and keep things. Al McLaughlin would be one of them, who is at Lincoln Labs. And Mike Dertouzos was there, he was the chairman at that time...

NORBERG: I have talked to him.

REDDY: ... of those activities. I don't know if he keeps things or not.

NORBERG: He said he didn't.

REDDY: He didn't. So I am just trying to see... I need to kind of go back and think. Newell was there. Newell usually keeps notes of things. He's much more careful about that than I am, so there may be things in his archives. Okay, so anyway...

NORBERG: So back to the topics that we were discussing.

REDDY: So, what was the question again?

NORBERG: Well, I am interested to know what sort of topics were discussed by this advisory group when Cooper brought it together, the nature of the discussion.

REDDY: The nature of the discussions were all of them formed both to communicate from his point of view what was happening, and then kind of solicit input from that group to say what directions, perhaps, broadly, and also, that was the time this whole strategic computing thing was evolving. It hadn't been put in place yet, 1981, 1982, 1983 I think. And there was also some concern about who was going to follow Bob Kahn. That came only towards the end of that, 1984, 1985, I think. What else?

NORBERG: Do you remember the discussions about the strategic computing initiative?

REDDY: Some. In particular, the discussions where Cooper said, "Look, now you are big boys. If you are going to ask for \$250 million, unfortunately we have to go through competitive procurement. And as long as your 50, 100 million, you're still in the noise level. We can do lots of things and get away with it, but we can't do that anymore." And I still don't believe that is absolutely correct. I think there may have been other ways of doing it, but he chose to do it that way. It was probably easier.

NORBERG: How did the competitive process work then?

REDDY: Before that?

NORBERG: No, after that.

REDDY: After that there was this so-called BAA mechanism. And there would be something that would come out

on the *Commerce Business Daily*. And everybody would submit a proposal, and it would go through a review process.

NORBERG: Okay, who were the reviewers? Not by name, but was it people outside the office or inside?

REDDY: I believe all the reviewers who were within the government. They may be people from ONR and other groups that have been brought in to help them with reviews, but it was all kind of determined, as far as I know, on an ad-hoc basis to provide a framework for deciding this.

NORBERG: You implied there was a competitive process before that...

REDDY: Yes.

NORBERG: ... when you were asking me the question. What do you think it was?

REDDY: The competitive process before that was more of an informal kind. You see, for example, when the original speech understanding program came along a lot of people said they would like funding, right? And so everybody kind of submitted maybe a short something or other. And Cordell Green, I think, was the first program manager that was still there, and then he kind of looked at all of them and said, "Okay, I have this much money and I think I can divvy it up in these five different ways and proceed from there."

NORBERG: Okay, so the decisions were made inside the office by the program manager.

REDDY: But it was still on the basis of some collective evaluation, or evaluation of relative merits of different proposals.

NORBERG: I will come back to that in a minute when we are talking about the further developments in speech

understanding. But let me go back to 1969 and ask you to elaborate a bit more than you did a few minutes ago about the decision to come to Carnegie Mellon.

REDDY: Right. The decision to come to Carnegie Mellon was mostly an accident. I was there at Stanford at the end of the three years. The faculty kind of unanimously recommended me for the department for promotion to associate professor. And it went up to the dean, and the dean said, "We already made an exception for Raj to keep him here as an assistant professor. We don't usually keep our own people. They have to go out and then come back, so I just don't think I can agree to this." At that same time I had an offer from Berkeley to go to Berkeley as a tenured associate professor. And Feigenbaum, I think, mentioned to Newell that I might be about to move because when the dean said no, it was clear that I was probably going to leave. So Allen called me and I came over, with no particular expectation of being here or not. And I was here for a day or two and met with Alan Perlis, Simon. And, whereas it took Berkeley six months to get me an official offer, it took Perlis one day. So I had these two and it was kind of difficult, but I came to CMU because of the presence of Newell and Simon, and because I was more interested in research. At Berkeley at that time there was no significant AI and no money. The computer science was kind of fragmented, so I just felt I could be here.

NORBERG: What other duties did you have at Stanford besides your research program.

REDDY: Teaching, and I had, I think, four or five Ph.D. students at that time.

NORBERG: So you had contact with the people in the computer science department down the street.

REDDY: Oh, all the time. Part of the reason is I was one of their own Ph.D.s, so I knew everybody, not just AI types. And even though we moved up into the hill into the DC power building I had an office and I spent more than half the time on campus teaching.

NORBERG: All right. When you arrived here at Carnegie Mellon what was the situation like? Who were the faculty?

What was the administrative arrangement for computer science and computing activity?

REDDY: It was kind of a fairly small faculty at that time, I believe - back then about 12 or 13. The main, key people were: in hardware there was Gordon Bell; in the software there was Nico Habermann, Bill Wulff, Alan Perlis, of course, and some of the other people. Floyd [?] had just left from here to go to Stanford the year before. And in AI there was Newell and Simon, and I guess I was the only other associate professor. There were a couple of professors and I came in as an associate professor. That's... What else? There was some work in performance evaluation - operations research type of thing - Jack McCreedy. But somehow that slowly died away. There was very little in theory. We had a couple of people, like Al Meyer, who went to MIT, and after they left there was a kind of long hiatus before we got some senior theory people.

NORBERG: Did you bring your DARPA money with you?

REDDY: No. When I came here there was some negotiation with Larry Roberts back and forth, and Larry's view was, "We have given you a pot of money here and a pot of money at Stanford. We would like you to kind of find a way of using it. And the thing that changed it was, Lukasik, in 1970, I think, was asking for, "What great ideas are there? What are the new things that ought to be undertaken?" And Roberts came and asked everybody, and there was a list prepared. And there were four or five areas that were identified as potentially interesting. And I think when he went to Lukasik and said, "How about computers that can hear and understand speech?" And I think Lukasik liked it as one of the new things. So that led to a meeting at CMU that Newell chaired of a group of people who produced a report called "Speech Understanding Report" in 1971, which became the kind of a blueprint for studying the research over the next five years. And so once that came along, new, additional substantial funding became available.

NORBERG: Okay, I heard another tale about the origins of the Speech Understanding Report, that is the group that was put together to produce the report. It had to do with a sort of a bubbling up from the community to do more work in speech understanding, not necessarily connected with any attempt at getting an overview of new problems by Lukasik.

REDDY: It could be. You know, the bubbling up is what happened, basically. You can ask Lukasik. He is still here; I saw him six months ago. The understanding I have is there was a bubbling up of the kind where people were asked what they thought were new and exciting initiatives. I think there was work at BBN; there was work at Stanford. Arthur Samuels tried to do something to continue the work after I left. And there was work here, and there was an attempt to see what we could do. And clearly, there was a statement that this is an interesting problem, but I think Lukasik gets most of the credit for deciding that's what he wants you to do, because Larry Roberts was more or less, I believe, neutral about it. His role was to get more money for the IPTO program, and if the way he got it was to get the money for new initiatives that was Lukasik liked, he was happy to do that. So that was the time, I think, it went from 25 or 20 million dollars to 40 million dollars with all these new initiatives. One of them was the speech.

NORBERG: Do you remember what the other initiatives were? I know this is reaching back, but...

REDDY: I don't remember.

NORBERG: One of them, I think, was distributed information systems.

REDDY: If it was, nothing happened. The networking people kind of continued. The speech thing suddenly started. There may have been something in natural language, but speech and natural language were lumped together in this program. The vision stuff came a little later in 1975, 1976, the image understanding program, along the same lines as the speech understanding program. The VLSI stuff came in 1978, 1979. And as I look back at the major thrusts of DARPA. There may have been some architecture. The C.mmp, Cm* I guess by that you mean those kind of architectures, multi-processor architectures. I believe they did come at that time. Whether it was part of that initiative that got started in 1970 or got started a couple of years later I don't remember, but those were some of the...

TAPE 2/SIDE 1

NORBERG: I would like to explore two things with you and to provide some sort of continuity here. What do you remember about the deliberations of the group that produced the speech understanding study? Because you were on that.

REDDY: Right. We all met right here. I think it was February of 1970... or I think it was April of 1970. The reason I remember is my second daughter was just about to be born, and I wasn't sure that I would be able to attend it or not. And based on that, there was an attempt to kind of formulate what are the issues and how we talked... there were a number of ideas proposed. And then we went and did a bunch of experiments, like one of a voice typewriter, or various other things, dictation type, data entry by voice, and so on. Each as an attempt to see where the opportunities were and where the excitement might be, and how we would justify it, and so on. And all those experiments were reported as appendices. And I haven't gone back and looked at it recently, but I think most of it was written by Newell and some of it was written by me, because it was all kind of put together here.

NORBERG: What do you remember was the charge to that group?

REDDY: Larry Roberts was here for the initial meeting, and said, "Let's kind of explore what it is that we think we can do with a major new initiative over a five-year program. And so there were people who wanted to do speech, and there were people who wanted to do natural language. And the statement was, "Maybe we can combine them together and produce a speech understanding system." And so that was an attempt to distinguish the state of activities from other groups of activities.

NORBERG: Why do you think they didn't just put out some sort of a request for proposals from the DARPA office rather than putting together a group of this kind?

REDDY: I think the rationale was kind of the same thing that is now happening with respect to ISAT studies. Sometimes when you are looking at a brand new area where the amount that is known is fairly minuscule, the way you begin to flesh out the issues and the problems and the opportunities is to bring a group of people who are

knowledgeable broadly, but haven't maybe thought about this area in depth, and then can deliberate. That's what happens with all the National Academy meetings. They pick a topic of national interest and say, "What can we say about it?" You'll find that there's not much any one person can say. But when you bring them all together and spend some time deliberately studying it, then several ideas come together, which you can put together into a report. I don't think the National Academy model was at the top of anybody's mind when they were doing it, but I think that's what happened then, and it's still an appropriate way of looking at a brand new area where there's no history of careful study.

NORBERG: What happened after the report was submitted?

REDDY: So the report came out, I believe, in 1971. Five groups were funded, and the stated expectation was at the end of two years only two groups would be funded to do more. That was kind of a difficult, nerve-wrenching experience for everybody in the DARPA community. It led to a lot of competition, or apparent competition, although everybody was doing things their own way, perhaps there was not as much communication as there could have been. But there was. You know, there was probably more communication than if there was no competition, because people were getting together every six months and making progress reports and demonstrations and show-and-tells, and so on. The whole system, I think, worked. In retrospect, it's probably one of the best experiments that DARPA did, because there's a measurable metric progress that one can see that happened at the end of the five years, which we could not do before. Now, some people will look at it and say, "Well, that's nothing, because of this, that, or the other. And it's not my favorite problem. Dreyfus' standard trick is to say, "You have done this, this and this in AI, but you can't do this." Well, the next day you do that and they say, "Well, that was ? ...

NORBERG: [laugh]

REDDY: It's a moving target. But again, as most of the ideas I had of all the different issues and so on were summarized in that paper. And I go back and I look at it even now and it's still directly relevant.

So I think the one thing that the program did is remove a lot of the mystery of the nature of the problems, and lay foundations on the directions that the field would move. So, for example, ? -Markov models were one of the things that came out of that, the Dragon system that was involved here. It took another six, seven years for the rest of the world to adopt it, until the early 1980s. Before that, everybody was doing simply dynamic program template matching, and that worked pretty well. But in 1982, '83 suddenly there was a big shift over to... And now, everybody is working on ? -Markov models. My own current feeling is at some point we will kind of move to other paradigms of learning. To me, people kind of somehow automatically think ? -Markov is the whole thing. All it's doing is a very narrow part of the overall problem. It's trying to decide how to model an acoustic phenomenon. The language is not modeled using the ? -Markov model. All the other things are not modeled; it's just what does a sound look like in different contexts, and the automatic learning of that property. But all of those ideas came from the original speech understanding program, the use of syntax and semantics for their role in speech recognition. Some of the ideas have been in ? -Markov models. It was kind of co-invented by IBM at the same time with a slightly different formulation. In fact, you can actually do real-time, connected speech recognition in restricted domains even on those days' computers, which was not something that was viewed to be practical or possible.

NORBERG: And when you say that people got together every six months or so to discuss progress, who was present at these meetings?

REDDY: There was a committee, essentially the same committee of people, I think, that ordinarily help put together the proposal. There were a bunch of... some of them were already contractors, and there were a few people that were not directly working on the thing. And these were people like Newell and "Lick" Licklider and Dennis Klatt and so on, who were contributing purely by being in an evaluative sense, being there. And so that's the group. The DARPA program manager, whoever it happened to be at that time. I think it was Steve Crocker after Cordell Green, and the main research people.

NORBERG: Was this group actually making statements about progress in the field back to DARPA?

REDDY: Yes, I am not sure they were actually making progress statements, because the key people from DARPA were part of that group there, and they were kind of aware of what was going on.

NORBERG: Well, who was going to make the decision about the two remaining PIs when the cut had to be made from five to two?

REDDY: This group, I think, and they did. SRI, which seemed to be the weakest of the lot, did get cut out. BBN and CMU were the two that kind of survived. And there were others... Let me see, who were the other ones? There were a set of, you might call them subcontractors, or technology contractors, who did not get affected. There was something, I think, at Lincoln Labs. They kind of got given a different assignment at that point. I think maybe there were only four; I was thinking there were five...

NORBERG: I wasn't questioning that, but what I was going to go on to say is that, can I consider this paper that you produced in late 1975 as essentially an evaluation of the state of the field in 1975?

REDDY: Right.

NORBERG: And therefore, a possible statement of what needs to be done for follow-on work?

REDDY: Right. There was also a follow-on work reported that was produced.

NORBERG: Okay, which is different than this publication.

REDDY: Yes. This probably was written in 1974; that was produced in 1975, but it never got acted on.

NORBERG: Which was produced in 1974? Which in 1975?

REDDY: This paper was written, I think, in 1974, although it was not published...

NORBERG: Yes, it just says it was received September 1, 1975.

REDDY: Yes, okay, then it's 1975. At the same time... It was like six months or eight months in writing. At the same time there was this follow-on report that was produced by the same committee, the Newell committee, too of what could be done, which was essentially rejected by Heilmeier. And I think Heilmeier just came in and said, "I don't want programs to continue beyond five years." I don't know whether he heard anything negative about speech, but in retrospect he obviously doesn't believe in the decision he made, because at TI he supported a lot of speech research and now he's at Bellcore. So I think it was more of a political decision rather than a careful, considered evaluation of progress in speech.

NORBERG: Did your research interests begin to change at that point when funding seemed to be going away from speech?

REDDY: The nice thing is they did recognize CMU did make major contributions, and we never lost our funding substantially.

NORBERG: For speech.

REDDY: Speech. All they did was they moved a lot of the work we were doing back into basic AI and kept funding us at not the same level that we were being funded when we produced the Harpy/Hearsay systems, but at a somewhat lower level. So we used to have about ten people, I think, twelve people. We went down to around six. And some of the key people again went off and started other things. Victor Lesser [?] went to U Mass as an assistant professor, who is now a senior person there. Hayes-Roth went to Rand, and Lee Erman went to ISI, and from there, after a few years, they went to Teknowledge. But all the key people, there were about four or five of us with Ph.D.s that were working with me, and they have kind of all gone on to other things, didn't come back to speech.

So you could say that's a negative result of the funding cut. But at the same time, in some sense, they were young and this was the first research they did. There was not the same kind of commitment by them, for example, that I have held, and I would still do research, and even if DARPA funding goes away completely, will probably continue to do speech recognition research. The question is, what could we do, and what difference would it make?

NORBERG: All right, let's go back to 1969 when you came here then, and let me ask you what sort of research program you set up when you arrived.

REDDY: The same. You know, basically I was working there on speech, vision, and robotics. I didn't start with the robotics thing, because there was nothing much I thought of I could do with a manipulator. And so we set up an image understanding research program. One of my students was... We were funded by DARPA at a lower level. There wasn't an IES program at that level. It was Ron Ohlander, who then went back into the Navy and then into DARPA, and from there he went to ISI, where he is deputy director or something. And he did some of the initial work on color image processing and region splitting and some other work... And that worked out well. And there was some work on image understanding, on change detection that was done by Keith Kreiss [?], who is at USC now as a research professor. And Steve Rubin did some work on matching against a model, model matching. And he then went on to do computer graphics and other things at Schlumberger Labs.

[INTERRUPTION]

NORBERG: You were describing the various work that people were doing here in that period of the early 1970s.

REDDY: So basically the answer to your question is when I came here I started the speech program and kept going at it, and that included all the other people that I mentioned. And then there was the image understanding program that was happening in parallel. Those were the two broad areas that I was active in. Oh, there was another project I started at that time, which also has had a kind of interesting impact. I used to be a consultant for Xerox PARC from 1970 to 1980, and in order for me to kind of do some work for them on the computers here, I said, "Why don't you also

give us a free laser printer?" At that time it was not a laser printer, it was a selenium drum. Actually it was a huge fax machine type of thing. And we were able to interface that into a PDP-11 and produced the first laser printer outputs. The type of outputs were computer-generated fonts and text and other things. And I kind of got it mainly because I was trying to produce displays in wave forms and other things, a hard copy of it, which was very difficult to do any other way, using plotters and so on. That became kind of a standard printing device here, beginning in about 1971 or 1972, and had a life of its own. The third project we did at that time was when we came here there were a few hardware engineers who were interested in different kinds of graphics. And we needed graphics for doing some of the speech work. So they built some graphics workstations called GDPs using very high-speed DDA converters, so that it was able to draw visitors at much higher speeds than previously possible. And that work kind of went on to lead to a spin-off company here called Perc Systems, Three Rivers Computers, Perc Systems, which produced one of the first workstations, and might have been like a Sun or an Apollo, but for the fact that they were in Pittsburgh and made two or three wrong decisions along the way. But they were the first to try to build workstations and get in to the market.

NORBERG: Using the basis of their results from the research they had done here?

REDDY: No, the basis of the research was the one that spun them off. They were selling graphics terminals, for instance, and then they switched into making workstations.

NORBERG: There are a number of ways I could go with this, but given our time constraints, there's one other area that I do wish to cover, and that is, when Newell went on leave in the middle to late 1970s, I understand that you then took over most of the interaction with DARPA. What did that entail?

REDDY: Mainly working with our program managers, Dave Carlstrom, and a number of other people there. In particular, interactions with Bob Kahn. Bob was always supportive of CMU's work. We had kind of little ups and downs in funding, but it was kind of growing at some level. But with the image understanding project coming along, around 1976, since we were already doing image work here, we were a natural group. And then Takeo Kanade came

around 1978 and then that became a major activity in itself, probably one of the largest activities in the CMU now, probably around the country. The nature of my interactions with DARPA were all kind of identifying research areas and then helping to promote whoever was here, and then getting them the funding. I was more of a catalyst than a principle investigator. I think my name was on as the PI for four or five years. I was not actually managing all of this research. I was more of a catalyst for all of it, and a facilitator for all of it, because I had my own research project. And broadly, that's the kind of interaction. There was nothing unique about it, but somebody had to manage it.

NORBERG: Sure. Okay, but what I am interested in is how, then, was the interaction structured? Did you talk about specific projects that were to be done, or was there an overarching grant of money to the university.

REDDY: It was opportunistic. Basically, I remember having a long talk with Bob Kahn about VLSI before he even started the program, because I was interested in it from a different perspective than them. I said, "Bob, there's all this stuff that's happening at PARC," which I knew - Carver Mead hadn't published his book yet. And I said, "DARPA needs to find a way of setting up a program whereby we could design our own chips." And I think he talked to a number of other people and he got the same thing from all of them, and then he set it up. During that time Bob Sproull came here, although he was in graphics. I said to Bob, "Of all the people around, you're probably the one that is most appropriate to become the PI for this. Would you like to take it over and see what can be done?" And he said, "Yes." And the same thing happened with respect to the whole distributed sensor nets area. That was the other area that Bob Kahn wanted, and both of us were starting. That involved signal processing sounds and interpretation and so on. And which, in the beginning, we decided the most important thing to solve is the distributed operating system problem. So Rick Rachtel [?] was here; he said, "The money is for overall sensor nets and other things, but why don't you spend as much money as you need on kind of getting the operating system thing?" And that led to Spice, and Spice led to MACH, the new MACH operating system, thus the new computer. And it kind of has its origins in the distributed sensor net project that Bob Kahn and I started here. The third area was with Duane Adams on graceful user interfaces - graceful interaction. Another area I was kind of interested in was a paper by Gene Ball and Hayes and me called "Breaking the Man-machine Communication Barrier." I don't know if you have seen it. And that was something where there was an attempt to understand, tried to articulate, do

some protocols and see what are all the different things we could do. And intelligent help facilities and identification from description, and there were flexible parsing. There were about five or six missing science items we identified in that and started work on that. And DARPA funded that, and multi-media user interfaces. So many of the things that are currently now beginning to have a second birth and a second generation were all started in about 1979, or so. And it kind of led to a degree of funding and...

NORBERG: When did speech have a reincarnation in DARPA?

REDDY: Speech had a reincarnation in 1984 as part of strategic computing. So from 1976 to 1984, we were funded at a base level, with about five or six people working on it and doing various things, but a significant new spurt came about that time.

NORBERG: What was the spurt expected to accomplish?

REDDY: It was primarily an attempt to demonstrate a spoken language resource management system for Navy ships' databases, or something like this. It wasn't articulated that way at that time. We were trying to find a task, and we knew we would find some task that's common to everybody. But everybody was going to start, and CMU was going to be the overall systems integrator, and the other people would do different components to set it up in a different way. And as usual, everybody wanted to build their systems, and this idea of trying to integrate components from different places didn't quite materialize. And so we abandoned that part of the role and said that we're not going to do that, because nobody else really wants us to do it anyway; let everybody do their thing. And we built our systems and we continued to hold our own in the community. So if you talk to people, like...

NORBERG: To who?

REDDY: Charles Wayne, who is the project manager. So, let's see. Anything else?

NORBERG: Well, there are a number of things I could ask, but I realize you have another meeting to attend. Let me just ask one more question about this issue of application, and that is, do you think that the justifications that you and your colleagues needed to provide in the 1980s required a good deal more emphasis on application or not? Could you still fund the basic research program in the way you had done before?

REDDY: I think the major change came in 1984 with the Strategic Computing Program. Until that point, if I go back and look at all the proposals I submitted with me as a PI, they were kind of discussions, handshakes, who has money and who has a problem, and whether they would work or not. And then we would submit a composite, single proposal for three, four, five million dollars; I think eventually it became six million dollars a year. And then when the strategic computing came then we had a separate proposal for that.

NORBERG: Yes, because I have seen lists of contracts and grants to CMU's computer science activities in 1979 that totalled six million dollars from DARPA, and eight million overall, the other two million coming from NSF and NIH. And that seemed to me to be a very heavy emphasis on DARPA funding.

REDDY: Right.

NORBERG: And the question is, how does one justify all of that? It certainly isn't all going to basic research.

REDDY: No, it was actually going all to basic research.

NORBERG: How would you define basic research here?

REDDY: So basically, there is all the activity that was going on in original AI. So if you look at the increments in funding from about two million to six million dollars, it came for distributed sensor nets, image understanding, and graceful interaction. Those were the three.

NORBERG: I see.

REDDY: And those were the three that provided another two million dollars. And then, if you look at where the funding went from six million probably to twelve million in 1984, it's again the big emphasis on WARP and hardware design and production of high performance computing, and a big emphasis on the MACH operating system, and the distributed operating systems. That's where the other major chunk of increment came in.

NORBERG: Well, very good. Thank you very much. I appreciate your time.

REDDY: You are very welcome.

END OF INTERVIEW