An Interview with

ALLEN NEWELL

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Abstract

Newell discusses his entry into computer science, funding for computer science departments and research, the development of the Computer Science Department at Carnegie Mellon University, and the growth of the computer science and artificial intelligence research communities.

Newell describes his introduction to computers through his interest in organizational theory and work with Herb Simon and the Rand Corporation. He discusses early funding of university computer research through the National Institutes of Health and the National Institute of Mental Health. He recounts the creation of the Information Processing Techniques Office (IPTO) under J. C. R. Licklider. Newell recalls the formation of the Computer Science Department at Carnegie Mellon and the work of Alan J. Perlis and Raj Reddy. He describes the early funding initiatives of the Advanced Research Projects Agency (ARPA) and the work of Burt Green, Robert Cooper, and Joseph Traub. Newell discusses George Heilmeier's attempts to cut back artificial intelligence, especially speech recognition, research. He compares research at the Massachusetts Institute of Technology and Stanford's Artificial Intelligence Laboratory and Computer Science Department with work done at Carnegie Mellon. Newell concludes the interview with a discussion of the creation of the ARPANET and a description of the involvement of the research community in influencing ARPA personnel and initiatives.

ALLEN NEWELL INTERVIEW

DATE: 10 June 1991 INTERVIEWER: Arthur L. Norberg

LOCATION: Pittsburgh, PA

NORBERG: Can I ask you to briefly describe for me, as you understand it, the origins of this field in the 1950s? And the reason I am asking the question is I want to distinguish the various approaches and the reasons for their emergence. Why Carnegie Mellon is interested in complex information processing as a group; why the MIT people seem to be more oriented toward machinery, and so on.

NEWELL: So this is the field of AI you're talking about.

NORBERG: The field of AI.

NEWELL: You will push me in a moment, but it's not clear that I can give you more than what has become a standard mythology on this, which is to say that from my point of view there is this highly personal and direct relationship between Herb Simon and myself, which starts out at Rand (it's been written about, so that's sort of available), in which we both start out as people concerned with psychology and behavior. Herb, of course, is professionally from that area. Me not, when you look at my background. Organizational research was concerned with psychology. I actually joined the American Psychological Association in 1952, so it goes way back. So I am, although a bunch of odd things in terms of education, I am really operating as a psychologist, and Herb, of course, is as well. So, in fact, that whole orientation is sort of absolutely there in simply the set of problems that Herb and I are dealing with. And when we come back to CIT [Carnegie Institute of Technology] - Herb is here anyway, but when I come back it is the same thing. Now, what brings me back to CIT is officially the issue of continuing these organization studies. And in fact, there was an immense amount of planning fantasy and all the rest of this about how we were going to create a... Do you know anything about the Systems Research Lab at all? Is that part of your world? There was an attempt to build a laboratory at Rand... more than an attempt... a real attempt to build a laboratory at Rand, where they did organizational studies in which groups worked for up to a month in air defense situations. And we thus had 20 to 30 people working continuously eight hours a day gathering massive amounts of data on their behavior, trying to understand that. It was out of that that SDC (System Development Corporation), systems training and the air defense command, and all that sort of stuff evolved. That's a whole big area and full of its own great ideas. So when I came back to work with Herb, who is deeply involved in organization theory, which in fact is the thing that brings us together, that is sort of the imagery that's going on between us. And so, there's lots of fantasies about how we're going go do that kind of organizational studies here, and so forth. The concern with individual behavior, the concern with trying to do something in terms of understanding reasoning and thinking and so forth, sort of grows up within that. It doesn't happen after I am here; it happens sort of in the process of coming here. And it happens again; there is this piece of true mythology about Oliver Selfridge coming out to Rand giving an afternoon seminar for the four of us, which just totally turned me on. And within the context of understanding the behavior of the tellers and the guys who draw on plexiboard, which I can't remember what they're called, that behavior was the behavior we were analyzing. So we were analyzing individual behavior within an organizational context, but then shifted therefore from the real organizational concerns to this issue of individuals. That was sort of going on after I was already interacting deeply with Herb and made the commitment to not go to the Center for Behavioral Sciences. So Casbaugh [?] was just getting started in his first year, and I made the choice. I was one of the so-called junior fellows.

NORBERG: Where was the center?

NEWELL: The center at Stanford, that now exists at Stanford [Center for Advanced Studies in the Behavioral Sciences]. This was its first year; 1955 was its first year. They have had a great stratified system, called senior fellows and junior fellows. Junior fellows were supposed to go sit at the seat of the senior fellows. I was one of the junior fellows. When push came to shove I decided I would rather go to work with Herb and came back into this environment with all of this organizational and social psychological and performance stuff uppermost. Although what I had been mostly concerned about was the computer technologies for building the simulated system. So I was sort of deep in that aspect and thus not deeply involved.

NORBERG: Let me ask two questions about that, please. One of them is, this sounds to me like the interaction of you and Herb Simon...

NEWELL: Right.

NORBERG: ... was a highly personalized...

NEWELL: Sure, absolutely.

NORBERG: ... thing, rather than a whole group of people who happened to be at CIT at the time.

NEWELL: Yes, absolutely. Absolutely.

NORBERG: And the only connection then would be with a similar but not identical kind of activity going on in organizational studies and behavioral studies at Rand. Is that fair?

NEWELL: Yes. Herb essentially is an organizational theorist. He is lots of things, but he is that. And so he found us at Rand. He was out there doing his thing as an economics consultant with the decision theory people. We were a bunch of peculiar characters off on one side doing these organizational studies. So he found us and was fascinated. He came over just to talk to all of us, and then he and I began to... So very personal, absolutely personal, the two of us.

NORBERG: And my second question is how did you become interested in the computing machine side of this?

NEWELL: I think the only answer to that is that as we cranked up... This is now actually before... Does the name by the name of Bavelas mean anything to you? Alex Bavelas was a psychologist; a mysterious psychologist because he never published, but was famously known all through psychology. He did a series of so-called task communication studies in which he put people in a room with partitions so they could only communicate by messages, and so initiated a whole area of small group studies based on these task communications, where humans only communicated with little messages so you do task analyses. People at Rand got very interested in that. In fact, John Kennedy sort of came out to Rand because of Rand's electrical engineering department's (whatever it was called) interest in this. I got very interested in this. So I am sitting in the mathematics division, which is a free-floating division within Rand, famously so. Williams... a guy by the name of Jim... No, no... Anyway, famous guy, because he was not a Ph.D., an astronomer, worked at OSRD during the war, became the head of the mathematics department at Rand. And he had a habit, which he actually did on me, in which the day I walked in when I took my job out there, he said, "I have just bought all your time for the year. Here, I give it back to you. Now, go off and do something interesting." I mean, it was his standard... J.D. Williams... his standard thing. So I was floating around doing whatever I damned pleased. It was a hotbed of game theory. I turned out not to be terribly excited by game theory. And I got interested because of some of this other stuff, and some of the summer studies, in looking at small group behavior. And I had an immense amount of interaction with a guy by the name of Freed Bales. R.F. Bales was a professor at Harvard in small group studies, who was again a consultant out at Rand. Rand was a place where there were hundreds of consultants every summer. The summer was actually occupied by consultants. About a hundred of them came out there. So I was working on small group studies, although I was in the math department, and just, in fact, becoming a scientist. I mean, I was just a kid. And then, in fact, we invented this issue... It's a fairly complicated story, which I knew no part of, because I am in some sense the junior member of it. John Kennedy is the entrepreneur in this. Sort of this idea of large scale organizational studies to study the air defense problem in a real context. And as we got into this, three or four of us, mostly psychologists, John Kennedy, Bill Diehl, and Bob Chapman. They were all psychologists, and it kind of fell to me to get concerned with how to build the simulated environments. So I went down to the computer center and there was this non-talking character called Cliff Shaw, who I proceeded to talk with. And so Cliff and I sort of went after, trying... There weren't any electronic computers. There were only things like the 604s and so forth. There were only things like the early IBM computers, all of which you know about. Everyone knows about those things. But it was all before the 701 showed up.

NORBERG: So, even the JOHNNIAC is not available yet.

NEWELL: The JOHNNIAC isn't available at all. I don't even think they're building it when this starts, but they must have been; I just wasn't aware of it. So anyway, Cliff and I sort of made do to build this simulated environment of all these radar scopes in a true air picture. So there I am getting involved deeply in computing technology, whose chief characteristic - so the whole world is determined by our odd things, as you know - whose chief characteristic is that it's absolute non-numerical computing. So I have never done any numerical computing in my whole life. My whole use of computers back to this very first job was how to present simulations of radar scopes for a whole complex organization with a highly rich and intensive situation that... Well, there's actually an interesting thing here. The thing I learned from these Bavelas studies is that they were too lean to elicit real human behavior. One of my problems was that the subjects I had got for this... I would gather some subjects out there for the summer to go play some of these little games turned out to be characters like...

NORBERG: These are people we're talking about.

NEWELL: These are people. Okay, so I set up a little situation to do these four person, small group experiments where they passed messages around and I had created some kind of an artificial task for them to do. Now, the people I got as, I won't mention all four of them, but two of them were a guy by the name of Kleene, which may be a familiar name to you...

NORBERG: Stephen Kleene from Wisconsin?

NEWELL: Yes, sir. And a guy by the name of Lloyd Shapley, who may be familiar to you from game theory.

NORBERG: No.

NEWELL: Well he's one of the more famous people in game theory. And another guy by the name of Mel Howser, who turned out to be one of the world's great topologists. So sitting around in this thing turned out to be some fairly bright people. So they would take this little problem I would get, and without communicating with each other they would sit back and think about it, figure out how it ought to be, and then go behave accordingly. So my whole early experience was I couldn't get situations which were complex enough to actually elicit the real human behavior, partly because these damn guys as subjects were infinitely smart. But what that meant was that when we got into these larger studies I was really concerned about getting a rich - I am going to sound like Gibbs - a rich, thick environment that would have enough knowledge in it so that the people would have to behave like normal people in the real world. None of this issue of an artificial situation where they could overpower it with their own brain power. And so we engaged in deep simulations. They don't look so deep when you look back on them, but they seemed to be at the time. We took actual air situations, the one up in Washington, for example. We got all the flight plans from up there. We modeled the actual flights. We had the frequencies right. We worked on this map of the area. We got as much as possible of the actual air situation into the simulation. And then of course, what the 20 guys in this Air Defense Direction Center saw was in some sense what they saw on the radar scopes. And we simulated what the next character a hundred miles away would see, and these guys who were generating scripts... All of that had to be enough to produce eight hours a day of simulation for these groups.

NORBERG: Now, what were they going to do with the simulation?

NEWELL: They essentially were a crew in an Air Defense Direction Center in which they would sit there and process the flights. And pretty soon some Soviet characters would come floating in, and they would have to recognize that these were in fact enemies, and sometimes they weren't enemies. They would have to track them. They would have to decide when to scramble. We started up with college kids, but then the next two experiments that we ran were done with military forces. We actually got a military crew assigned in there, so we actually had Air Force officers from the Air Defense Command essentially living in their little bubble, which only knows the outside world from what the radar scopes and from the information that is told in from the outside. So they essentially ran the center all day, every day just like a shift. We actually gradually poured in heavier and heavier loads of planes. The bottom line - it's all irrelevant for this, probably - but the bottom line of this whole thing was that the systems analysts who were designing Air Defense Direction Centers had some theories of how much load they could handle, and whether they could handle them. And they were, as you would expect, simple linear theories. And so, in fact, they had things that the Air Defense Direction Center could handle 15 planes, or something. Well, we showed that in fact if you start pumping planes in on these characters they will learn how to filter stuff and work with stuff and so forth. So we had a graph that sort of showed over the course of the month that the capabilities of these people went up by orders of magnitude. This organization, because the whole focus was not on individuals. The whole focus was on the team working together. So this was real organizational stuff. Fundamentally, that's all we ever found out in that situation. There was lots of talk about other things. Out of that the Air Defense Commands decided that it wanted organizational training for its actual troops in the field, so they created something called "systems training," which was to provide the simulated situations out in the field. And then they created a corporation called SDC, which was the outfit that could do this. And SDC got diverted, because it also picked up the SAGE programming task, which was not what it was supposed to do, but it was a vehicle around, so it did both of these tasks. And for several years there was air defense training all over the United States. There actually were some situations in which the entire nation was being run on a single simulated thing for three or four hours, lots of talk about choosing it at random so the Soviets wouldn't attack us. When the whole system was down, working on a simulated task so that the headquarters and so forth would also get training in how to interact with it. I was out of thing [?] by then.

NORBERG: Okay, so your interest in this started on the machine side.

NEWELL: Right, and you can see a lot of the elements. No concern with numerical computing; total concern with sort of non-numerical stuff, not artificial intelligence, but non-numerical stuff. Concern with human behavior was very much involved. It goes way back before I met Herb. So when Herb and I got together then there was all this concern about organizational behavior mostly. But as the opportunity arises in terms of thinking about the computer as a model then you get focused on human behavior, so both of those get going simultaneously. The shift, I think, done by the computer itself away from the organizational level, but that had not taken place completely by the time I left. It was just there by the time left. And coming back here, because this is GSIA [Graduate School for Industrial Administration], concerned with organizational behavior, lots of that stuff in the air, which then just gets beaten out, just plain beaten out of me and beaten out of Herb. I mean, it doesn't get beaten out of Herb in the sense that he does, in fact, continue all of this other theme. But in some way Herb's own life shifts over to this individual thing. So he finally moves out of GSIA into the psychology department as an individual thing. All the non-numerical stuff. What Oliver Selfridge does when he comes down is to talk about a little crazy machine called the MTC, the Memory Test Computer, which was hardly a machine worth talking about, but that they were playing with. They had done a little pattern recognition task on it. And that just made it clear exactly what was going on as far as I was concerned. I mean, absolutely clear.

NORBERG: In what way?

NEWELL: When I heard Oliver talk about that, I understood exactly how computers could do arbitrary information processing, symbolic processing, adaptive processing, and so forth. And it was quite clear to me what the path was. It was as close to a conversion (not quite)... but to a conversion experience. I mean, it was a conversion experience. I walked out of that room - Cliff wasn't at this meeting. I walked out of that room; I walked down to Cliff's office. I went in; I gave him the whole hour lecture. I just had it all. I gave it to him. I went home that weekend. I designed a whole system like that for our air defense, for understanding the people doing the air defense direction center, and I never looked back. And it really did happen just like that. I was all primed for it, literally primed for it, but the conversion to deciding that that was it, and that I knew exactly how it was going to happen, happened mid-November 1954. But you can see all the stuff that prepared for it. Conversion experiences are all the same.

NORBERG: Yes. Then you came here.

NEWELL: Then I came back here. And we spent a certain amount of time talking about these other things and mostly what we started working on... I was going to graduate school. Going to graduate school is sort of a blur for me. I mean, I mostly remember working on the Logic Theorist and chess. I went back out to Rand in the summer. I was still a Rand employee. I went back out to work at Rand in the summers with Cliff, and each of these steps now were internally, technically driven. So I only have an internalist's picture of all that happened. Things like the invention of list processing was totally internally driven.

NORBERG: Those things are discussed in Pamela's book [*Machines Who Think*]

NEWELL: Yes, as I have told you, that's a good book.

NORBERG: So we really don't need to go over that. But the mention of Selfridge suggests to me that I ought to ask you about interaction with other people as time passed in the late 1950s, when you were here now. And I will assume that while you were ostensibly a graduate student, although I think that's sort of a misnomer in your case, while you were ostensibly a graduate student here I am going to assume that there wasn't a lot of contact with the outside. You were just trying to get your tasks done and get your dissertation done.

NEWELL: No, I don't have a dissertation.

NORBERG: Well, whatever. In order to satisfy the requirements, you were involved in a fairly internally driven process. When that's over do you then begin to have contact with the people in what is now called the AI community? And what I am trying to drive at here is what did they call themselves? What do you remember them calling themselves at that time?

NEWELL: No, because they didn't call themselves anything until the Dartmouth conference.

NORBERG: That's 1956.

NEWELL: 1956, so that's not so far away. It's a fairly small period of time. And John [McCarthy] makes up that name partly out of real frustration, his terrible difficulties in dealing with Shannon and all these other guys. It's a story he has told before, though I don't know anything directly. I do know that I gradually get to know John and I gradually get to know Marvin [Minsky] all through this period. And the Dartmouth thing is a piece of it, but I don't recollect it as the only piece. And I don't have the sequence well-formed. I mean, one of the things that's funny here is that John and I were graduate students at Princeton together, but we don't have anything to do with each other. I am only at Princeton for a year before I take off for Rand. And I can remember funny, competitive things way back, because it was always the case that the three of us were sort of very cooperative and very competitive. We were sort of identical ages; we were sort of all in this, and this is an unnamed thing yet. And we don't see it as a field yet. I mean, that's not true. McCarthy may easily have seen it as field or something, but I don't see it as a field. But I remember meeting John at IBM one summer, and I guess I could probably track down the summer. He was visiting Art Samuels, and I happened to be up there visiting Art Samuels. And John started talking about the need for languages and so forth. Well, hell, I already had a language designed and written in list processing, which was a chess language, as it turns out. So I had already sort of gone down that path, and I kind of remember that afternoon sitting around up at... wherever it was. I am not sure... Yorktown didn't exist then, but it was up in that part of the woods. So there was in fact kind of knowledge across. On the other hand, for instance, in 1956 Herb and I - this is prior to the Dartmouth conference... There is this funny set of interactions with Shannon in which Shannon invites us I believe to that symposium. I think Shannon may have set up that... This is the Information Theory Symposium. I think Shannon was the actual guy who set it up.

NORBERG: That's correct.

NEWELL: So we have a letter. And in this letter there is some stuff about a guy by the name of Trench Moore. Trench was a student of Shannon's, and in fact Shannon thought that what Trench was doing was infinitely further ahead than what we were doing. So there's this funny little exchange and counter exchange and so forth that relates to what Trench is doing and so forth. I didn't meet Trench until the Dartmouth Conference. I didn't meet Herb Gelertner until the Dartmouth Conference. I am sure I must have met Nat Rochester, because I probably knew Nat... You see, from the Rand position you sort of have a real central position, and in particular, it turns out that as we got into this whole area of simulation, not for us, but for the Air Defense, we went around the country and talked with every manufacturer. The state of the technology then was incredible. The things we were worried about were was there an electronic machine so that you could do conversions from IBM cards to tape in hurry? And the answer was there wasn't any, but there were people who were willing for $200,000 to build you one in six months. [laugh] The 701 was floating in the background. So we spent a fair amount of time at IBM, and I am sure I got to know Nat at that time. So there was a way in which I, as a real, young, bright guy, coming out of Rand with this incredible project and going around the country talking about all the things with the different manufacturers and so forth, all produced a state of connection and awareness. It was unrelated to artificial intelligence, but it was certainly related to computer technology. I remember staying at Nat's house one time way back in there somehow, and I think that was well before Dartmouth, talking about the history of the 701 and some of the politics of it and all the rest of the stuff. So I pretty soon got to know these guys, and I can't remember any of the sort of initial events for any of them. They were not memorable. And the Dartmouth thing, I think, had the real effect of pulling everybody together, although we were fairly offish with respect to... partly because we had already done something. So I don't remember the Dartmouth Conference having any big impact on me at all. It sort of passed in the night, because, in fact, Herb and I were totally consumed with our own path, so no notion of a field. Just the notion of going off and proceeding to do these things. Now, the notion that one had done something great was absolutely there. Never afterwards have I ever done anything such that there was a spate of mail from people of all kinds, most of whom I didn't know, technical people, saying what a great thing the Logic Theorist was. And a lot of people in Herb's field - Laswell, for instance, sent a letter of Herb sort of saying, "Wow! Congratulations! This really looks like a real breakthrough kind of thing." And this is all happening in 1956, 1957, this period. So there is certainly not a notion of working in a tunnel. There is a notion of being connected with the whole world. Now, a lot of that happened through Herb, of course. Herb is connected to at least one big part of the world. But again, Rand produces this other kind of connection. So I never found myself worrying about that. I just took all that for granted, and it all happened. There was never anything that I would call deliberate planning by a cabal with respect to the field of artificial intelligence. Now, I might actually be wrong on that, but right at the moment you will search the old memory and there's all kinds of understandings between Marvin and John and myself and Herb about what was happening and where things were going, and there were differences. Although not quite the difference between AI and psychology, because, after all, Marvin, unlike John, comes out of a deep concern for dealing with... You know, its the neural net thesis. He doesn't come out on what you would now call the AI side. He comes out with a concern for how to simulate mind, how to understand mind.

NORBERG: How to understand it? Simulate it and understand it I don't think are the same thing.

NEWELL: They're too close to make any difference. What does Marvin do for his thesis? You probably know all about that. He's building machines, but he's building them to understand how to put neural nets together to do mind-like things.

NORBERG: Oh, but the way he described it to me was that he was interested in, as you used the word, simulate - that's right. He was interested in simulating intelligence. He didn't care whether the underlying substrate that produced the result in a machine was the same as the way it is produced in a human being. And therefore, understanding intelligence is irrelevant to him. That's my interpretation of what he told me.

NEWELL: Sure, he's undoubtedly right. But, the way he looks at it and how John McCarthy looks at it are very different.

NORBERG: Yes, indeed.

NEWELL: And in the sense, I'll take Marvin not as a technologist, and I will take John as a technologist. I'm talking about these early years.

NORBERG: Yes.

NEWELL: ... in which John's view was mathematical and technology, and Marvin's was not. Marvin always had this element that comes out of working with neural nets. You don't think of neural nets as a technology to go do things. But that's very separate from the psychology. So the thing that provides the cutting plane is that Herb and I always have the psychology uppermost. Well, you have got to be careful, always. But, in fact, the whole early part of this thing at GSIA is loaded with concern for management science. There was a whole little field called heuristic programming, which is now kind of a dead term, which got created there precisely because in that environment you can worry about the psychology and you also worry about how to do operations research. And Herb, as you know his history, is deeply involved in things like running a paint factory and using servo mechanisms. So again, not computers and psychology, but just technology, operations research, understanding organizations and how they function in a practical sense, all of those things deeply embedded with Herb. At that time he's concerned. So that same duality is there in Herb at the organizational end. I've never quite thought about it this way.

TAPE 1/SIDE 2

NEWELL: It doesn't seem at all odd to a guy like Herb, who never in one sense compartmentalized himself this way, and that doesn't start... But this shows, which I had never thought before... This doesn't start with sort of the computer end of the thing. So now, Herb himself, you see, has his own world-like resources. There was a grant from the Carnegie Foundation - 40K. And 40K was worth something in those days. And so things got fueled and things got done. And GSIA was, of course, in its creation phase. It was a model of organization in it's first pulse when they had infinite resources and infinite imagination. I had the good fortune to be in about 5 such organizations. I was at Rand when it was that way? I was here when it was that way, GSIA, when it was that way. All of them had this explosive creative energy and freedom, because there aren't any constraints. And that was GSIA at that time. And in fact, the big things that GSIA was doing weren't connected with this at all. They were connected with a new model of how to do business education and how to agree that there ought to be science in business, and Herb was a major participant. This had gone on just as a little funny thing in the middle of all that.

NORBERG: Let me take you back and ask a couple of more questions that are puzzling me now that you have gone through that. One of them is, what was the value of heuristic programming, if any, to complex information processing?

NEWELL: It did the same thing. Heuristic programming is the science of AI - whatever you want to call it - applied to management science problems in analogy to dynamic programming and linear programming.

NORBERG: So to jump ahead just one little bit here. That is a program within IPTO in 1962. Heuristic programming is an area that they wish to investigate, Licklider wishes to investigate, anyway. And what I am trying to understand is whether that's the same thing that you're talking about.

NEWELL: Yes, but... Yes and no. The yes is that that's where that term came from. You see, we were there very early. We were there not in terms of the psychology, but in terms of applications of AI very early on the management side. Heuristic programming was the nickname, was the moniker for that. That got known all around, so heuristic programming was an alternative name to artificial intelligence. You could talk about applying heuristic programs. So that's where it came from in Lick's program. I am almost sure. Now, he didn't have it tied anything like so closely to management science. So in that respect that was my yes and no.

NORBERG: My second question is, did you people, you and Herb, begin to notice in the late 1950s applications of the work that you and he had done in GPS, Logic theorist, and so on, in other people's work? Did you notice anybody using IPL, for example?

NEWELL: Yes, now, let's see. That's a funny question to ask. The answer is, of course. The answer is, I believe that we were aware of absolutely everything that was going on in the field. One of the thing that happens in new fields (and I have, again, had the good fortune of being in several new fields that had this property) is that the guys who are there first know everything. So, for instance, in the computer field, when I was first in the computer field, everybody knew every computer that existed. They knew everybody's order code. Then pretty soon you didn't quite know everybody's order code, but you still knew every computer. And so in the area of artificial intelligence, I knew every program that had been produced by everybody in whatever field. There was not a strong emphasis on fields here. I just knew everything that was done, and that comes from kind of being there first, and then it all happens naturally. So the answer is. For instance, we dug up various things like this one that Herb still loves to quote, this guy, Goodwin, out of Westinghouse doing electric motors, who couldn't give a rusty about us, didn't know anything about us, but was doing some rather interesting design of electric motors, doing some sort of generate with a bunch of programs. So there was an instance of design of electric systems being done by artificial intelligence by these techniques. We grabbed a hold of that very early and that showed up in our papers and so forth as a sort of survey of what was going on. So we actually... So my belief is I knew everything that was going on. But that was...

NORBERG: I don't doubt that.

NEWELL: No, no, but when you asked that question... So this is really a way to answer that question. Now the issue of evoking from me what all of those things were - there weren't that many of them - would take a while if you want to do it, but I think... For instance, it doesn't happen until later that Gary Powers at MIT starts to use GPS for chemical engineering, kind of find out about that, not instantly when it happens, but six or eight months afterwards. The field has the typical characteristics in which those characters communicate with you.

NORBERG: How did this communication go on?

NEWELL: Mostly by letters and papers. There's a guy in a different field, but he's at MIT so he's sort of wired in. So I think we got a letter and a first paper that he wrote. It might even have been a draft of the paper.

NORBERG: How about among the people working in AI? What sort of communication was going on among them? Remember, I'm still before DARPA now.

NEWELL: Right, you're not much before.

NORBERG: Before 1962.

NEWELL: Well, I guess I would say that most of it... There were hardly any papers. So I would say that most of it was at conferences. This was the period, the sort of wide-open cybernetic conferencing period that occurred right after the war. Not quite. Cybernetics stuff starts in 1948. But there were a whole pack of interdisciplinary conferences of the usual quasi-invitational variety, and very few institutional boundaries, because there was a sort of an unbounded belief that everything was connected to everything else, technically speaking. So this is the kind of view that's expressed at the beginning of any historical period, that everything was connected with everything else. And so it's perfectly reasonable to go to conferences and find physiologists and engineers and physicists and so forth all tied together by kind of a concern with operational mathematics and cybernetics, servo mechanisms and Laplace transforms and the digital computer and information theory and so forth all sort of lumped together. So, for instance, when I was at Princeton in 1949, I went up to hear Shannon at the AAAS meetings. And I didn't meet him. I was just a lowly graduate student. And game theory is another one of those pieces of glue, and I am deeply embedded in game theory, because I went to Princeton for game theory, so it all sort of looks like one big mish-mash. But I think its conferences. Papers when they get written certainly got shipped around, not in the volume that they do now because of Xerox but they did, they got shipped around. I don't ever remember using the telephone. Too expensive.

NORBERG: Is it your recollection that there were a lot of conferences in the '50s?

NEWELL: Well, I have to go into the 60's. It's sort of my recollection, but I could be wrong. But the '60s and the late '50s blur a little bit. But I remember a very strong distinction in which not many people in that whole area understood the computer and so there was a little piece of preciousness on our part all along, which I used to make a big point of in terms of going and looking at the information theory books and observing that not one of those characters understood the makings of a computer. A French guy by the name of Heilo(?), or some dam thing, in which it is quite clear that it's just an example of a complex system. So understand he's full of information theory and he's full of cybernetics and feedback mechanisms and he absolutely understands nothing at all about the computer. And so in that respect this was not a community all of whom totally understood each other, but there were so many different things going on that that was not a big deal nor was there any particular intolerance. This was a period of self-organizing systems that comes actually a little bit later in the early '60s. So I would sort of say conferences.

NORBERG: You see the implication of my question, no doubt, in that DARPA seems to feel that they stimulated a good amount of interaction among people, particularly, through conferences and meetings; meetings like the principal investigators meetings that they use to run.

NEWELL: But that was much later.

NORBERG: Well, in '65. And if you say you recall many conferences before 1960, say, then where do they get off saying later on that they really produced this interaction.

NEWELL: Well, partly because ...you know... they didn't produce the Macy conferences. Famous set of conferences...48 to 50 some odd, 53-54. So my resolution of that would be that they were focused on the field of artificial intelligence. That doesn't exist kind of before the late '50s. What you get is a much more general post-war cybernetic, operational mathematical, information theory world, which we all participated in without distinction and everybody was sort of interested in everybody else's stuff. That was the flavor of things.

NORBERG: And then toward the end of the '50s that becomes to dissipate, people tend to go there own way.

NEWELL: Yes. In fact, there isn't any artificial intelligence before then.

NORBERG: Before '56.

NEWELL: Well, there really isn't much artificial intelligence in '56. There's only talk about it. I mean there's just a couple of things. There's a checker program and there's LT. By the time you pick up the other chess program you're at 58. So there is one more program, one more program, that kind of thing. Let's see, the Los Alamos stuff with Ulam was concerned with... must have been 1960, something like that, so they kind of come along one by one. So, from ARPA's point of view, it certainly pulled the symbolic computer oriented folk together, which were the AI folk. Now, I don't see '62 as a thing, partly because money was easy and therefore...

NORBERG: Now let me take you back. One more question back in the early period. You mentioned that the Simon money from Carnegie was at about Forty Thousand.

NEWELL: That's what I remember.

NORBERG: What other funding was available in the late '50s here at Carnegie? And we can stick to computing, if you like, we don't need to go into the other areas.

NEWELL: There wasn't any funding for computing.

NORBERG: There must have been a machine available.

NEWELL: Oh yes, but this is the era in which all universities, in a ripple effect, are establishing computation centers. And where does the money come from for that? It comes out of the universities and out of the IBM gifts and things like this. So, these are all internal sources. There isn't any external...that's not true...the external source is IBM...giving all these machines away.

NORBERG: What was available here? What was the computing center like at this time? Late '50s. Pick any date in the late '50s.

NEWELL: The center consisted of a 650 sitting down in the basement over there, run by Al Perlis. GSIA. Computing comes to CMU, a highly technical college, and it comes to GSIA. They are the people who embraced it and it gets put in the basement of GSAI. I'm sure for odd reasons, but mostly because the characters at the other end of campus didn't have the good sense to decide that they wanted the computer down in their environment. They just didn't much care. Well, I'm sure it was that that was where space was available. So, it's just available as a computation center. Go down and use it. The GSAI is a rich place. It's got an endowment of 6 million bucks, then. That was the original endowment from Mellon. That only started in '49. So that's not so far . The Ford Foundation is getting interested in business schools and is in fact providing all kinds of funds. Although, as Herb remarked to me, last weekend I think it was, I mentioned this out of entirely different context, and he said, "Not enough. They didn't give us anything like the money they should have given us." He said, "Do you realize that they gave Harvard as much money as they gave us?" Harvard, you know, the place that wasn't doing anything interesting at the time. Anyway, there was Ford money around so, one just did things. I don't ever remember in this period, ever asking the question of was there funds to do anything. Now, some of this comes out of my Rand experience, because at Rand you just did things. And I came back here with this same attitude. The issue of how we were going to float this huge permanent organization that was going to run these big organizational experiments never occurred to me that that was going to be any problem. Probably it was going to be a hell of a big problem, but we never got there. You just went and you did things. And that was, of course, part of the whole ethos of the '50s and '60s with respect to computing, again because these institutions were rich. The computing stuff was all done at Rand. In the late '50s, I actually had a model 28 Teletype, the early model teletype, in my apartment. I used to communicate with Cliff Shaw. He was out at Rand; I was back here eight hours a day as we de-bugged runs and designed runs and so forth.

NORBERG: Who paid for the telephone, Rand?

NEWELL: Rand. Cost $800 a month. And it never occurred to me to worry about it. I mean, we made some jokes about it. It was god awful, terrible clanking. Everyone around the area thought we were the bookie place. They really did. You see, we had this little upstairs apartment up here on the hill and thing clanked away. But Rand was rich. I don't have the foggiest notion how rich, but it was fundamentally a rich place. When I went into Rand it was very rich, meaning, it had this open expansion and so that was the ethos and that didn't die. Until several years later, we were still in that phase. You just went off and did anything, anything you wanted at however you thought was the right scale. And nobody worried about being - there was very little empire building. I'm sure there actually was, but you didn't look at people as somehow trying to build empires. You just went off and did what was interesting. The same was true with GSIA. So, funding was just a zero issue and un-constraining. The constraints all came from... You didn't have the wit to decide to do something stupid like build your own computer...do something stupid like that....which would required all of sudden getting involved in what really were constraints all over the place. The constraints were not there in the '60s. I have a big history of sitting on NIH computer study sections which doubled the facilities. The way I get wired into the biological sciences is that I sat on a computer study section for about six or seven years, concerned with promoting computers in the sciences.

NORBERG: When was that?

NEWELL: I has to be in the '60s. Early '60s in fact. I get tapped because of... It's kind of hard to understand why I got tapped. You know a little bit about the history of the NIH study sections? Anyway, this was one of the few study sections which had a mandate to promote. In fact, general studies sections are not allowed to promote. This one was given big chunks of money to promote computers in the life sciences and they went out and promoted proposals and operated it. We used to site visit four or five places every quarter. I have no idea how I folded this into my life. Absolutely no idea. We'd go take a couple of days and really be consultants for these places, little subsets that were on the study sections. Big proactive study section. I was telling you that for a reason.

NORBERG: Well, we're talking about money here [at Carnegie] and what sort of funds were available in this organization.

NEWELL: So I sat on an ad hoc committee, a little later, and delivered to Wes Clark and Bill Papian, who wanted to move towards the life sciences at MIT, 30 million dollars to start an effort to go do this.

NORBERG: That's a lot of money.

NEWELL: We just sat around in this committee...

NORBERG: This was thirty million from NIH? For one organization?

NEWELL: A new organization that was going to be created.

NORBERG: How much of a budget, annually, did they have?

NEWELL: Oh, much bigger than that. NIH has this huge budget. Much bigger. They're still... They were well into this experimental growth. Thirty million dollars. A lot of money. A huge amount of money. But the issue was, this was going to produce new computers for the life sciences. This was all in the wake of the LINC. Remember the LINC computer? This was after the LINC as a success story, which had been nurtured within Lincoln and MIT and Walter Rosenblith, who you probably know, Walter and Wes Clark and Bill Papian, Wes and Bill both being at Lincoln then, and Walter at MIT, Lick was the chairman...how could he be the chairman of this committee? He was at MIT...but son of a bitch he was....the chairman of this committee. You know, a typical NIH committee put together from colleagues in the field. So I must be wrong on that. I watched this and I participated in this organization delivering this chunk of money to this set of guys. Then, of course, they fucked it up. They blew it because they couldn't get along with each other organizationally and in two or three years it went down the tubes, totally and completely. I've been furious about it ever since.

NORBERG: Well, one loss out of a half dozen maybe, is not something to cry over, but I'm thinking about the amount of money here and I'm astonished by this.

NEWELL: It was a really big deal. It was done by an ad hoc committee and not by the study section. And it was late, late in the '60s.

NORBERG: It was about '65.

NEWELL: Then it was before '65, because he went down there in the fall-out of the failure of this organization and then George Pake was smart enough to pick him up. Even I was astounded to sit here and watch this community for what it believed, and I think were right, you know, and do whatever it takes to deliver the things that are going to make a difference. A whole committee delivered this to a brand new organization built on promise.

NORBERG: This undermines the DARPA story as far as I'm concerned, because DARPA gets on the scene with 9 million dollars.....

NEWELL: No, this is part of the DARPA story. The only reason that happened, but who do you think are the characters that are floating around in it. The same ones, the Lickliders, the Wes Clarks, the Bill Papians, the guys that at Lincoln Laboratory are the generators of this. I can only tell you what it was like from my side. I'll bet I even said that a number of times. This is the ARPA style of doing things. You go out and do whatever it takes, at whatever the level of imagination you can hack. So, I don't think it undermines the DARPA story. I think its an instance of technology transfer at the bureaucratic level.

NORBERG: I can see that, but the budget that you are talking about is so much larger than DARPA's, for many, many years.

NEWELL: In '65?

NORBERG: Yes, 18 max in '65. Maybe even less than that.

NEWELL: For IPTO? Now, let's see, when did we get 1.8?

NORBERG: '62.

NEWELL: No, No, we got less than that. We did not get a million. We did not get a million in '62. We only got 300,000 in '62.

NORBERG: Well, isn't that part of a 1 point something million grant for multiple use. You get the $300,000 first.

NEWELL: One of things I don't know... I violated my own principle. My own principle is never to state bucks without stating the number of years.

NORBERG: You're right.

NEWELL: Because it makes it a lot of difference. I believe in fact that was a five year award.

NORBERG: Then let's get on. If there's no problem with money, here, why did you go to IPTO in the first place?

NEWELL: We didn't go to IPTO; IPTO came to us. The story as I remember it, is Licklider, whom I didn't even know, goes down to IPTO.

NORBERG: But you said he was on this committee with you at NIH.

NEWELL: Oh, that was a couple years later.

NORBERG: So that was after he left DARPA. When he was at IBM, maybe?

NEWELL: He might have, he might have been at IBM. He certainly wasn't at MIT. I would hope not. NIH was honest on those dimensions.

NORBERG: I'm sure they were. Okay, so Licklider comes here to see you people.

NEWELL: One of those things you are impressing on me, of course, is that all this happened in not so very many years.

NORBERG: Yes, that's right.

NEWELL: I think of this event as late in the game, but hell, if Wes Clark goes down to Washington, that's after this thing blows up. And it didn't last very long. It only lasted probably about a year. So that means '64 and '64 is only two years after this other stuff. So things are moving very rapidly.

NORBERG: I will look into the story of NIH a little bit more carefully. I haven't done so so far, because I didn't think it was of this sort of magnitude.

NEWELL: Well, there was this computer study section, which must have started in '60 or '61.

NORBERG: That I can easily believe.

NEWELL: I end up on it kind of from day one. Bruce Waxman... You know Bruce...

NORBERG: Yes, I know the name.

NEWELL: Bruce was the original study section guy. The guy who created it, a fairly entrepreneurial guy.

NORBERG: So, we're back to Licklider coming here to see you people, somewhere in '62 or '63.

NEWELL: No, he never came here. Here's the way I reconstruct this. You'll see why I say that. There is thing called DARPA. I've never heard of it. So we're back in '61. Something like that. This is before IPTO. I don't remember when IPTO started.

NORBERG: '62.

NEWELL: '62. Licklider went down to ARPA. I didn't know that. I knew that only from reading subsequent histories. With a set of attitudes, which are standards of the time, about finding the right people and places and supporting them. He knew Burt Green. He knows Burt Green well. Burt is a psychologist. A psychometrician actually. A psychometric fellow from Princeton, who goes - (I don't know what he did right after he got his PHD - he ends up as the head of - they didn't call it psychology) - psychology in the Lincoln Laboratory. Burt built an IPL program called BASEBALL, which you will find...

NORBERG: I know that.

NEWELL: And there's a connection that doesn't really exist: actually Chomsky's wife was one of the members of his team. Anyway, so I get to know Burt, because he's working on BASEBALL, I went up and spend some time with him at Lincoln Labs on a couple of occasions. Oliver was up there as well, but I get to know Burt independently because Burt is building this BASEBALL program. So he gets all involved in this aspect. We decide we have to change the psychology department.

NORBERG: Here [Carnegie].

NEWELL: Yes, in fact, there is a piece of this in Herb's autobiography.

NORBERG: I have it.

NEWELL: But I will assume that relative to this stuff that is one of the sources. Anyway, the guy we get for head of the department is Burt Green. We get Burt Green because he is an information processing psychologist and Herb has just gone and torn the psychology department apart in order to say this is not going to be a department where nothing interesting can happen along the directions that I now think are important. And so he displaces the leader. He is not about to be head of the department and that whole story told in his book, but I don't think he mentioned his name. Burt is the obvious choice. Bright guy. Super-bright. Doing things in artificial intelligence and natural language and so forth, his BASEBALL program. So he comes down as the head of the department. Now, Lick knows

Burt real well. In fact Lick probably was responsible for getting him up to Lincoln Lab. I have no idea. So, Lick floats in. I knew nothing about this. He looked around for where he wanted to put money besides MIT. I guess he knew about LT and he knew about Herb and so forth, so he had this little piece of year-end money when he arrived. It's $300,000; he's got to spend it in six weeks. It's the standard kind of story. He writes a letter to Burt, saying "write me a letter and I'll give you this $300,000 and we'll get started." That's the start.

NORBERG: To do what?

NEWELL: Research in Information Processing. No proposal needed; just a letter. I mean the letter says some things about research in information processing.

NORBERG: So that came to the psychology department.

NEWELL: No, no it came to the... Who did it come to?

NORBERG: Licklider was also head of the Behavioral Sciences Program at the time.

NEWELL: Did that start that early?

NORBERG: Yes. He headed both offices when he came, and he funded some of the AI stuff out of Behavioral Sciences, it turns out.

NEWELL: For all I know, he could have done so. I never knew that. I didn't even know of the existence of it. No, it didn't come to the psychology department, in fact, the people who got a hold of it were Al Perlis and myself, because it was computer science oriented... But the channel was through Burt. Now, there was an interdisciplinary program here called Systems and Communications Sciences. The people who made that up were Al Perlis, Herb Simon, Burt Green, myself, and a bunch of other characters. This was a real interdisciplinary program. That is, it had it's own graduate students, probably had only about 15. They all lived in the computation center, so they had their own space in the computation center. So the guys didn't live in their separate departments; they all lived down there. It got fueled by the DARPA funds, independently of anything else. So the funds went to this program. Of course, they didn't go to this program. You couldn't do that. So I don't know how they got into Perlis' center, except he was the obvious guy and he became the PI. He and I became the PIs. There was no "get me a proposal;" there was no Lick "showed up" and we had some discussions. It just happened, so it was pushed. Once again, it reminds me of the typical DARPA style, which Lick brought with him.

TAPE 2/SIDE 1

NORBERG: You had the money along with Green and Perlis. What did you people decide to do with it?

NEWELL: I don't know... support people... just spend it. We didn't decide to do anything with it. One of the features of this environment was that it was decidedly un-entrepreneurial. That seems, in one respect, like a contradiction in terms, but we never took these funds and decided we were going to go out and do big things with these funds. That was an attitude typified the operation around here up until Raj [Reddy] shows up.

NORBERG: Maybe that's true, but I guess I'm looking for some specific examples of how you did spend the money.

NEWELL: Paid salaries. I haven't the foggiest notion.

NORBERG: I see.

NEWELL: I mean, *look*, we're fully engaged in research. We're doing just what we want to do. We have graduate students to support. I'm sure there's some stuff at the computation [center] to support. We didn't get a better machine until somewhat later. So, it just provides support for those things. This is an operation, the Systems and Communications things, that had no other money, was independent of anything else and now all of a sudden it's got a budget. And it goes to support that. This was a little later, and I'd have to go back and think hard, but very funny relationships grow up between ourselves and DARPA on this. The symbol of this is that we had an agreement with DARPA in the '60s, and I can't remember just when, that we were located in the computation center. Therefore, there was a computation center budget. People from all over the school were using it, and so forth. It looked like it was a big mess to try to decide which were the pencils were being bought for DARPA. So we agreed to take the total computation science budget and cut it 55%, 45%, no questions asked. DARPA would pay 55%; the school would pay 45%. That's how we spent some of it. No questions asked. It was done because everyone believed that you ought to minimize the accounting. So there wouldn't be an accounting problem. Every member of the computer science faculty, and before that, the system was supported by DARPA automatically. There never was an AI lab. There just was the total faculty. So the theorists, the linguists, whoever was here - I mean not totally supported, there was the GO [general operations] [?] budget, but that budget was very small, because, in fact, the school never gave us a dime fundamentally. If you understand politics of such things... Why would they be induced to give us any real money when they created the department? You see, we didn't have any money at all up until '65 when it became a department. The answer, of course, is that we had lots of money. So what was fueling this? Well, the ARPA funds were fueling it and there also was NIMH program grant that was located sort of up in psychology, except Herb and I ran that one. And so that actually helped to fuel things up. But there were no boundaries that got worried about. But, if you ask me for an accounting, a precise accounting, or kind of a functional accounting, I have no idea what that money bought. But I do know that we never asked the question about how to go and do something new with that money, except to do what came naturally, and we were very un-entrepreneurial about it. I have an illustration of that for you. 1965, we got this 5 million dollar grant from the Mellon group, which was two things. It was finally advertised as a present upon Guy Stever's coming up here and becoming president. It actually started out being something quite different, but due to internal politics it was converted to be this thing. There was in that Herb's chair, a million bucks to buy a computer, 1 million bucks to buy a building for computer science. We always used to brag that we the one place that actually paid our share of this building; no one else paid any money into this building. We did. We paid that 1 million bucks and 1.25 million bucks for 5 years of research money, an internal foundation. We looked around. We said that we've got so much money, we don't need that money. So we'll set up a little committee, but no one inside computer science will be able to use that money. We did it for the rest of the university. People from inside computer science could submit proposals, but then we could probably find some other way to fund these. And that's what we did, up until very late in the game. So we just decided - I won't call it the goodness of our heart, out of an attitude of what was important about money, as opposed to believing that we ought to use 250K a year, to go do some interesting things - we just proceeded on our own good way and said "Let's help to build up 'Information Processing' around the rest of this school. I know that's all true, because I'm the guy that decided that; I'm the guy that ran the committee. After awhile I ran it as a one person committee. The big point about this is that this is an illustration of an attitude, of a really strong attitude - I think now I'd go back and do it quite differently - but a real strong attitude toward how we viewed it. Starting in '69, when Raj came here, and when Gordon Bell came here, even a little earlier, that whole thing changed and we started to do some things like CMMP and so forth. The thing is very un-project oriented all the way through this. Al Perlis didn't believe in projects.

NORBERG: That's an interesting comment.

NEWELL: I didn't believe in projects. Didn't have projects. All you had were people doing science. Not little fiefdoms. It was all community. All because there weren't any financial constraints on people. All the students were funded out of a common pot, so the students just worked with whomever they wanted. I don't know how much you've heard about the mythology of the computer science department.

NORBERG: Not any.

NEWELL: There was a lot of mythology about being one of the very cooperative schools; we ran everything jointly; people are all treated alike. There are no boundaries; there are no laboratories in this department. All of this comes out of the ARPA tradition, as it was evidenced in the school here and out of this communication sciences program, this interdisciplinary program that preceded the computer science, which was genuinely interdisciplinary without the kind of constraints that force all human beings into living the way you live in Minnesota, the way everybody lives everywhere. So, we ended up with a place where graduate students picked people to work with without concern for where they get funded, totally and completely. In the early days, everyone was funded not matter who you were.

There were no field differences. We've always had psychology on our machines. We just let all kinds of people on the machines, anyone who was a friend is the wrong word, a technical friend. Anyone for whom there was a belief by people in the system that someone else in the university ought to be on our machines and share our resources, we would simply add without question. Those did in fact come into conflict. They always do. But not very much. And not very much in the '60s. But we didn't do very much in the '60s.

NORBERG: Tell me a little about Al Perlis and his style of running things, because I have the sense that Perlis was different than that.

NEWELL: Nope. Perlis was one of the world's great non-administrators. He did not believe in administration. Perlis believed that you solve problems by making some simple decisions. That seems completely at variance with the fact that he was simultaneously the head of the math department, the head of the computer science department and head of the computation center. But if you think about it, that is the only way you can run three administrative units is believe that administration does not count. Al Perlis and I ran this place together without, essentially without ever communicating with each other, because we always understood and believed in what the other was doing.

NORBERG: What sorts of thing was he doing other than running these three organizations?

NEWELL: He was creating computer science.

NORBERG: What does that phrase mean?

NEWELL: It turns out that Al Perlis' claim to fame... His technical claim to fame is fairly modest; he actually did a couple of things, in terms of algebraic compilers for small machines. And he did a few other things. But fundamentally, it turns out that Allen Perlis epitomized, literally epitomized for the technical people in the field the nature of computer science. People would go to conferences - by people, I mean people like myself, people in the field - people would go to conferences where Al Perlis was and in essence listen to every word he said, because every time he talked about a topic, which was full of these bonmots and epigrams and stuff like that, he was absolutely right about the way it should be. That's why he's the first Turing Award winner. Not *at all* for any technical contribution, some of course. But not really, because in one sense he understood what computer science was supposed to be and he laid it out, not in a bunch of writings, in a sort of epi-grammatic way by personal interactions with all of the significant people in the field and they all believed him. They fundamentally believed him. So in one sense what he was doing was being a guru. What he was also doing was directing a bunch of graduate students, running research projects. He was running these organizations or non-running them. I think mostly the mathematics department was absolutely non-run during those years, but I don't know. Decisions did get made. He would not spend very much time on administration, so he would make the decisions rapidly. We never fought so in one sense we just understood that things were okay. I'm sure, in fact, we did talk a little bit. But fundamentally, we never had to sit down and have big sessions about the way things should be. Partly, because I believed his vision, and he believed mine in some respects. There are some interesting things in the literature where he comments about my being the only guy who he has ever known who he has always agreed with. Every time I would do something, I would find out that that was in fact okay. So there was essentially no friction in the organization. That's the other reputation that we have.

NORBERG: As long as computer science is an area in which research problems define what people do and how they come together, then things can be run relatively smoothly.

NEWELL: I think that that's an interesting remark.

NORBERG: Between the two of you in respect to the major administration of this university.

NEWELL: That's right. Remember the major administration isn't providing any bucks for us. And it isn't providing much space for us, because the space negotiations are really computation center negotiations.

NORBERG: Okay. When do the facilities begin to change?

NEWELL: There is a shift from the 650 to the G20, that occurred in 1960, I believe, and this moved us from that end of campus down to the top floor of the Engineering Building over here [points]. We took over the top floor. By now there was really a fair sized organization. All these graduate students and everything else that went on. Then there is in 1965 the decision to get the IBM 360/65. Maybe it was 1964 when we got it.

NORBERG: No that's too early. It would have to be 1965. The model was only announced in April 1964.

NEWELL: We got it very early. There is a huge decision process about whether to get that or to get the CDC 6600, or whether if we couldn't afford the 6600 to get the 6400 which was just being talked about.

NORBERG: Does that imply that it was not a decision based on trying to get a time-sharing system?

NEWELL: No. The decision to go with the 65 was fundamentally a decision that we couldn't bring up a time-sharing system that we could live with. We had a lot of talk about how to do this on the 6400. In fact, you looked at those things entirely in terms of what you thought you could do with the machine. So it wasn't a question of buying.....not that the 65 wasn't a major attempt to build a time-sharing system. The thing that was not in the picture, because it was a little early, is the DEC machines. So that keeps us out of the growing ARPA community defined by Stanford and MIT, which had the PDP-6s and then the PDP-10s. I don't remember when the PDP-10 came out...

NORBERG: It was around that time ....

NEWELL: ... but it was a little after that. The PDP-6s were around earlier than that. You have to remember that we're buying a machine for the university. This machine was to be used by the whole university, not just by us. We're not a separate place. This 55, 45 split and all the rest of this was totally identified. Of course, there's lots of talk....and that actually keeps us from integrating with MIT and Stanford folks, quite a bit. That all changes, really when Raj comes in, because Raj comes from Stanford. It happens in the classic fashion, absolutely classic fashion, in which, you finally have to get a guy from the other environment, bring him in, and then, all kinds of things change. None of those characters were interested in management science. None of those characters were interested in psychology. None of those characters were interested in programming, which was Al Perlis and running basic machines and so forth. They were interested in time-sharing.

NORBERG: Who's the they that you are talking about.

NEWELL: MIT and Stanford. Actually there was more to the DARPA community than those guys - as you now know probably better than most everyone else - it was always the case that... In particular, there was the System Development Corporation. Licklider attempts to construct a time-sharing system that involved supporting Project MAC, and supporting the time-sharing system there, CTSS, and a time-sharing system at SDC, and supporting us here, which was a much more modest effort in terms of his ambitions, because we actually went up to remote entry procedures and whatever. My model is nobody cared. Of course, they cared, but none of the aspects of the ARPA community that you now see in which everything is driven, okay, none of that existed.

NORBERG: Everything is driven by what?

NEWELL: Everything is driven by the DARPA office. That is, when they created a vision program, the people in that vision meet every six months. When they created a speech program, they sent out tapes every 6 months or maybe it was every 4 months, but everybody did those tapes and they all got graded and scored. It's driven research. It makes progress, but it's driven research, and it's community research. So it's the issue that says that we form a little vision community, an ARPA vision community, not a bunch of ARPA vision investigators. This is now. This is now for ....

NORBERG: It's got to be 10 years or more, I think.

NEWELL: I tell you privately that it starts with the speech program.

NORBERG: Oh, back in 1970? That's early.

NEWELL: Well, it starts ....

NORBERG: Things do begin to change in the middle seventies. I know that.

NEWELL: There's an interesting story there. I'm a little worried here. In some respects, I'm telling a lot of this history from a fairly personal point of view.

NORBERG: That's fine. That's what an interview is all about. Let me go back to one other thing here that I'm still not clear about. When does the Carnegie Mellon situation, CIT situation, if you like, turn out to be time-shared.

NEWELL: '65. When the IBM time-sharing system comes in, and whenever you want to estimate that it finally gets around to doing time-sharing.

NORBERG: Then how was the system run before that, was it all done on batch processing?

NEWELL: Oh no. No, no. We had remote job entry on the G20, which was again our working towards time-sharing. It was a form of time sharing where people could do their editing on line and submit their jobs and get there things back. So it was sort of batch run, but interactive, editing called RJE, Remote Job Entry. We actually pushed that very strongly and that's what I meant when I said we were kind of conservative and so forth compared with some of these others. In fact, you don't get good time-sharing out of things like the PDP-6 and so forth for quite a while. CTSS, of course, was not on a PDP machine. The damn system out at SDC never worked.

NORBERG: You mean the Q-32? Why didn't it work like a damn?

NEWELL: It's unclear to me, except it has always been hard for the System Development Corporation to crank itself up to doing excellent things.

NORBERG: Well, that may be true, but given their task, that's not surprising, given the kind of work that was laid out for them. On the other the Q-32 was connected to a significant number of organizations on the West Coast to provide what Licklider at one point called the California Network. It sort of worked.

NEWELL: I didn't know that anyone had Q-32s out there except....

NORBERG: There's only one at SDC, and Berkeley and Stanford and UCLA were all connected to it by data phone connection. It was not the network situation we have today. It was like the remote job entry you were talking about. That's the way it was run and that was the way these other places got into this sort of connection with each other; it was pushed by Licklider; he insisted it be done that way.

NEWELL: Oh sure...Lick....Lick...I have to take something back in one sense, but I don't want to take it back in another. The thing I have to take back is ARPA pushed people all over the place, but never with the kind of consistent and ferocious style that it has adopted in the last 15 years. For instance, the really important part of that story is, if you look at the history -- which you already know all about -- if you look at the history of the ARPANET, Larry Roberts was totally permissive with respect to the research community and whether they got on that and used it. This was a major frustration. He said, "Here's this great thing" -- and Larry of all people, who was one of visionaries who really understood the potential of the ARPANET and "It's all free and I'm hanging it out in front of you guys and you guys sort of say, 'Don't bother me; I've got my own things,'" and he never did anything to make that happen. He went off and did some other things. He went off and created the Center for... I can't recall the one the Slotnik ran at the University of Illinois. Those guys had to live on the network. That is, he said, "I will not provide you with computing at home; you must use computing over the network," and they suffered from that. So he was perfectly capable of putting the cranker, but he would go out to the other guys who were otherwise gainfully employed and arm twist. Now it would be a lot worse. There would just be general programs of much more coercive character. Again, that's separate from the issue of whether that's good or bad. Very different styles. So, we got no technical direction from DARPA in the '60s.

NORBERG: Throughout the '60s.

NEWELL: Throughout the '60s. I don't know. I'm sure we did. I'm sure there were conversations between Al and some character down there and these were technical conversations between like-minded folk. And yes, I'm sure we were influenced by a whole bunch of things, but we went our own way and we did our own thing. There was no attempt to influence this decision to get the IBM at all. In terms of the research, I don't mean improper, just in terms of what was your research program and how are you guys gonna do this, this, and this in the future.

NORBERG: When is it that you remember having first had to write a proposal for DARPA?

NEWELL: I believe probably 1962 or '63.

NORBERG: '62 or '63, but I thought that was when you just said. Licklider sent Green this letter.

NEWELL: We got the money in '62. The 300K arrived. That was a little money to get started, but the next time around we had to write a proposal.

NORBERG: Okay, so that was roughly a year later.

NEWELL: Yes, roughly a year later. Those proposals were -- I think he wrote them all; I don't think that Al wrote them -- were sort of non things. They weren't letters, but they were not... Then in 1964 I believe, Licklider had this idea for making centers of excellence. Then we wrote a real proposal. I don't know if you've seen it.

NORBERG: I haven't. It's opaque to me until '72. The first Carnegie proposal I have is the outlined proposal you sent over the network in '72 which Licklider responded to.

NEWELL: I wrote this great proposal, I'm sure I can find it for you. This great proposal written in '64 for why we should be a Center of Excellence.

NORBERG: I'd love to see it. It would really be a very important document.

NEWELL: That's when we went on -- I think that's when, I just don't remember the conditions earlier -- what I consider to be the greatest of all funding arrangements that I ever heard of: a three-year funding arrangement in which each year we got one more year out at the front of it. The most important part of that is that you can't say very much of what you're going to do 3 years out, so this is really just an agreement to extend it, until the time of the Vietnam war crashed in, which wasn't really so very many years, we had this rolling 3-year funding,....which was not only stable but required essentially no details, because you were always talking about what was true three years from now, the new out year. In fact, the project... begins with T....a famous engineering project for engineering research that was cranked up by DARPA...

NORBERG: Traces.

NEWELL: No, not Traces. It had a scheme that was built on that idea -- I don't know whether it was taken from it -- which said that we would always have a 100% next year and 60% the following year and 40% the following year. When you moved forward a year, it was always this way.

NORBERG: Project Themis?

NEWELL: No, not Themis.

NORBERG: Well, then I can't remember.

NEWELL: Anyway, it had this same kind of attempt to get stable funding for people by providing for them, but 60, 40, not full funding. That I believe, although I can't remember, that I believe came along with this Center of Excellence thing, but I just can't remember what was true before.

NORBERG: Let's go back and explore another topic. I'm interested in a paper you published in 1963 on Learning.

NEWELL: Oh right. No one ever attended to that paper.

NORBERG: But yet it's referred to in the literature as a seminal paper.

NEWELL: Bullshit. Oh, you're talking about the one in '62?

NORBERG: Well, I have a 1963 date.

NEWELL: So now we need to decide what paper we're talking about.

NORBERG: Right. True.

Newell: "Variety of Intelligent Learning?" "General Problem Solving and Learning?"

NORBERG: Oh, you have me now. Learning Generality and Problem Solving. 1962.

NEWELL: You will have to give me documentation that anyone ever read that.

NORBERG: Well, I can't demonstrate *that*.

NEWELL: If you think someone said it was a seminal paper... My view on that one, as opposed to another one called "A Variety of Intelligent Learning and the General Problem Solver," which was published in 1962 from one of these self organizing conferences, which was indeed a seminal paper in the sense that everyone talked about it and talked in terms of it, which did not have a running program associated it. It was a theoretical paper built on top of GPS. It was a scheme for how GPS would learn its defferences. This paper 'Learning Generality and Problem Solving' is essentially an invited paper at an IFIP's Congress and my belief is, I should say, that nobody every paid any attention to it, and I'm really intrigued if you thing differently.

NORBERG: Well, let me turn this off for a minute while we negotiate.

[INTERRUPTION]

NORBERG: I was introducing that in an effort to get you to talk about the research that you were doing in this period just before you got interested in DARPA funding and then what happens after you people get the 300K.

NEWELL: Now be careful, because what I told you so far is that I never got interested in DARPA funding.

NORBERG: Well, that's correct and what I'm beginning to interpret from those remarks is that in fact as the money comes in the research program doesn't change anyway. You're still doing the same things.

NEWELL: I'm sure it expands, and the way it expands is there are more graduate students and these graduate students get supported and then there are more faculty and DARPA is supporting all the faculty that was associated with Computer Science -- up to some fraction -- but it is supporting people who I'm not doing joint research with at all. So there is no project character to it.

NORBERG: Then, let me ask the question differently. Rather than focusing on your research, let's ask, who else was here? So far you've only talked about 3 people, 4 people with Green, yourself, Perlis, Simon and Green. Who else was here now in the '60s, who would have been funded in some way on DARPA funds?

NEWELL: There was a guy here by the name of David Cooper, programming type, Englishman. Although, actually he had built the first LISP processing system over in England at the University of London, on a one-thousand word machine, he came over here for several years in a professorship. There was a guy by the name of Bill Watts, a computational linguist. Who else?

NORBERG: Well, Feigenbaum must have been here somewhere around that time.

NEWELL: He was a graduate student.

NORBERG: Was Waterman here then?

NEWELL: No, Waterman got his degree in 1969 and showed up here in '70. Gordon didn't show up until 1968, and doesn't make his presence felt for about a year or a year or two after that.

TAPE 2/SIDE 2

NORBERG: The conclusion I'm drawing is that there wasn't a very significant group.

NEWELL: In your terms, it was not a significant group. It was not AI. We didn't hire into AI. We hired into a computer science department. The outcome is that there was no AI lab here. There is just the computer science department, which is over all of computer science, so there are some theoreticians... Actually, Bob Floyd showed up in the mid '60s, now at Stanford. There was a guy that sort of did the first work on assigning meanings to programs, which later, not necessarily in the same form, was used in one of the more mature forms. There was actually a thesis by Jim King on program verification that used those ideas -- AI-ish like program. Bob himself was not at all AI oriented. He was program system oriented. As theoreticians, we had Al Meyer, now a full professor at MIT. We never had good theorists around here, relative to what we had in other areas. We had the start of a young, very good group of theoris ts. We had Al Meyer, Mike Fisher, who's a professor in theoretical computer science at Yale and Jim Standish, now out at Irvine -- was here as a student (these other guys weren't), and Bob Floyd, who was essentially on the theoretical side as well. That group was here in the late '60s and then blew apart in the course of a couple of years. Standish went to Harvard. Mike Fisher went to... So we lost 5 or 6 guys and we dropped to a faculty of around 7. So there really aren't many people by the '70s.

NORBERG: And this was about the time that Perlis decided to leave.

NEWELL: Yes, Perlis decided to leave. Right. In fact, the number 7 in my mind turns out to be who was around when Perlis decided to leave. Then I somehow remember counting the number of characters. You would not believe that Perlis actually made his decision on Christmas Eve.

NORBERG: And why not?

NEWELL: Well, it's kind of the wrong time to communicate this to other people.

NORBERG: Well, that's true. If he communicated it, that's true.

NEWELL: On the afternoon of the 24th. We were on sabbatical at that time. I had gone on a mini sabbatical for about six weeks. We arrived; we rolled into a snow storm... Do you know the West Coast at all? Have you ever been to Stinson Beach? We rolled into Stinson Beach -- it wasn't snowing then, we must have missed it, but there was a heavy rain storm. Found the place we had rented where we were going to stay for the next two months, walked into this house, began to unpack, and the telephone rang. Raj had finally pried out of my secretary what my number was and called me to tell me that Al Perlis had said he was going to Yale.

NORBERG: Now was that the first year that Reddy was here?

NEWELL: No, Reddy was here in '69. So he had been here a couple of years.

NORBERG: Would this be in 1970 when that decision had been made?

NEWELL: '70... '71... It was winter of '70, so the beginning of '71.

NORBERG: So there were not many people around during...

NEWELL: Bill Wolfe was around. So, essentially, the Raj was an associate professor; Bill Wolfe was a young assistant professor; a guy by the name of Zalcstein was around.

NORBERG: How is that spelled?

NEWELL: Z-A-L-C. He kind of dropped out for a while. Then he showed up at NSF as a program manager. I ran into him the other day. Young theoretician, basic automata theory. I think he's moved a fair amount. How many other people? I said there were seven of them. I can't remember... I think David Cooper had left. He went back to England to be a professor at Suffolk. This, of course, was after all the other characters had left and we were already pretty lean because they left in the very late '60s. Gordon Bell must have been around, because he came in 1968. Not a very senior guy. Nico's around [Habermann].

NORBERG: Is it fair to say that what Licklider and his successors were betting on was you and Simon?

NEWELL: Yes. Well, and Burt. And he was wrong about Burt. I mean it turns out that a few years later Burt quit the head of the department and went back to being a psychometrician. He said something like, "I just can't hack it any more." He just actually reverted into being a pure psychometrician and went down to Johns Hopkins University where there was another guy by the name of Torgenson, another psychometrician, who joined him down there, remained very quantitative and got out of the field. But it's not clear, by the way, that Lick ever had, I don't ever remember Lick being here at CMU ever. I'm sure he was, but I don't remember it. So I don't know if Lick had any notion of what was here.

NORBERG: Well, I'm trying to fit this into the concept of making CMU a Center of Excellence, because, in fact, I have seen materials in the DARPA files which talk about making CMU such a center.

NEWELL: But observe that this is not built on AI only. A Center of Excellence is across all of computer science. So Al Perlis is here as well.

NORBERG: Well, okay, but that's only 3 to 4 people that we're talking about in that period. Did you people have plans to expand this, and were they written in that 1964 proposal, if you remember it?

NEWELL: Nope.

NORBERG: They were not.

NEWELL: Now...we could....let me see...so this is a yes and a no kind of thing. The answer is we had plans for doing all kinds of great things. They were not couched in terms of institutional plans for expansion. They were not couched into institutional terms at all, I think we'll find out. The ? really related to was there coupled in a real expansion. And the issue is... you see, there all kinds of funny indicators of this same thing. By the time Gordon got to be fairly active in the early '70s, we already were into the Viet Nam era and the funding changed drastically and we did everything on a shoestring. We bought our machines second hand. We actually built C.MMP, planned it and started down that path by scraping it out of our hide. I can remember Larry Roberts being absolutely furious when he discovered we were doing this, because we never bothered to tell him we were going to go and build this multicomputer. Never occurred to us to ask DARPA whether we ought to build this. It certainly never occurred to us to ask for funds. We just proceeded to go plan it and do it and to get the resources from within ourselves.

NORBERG: Well, the way you described it to me a little while ago was that you didn't need to worry about matching dollar to project.

NEWELL: Well, now you see were talking about what happened in the early '70s when Raj comes on and Gordon comes on and we start to build, start to have projects and we have to shift to having projects. In the '60s we are totally non-project oriented, in the '70s under the impact of first with Raj, then with Gordon and then of Bill Wolfe -- Bill Wolfe, in one sense, doesn't bring this because he is actually a young assistant professor who grows into it -- but Raj brings it and Gordon brings it. We shift towards doing projects and those occupy the early '70s. Then, of course, there are all those problems about how you actually put together the team to go do this and how you scrape together resources to do it. Of course, with the speech program, which is the one that Raj did, there we really did have external resources now. The whole thing was negotiated and it became the speech program. For the multiprocessors, we just went off and did it. It actually grew out of an ARPA activity. There was an ARPA effort, a joint effort -- ARPA kept calling these meetings in which everyone gets together. There was one -- a pretty interesting one -- about how to build an AI machine. They all got together for a few days -- I wasn't there -- Gordon was at it, some graduate students, too, I think were there. I don't remember where it was held.

NORBERG: Do you remember the date?

NEWELL: No, but I can find it. It's got to be 1969 or 1970. You'll see why it's got to be that. Gordon proposed a design of an AI machine, which is a bunch of PDP-10s lashed together. In fact, there are some papers out that give that design by Gordon Bell, and Peter Freeman, who was a graduate student at the time. That's why I think there were some graduate students at the meeting because there were some graduate students that helped with that whole design, but it might have happened after Gordon came back from this meeting. Nothing came of that. About a year later, six months later, nine months later, Gordon sort of looked at the design and said -- I'm sure it was Gordon -- plus Nico, plus Bill Wolfe, because they were the original sort of proposers -- "Mini computers are now cheap enough and this whole design makes sense with mini-computers. So let's go put it together with mini-computers. So it flows out of this ARPA exercise," and it seemed like a good idea and they proceeded to go and do it.

NORBERG: Using PDP-10s?

NEWELL: No. Using PDP-11s. 11s were just coming out and, of course, Gordon knew all about them, being one of the architects. This is an issue about...there were a lot of things impractical about doing this with 10s, but with the 11s it all made sense. And that turned out to be C.MMP. So in one sense the design and design of the idea came out of this community effort in which ARPA got everybody together and says, "What's the right nature for an AI machine?" They had all kinds of different ideas and nothing much came of them, except C.MMP came out of it by the time he completely rescaled it and said, "No, we shouldn't use PDP-10s; we ought to use 11s. They are cheap enough; they are reliable enough." And he probably said, "I can get the 11s cheaper." I don't know. So, it was a kind of a lash-up project. But it did become a real project and then there was this sort of a fracas. We actually were proceeding when ARPA discovered we were doing it. They said, "We didn't give you guys permission to do this." We had notions of what we were doing. "What are you guys running off and doing your thing?" The only answer we had was, "We thought it was a good idea; we're doing our thing and we want to do it. We didn't ask you guys for any money. You can't have any complaints."

NORBERG: It wasn't coming out of the 55%, I take it.

NEWELL: Well, sure, but that was our money.

NORBERG: 55%? I thought you said they were paying 55%. It was coming out of 100% then.....

NEWELL: It was coming out of ARPA money, but it was money ARPA had already given us. It was an attitude coming out. They didn't have any control over the money they'd given us; that's our money now to go do what we want with. We didn't tell people in ARPA in the '60s what we were going to do with their money. We told them in sort of general terms. Once we got the money, we did what we thought was right with it. We didn't go and ask them for additional money to build this machine. If we had asked them for additional money, of course, we should have had a negotiation about whether we should build this machine. But since we took it out of our own hide and found the money in our own ARPA funds, but it was our ARPA funds, then we should be allowed to go and do what we wanted. The fact that it had various long term consequences, that was not of big moment to us.

NORBERG: Sounds to me that the first four or five years of this department are rather unstructured.

NEWELL: Yes. And that's Alan Perlis, again. By the way, the first years until '65, it's not a department.

NORBERG: Yes. I realize that. From '65 on.

NEWELL: In fact, the becoming of a department is a non-event, an absolute non-event. I don't think I even knew that we became a department. I never attended a single meeting to discuss it. Al and I talked about it a couple of times. We agreed it was sort of the right thing to do. Al went off and did it. I'm sure he talked to a bunch of committees, and so forth. There were ways in which that got tied to this grant, because in fact it was presented to the foundation that the reason we had the department was because they had given us the 5 million dollars. It was an internal joke that had nothing to do with it.....

NORBERG: Someone has to approve the awarding of degrees, so...

NEWELL: But we were already......right....so there was certainly that certification that happened.....I just don't ever remember attending a meeting. One of the reasons, of course, is there were no resource questions involved, fundamentally.

NORBERG: What I'm leading toward here is that I've seen some documents that suggest that there was some concern in 1970 of the role of the department in the development of the field. Let alone what DARPA was interested in now. But the role of the department in the development of the field. That's at the time that Perlis left for Yale.

There was a fear that others would leave the department also, obviously, I've read a memorandum...

NEWELL: You've got your timing wrong...

NORBERG: No. I read a memorandum that you wrote to Cyert at the time about the needs of the department. I didn't see his response.

NEWELL: Right. I know about that. I'll tell you all about that.

NORBERG: I only saw your side of it. This was the time that Traub was brought in as chairman...

NEWELL: It's shortly after Joe comes in as chairman. The reason I said you've got your timing wrong is, we had lost five people from the department or something like this.

NORBERG: I didn't know that from reading the document.

NEWELL: So we had taken this huge hit, which had totally destroyed our theory capability, and then we lost Perlis as well. The department, as I recollect, was down to something like seven people total. That doesn't count Herb; it counts me, but not Herb. So we were feeling very lean and Joe Traub came in to try to build that back up. Not that we viewed ourselves as a department on the ropes -- that never occurred to us -- but I'm sure we felt like all kinds of shit had hit the fan. We were worried about our financial stability. Take this back against the era of Viet Nam. The most significant feature of the Viet Nam era, which is the other side of this great support picture, is we were told for three years not to submit any proposal at all. They just said, "You've got all this money; don't submit a proposal. Just live on what you've got." So we chewed up all this fat. This, actually, is related to an event in the Vietnam war, which you may or may not remember, in which they financed a year or a half a year of the war without going to Congress.

NORBERG: Yes, I do recall that.

NEWELL: And what they did was, they went around and under every rock they could find -- a guy by the name of Alexander -- every rock -- and we were one of the rocks -- so in fact we had this amount. And they said Fine, we don't have to give you any money at all for the next indefinite period. When we finally came out of that we were on year to year funding. Every year we had to put in a proposal and we got money for one year. And that gets to where, in fact...we don't know if we're going to get the money for say the current year until a month or two after that year began. So we were in fact living in a state of faith. I can't remember how bad it was, but it was pretty bad. So we lived in a state of extreme instability in which we have a shop which is sort of fixed at 1.8 million is my recollection. Maybe it was 1.5. No, it's 1.5 in the late '60s. I think it was 1.8 million. It's fixed. We're told to be fixed. We're a little bit early yet for the inflation to begin to eat away at that. And we're also doing things like C.MMP, I might remark. The Speech does come in on top of that, but that's in the mid seventies. '72. So we begin to get relief on that. So we are living really strictly on the margin. So in my mind it's not related to the fear of losing people; it's the fear of losing financial support. We went to the president with a thought out campaign for two things: for a review of this whole thing, for some insurance. What is the school going to do if the bad things happen to us? The bad things not being the loss of personnel, but the bad things being the loss of funds. And secondly, that we believe that the school ought to provide us with some funds of an uncommitted kind. The way we thought about them is we would bank them. We might spend them, but fundamentally we would have something to work with. We engaged in that discourse for a couple of years, I think. Finally the funding we were talking about was $100,000 a year.

NORBERG: That's not much.

NEWELL: No, not much. But we talked about this from the school. The view around here is the school has no money at all, so it doesn't provide any money. We finally got one year worth of $100,000, and it wasn't worth the fight. That's the last time we've tried it.

NORBERG: Essentially, his response was no.

NEWELL: Right. Well, his response was yes, but operationally it never worked. But this was again part of a deliberate diversification. It wasn't that we were actually running quite that scared. We had been living all right for 15 years. We had developed a lot of skills for this. We didn't believe ARPA was going to go away. But you look at it and do a rational analysis and it had all these sources of instability, so you do what preparation corporations do -- you sit around and invent a diversification policy. Try to build a rational policy. All this was a part of a rational policy. We went out and endeavored to do this and we had a lot of good conversations with Cyert. Nothing fundamentally came from him. I'm not sure there wasn't a memo back, but...

NORBERG: All I say is I didn't see it. All right, I will take up from there tomorrow when we get together.

DATE: 11 June 1991

TAPE 3/SIDE 1

NORBERG: We are talking about the proposal for 1964.

NEWELL: There is a funny contradiction in terms between this place growing and turning out to be a kind of key place in artificial intelligence and computer science and so forth, and yet, not being full of entrepreneur types who actually ever took that as their somehow goal in life, and that includes Herb, and Alan, and myself. Also, it turns out Burt. But none of us had any sort of general expansionist tendencies, except for the fact that you simply went and somehow did things at the scale that you thought was appropriate. I never thought that what I did at Rand was somehow expansionist and so forth, even though in fact these turned out to be incredible massive experiments, some of the biggest experiments of their time but I never looked at it as part of a growing... So all that was about, and therefore, in one sense, it clearly depends upon the era, which is not just ARPA but NIH, which provides money without much strings, without much forcing about how the planning is going to go. In one sense, you get a kind of an open-gas expansion without those sort of deliberate kind of machinations that are really necessary and important if your pushing against a resistive medium. And of course, as you observed, we didn't in fact, sort of take off like this.

NORBERG: Let me ask you retrospectively to think about that period and offer an explanation as to why that is the case. That is, why was there a focus on the issues involved in the field, rather than the building of a major agenda or the building of an institution?

NEWELL: My answer to that has always been -- because this isn't a new question (I'm not sure where the question comes from except maybe in conversations between Herb and myself) has always been that that is in fact the nature of Herb. Herb has never been an entrepreneur. It's the nature of Alan Perlis, who was never an entrepreneur. And it's my own nature.

NORBERG: So it's a personal description you're giving.

NEWELL: Right. And you can see one funny aspect of it in Herb's repeated statements as he discusses me on odd occasions that I just keep teaching him how to think by moving the decimal point by a factor of 10. So he was in fact a typical social science small thinker, typical in one sense, but relative to this dimension, but I wasn't the entrepreneur type. Let's see, who would I pick? Mike Dertouzos is a good classic example. Jerry Weisner is another good classic example of people who adopted strongly the issue of institutional growth. Now one of the things is I've never been interested in institutional growth. There is a funny -- not a real contradiction (there are no real contradictions in history), just dissonances -- I took, and I don't know as I view it retrospectively, whether this was a point of honor with me or not, I don't know, but during the first 20 years of my professional life, I took no pleasure from the institution, and I took no pleasure from my graduate students. By pleasure, I mean that I viewed some of the things that were somehow what are the things that you achieved. Well, one of the things you achieved there is this environment. Now, that has changed, actually, but for 20 and maybe even more than 20 years, that was, absolutely truth. These were obligations. You did these obligations because it was appropriate if you were in an environment -- good old protestant ethic stuff. But they were always completely secondary to the science and they were all viewed as obligations. A piece of evidence for that is that none of my graduate students worked on my research. They all picked a research project and I helped them with that research project, but my research was my own research. Actually, I violated that principal in... When? It turns out with respect to three graduate students in the early parts of the system where we got to working on chess programs. They all failed to get their degrees -- a disaster, not because they were working on my problem but because of the quality of the students. That was the view that said again that I dealt with graduate students because that was what belonged to the nature of the scientist life, but not because those graduate students were important to me in terms of what they could do for me or that their production as in the standard model of science which is absolutely true -- I could give some evidence of that in a moment, if you want -- that the progress of science and the progress of moving a scientist's ideas goes very largely through his own students. Not for everybody, there are counter examples, but for me the reason Skinner did so well was that he had a great bunch of graduate students as it turns out. They all went out, we're first class guys, and it really made a difference. That's a big piece of the Skinnerian movement.

NORBERG: When did your view about this begin to change?

NEWELL: When I got to be an old man. That's a joke answer but...

NORBERG: I see it a little bit differently than that.

NEWELL: In fact, it's well after this institution is a famous place, in the mid '80s.

NORBERG: Well, let's go back to where we left off yesterday, because I would see that as the possible turning point -- a bit earlier than you recognize.

NEWELL: But I don't have a memory of where we left off....

NORBERG: We were talking about when your series of memorandum to Cyert about the condition of funding in department.

NEWELL: That's not a turning point at all. That whole thing with Cyert was a blip. I mean it just doesn't mean any thing.

NORBERG: We finished up just talking about that. What I wanted to pick up on today was the planning for a new department that was occurring simultaneously with that series.

NEWELL: No planning for a new department.

NORBERG: Well. What about the hiring of Traub?

NEWELL: That's because Perlis left.

NORBERG: I realize that, but why select him? Why not someone else?

NEWELL: There's an interesting story behind that. We did the usual thing. We didn't have a department head, so we put ourselves on a committee, which, in fact was a committee of the whole -- another interesting thing. We were so small then that the department -- it's all related to the ethos that exists in this department for lots of irrelevant things in which there is total departmental involvement and things like that -- the whole department went off to get a department head. I can't remember that process. It was a typical committee looking at candidates and all of a sudden Joe Traub showed up and he said he wanted to be department head. People aren't suppose to do that. We all had this flurry of meetings and so forth and Joe came on very strong. We sort of checked him out. He was in Washington at the time; he had this history at Bell Labs -- the work in numerical analysis and so forth. We said, this guy isn't all that great, but he really wants this job and we think he'll do a good job, so what the hell, let's do it. That's how we got Joe Traub.

NORBERG: Do you want to go on record saying that Joe Traub wasn't all that great?

NEWELL: I don't know. Yes. Let me state it carefully. This is a historical record. Joe was not a great scientist. He was good, meaning there was in fact the 1964 book on numerical analysis he had done, a research book, not a text book. So in fact, he was a perfectly good senior appointment. He was not in fact, and I want to use the words highly visible, not just as visibility per se. He was not in fact, the kind of person we were looking at. He was not another Perlis. That doesn't mean he was of lower quality, but he was not a star.

NORBERG: A fair assessment.

NEWELL: Right. And he was definitely at that time. Joe's claim to fame is, in part, continuous complexity theory, none of which had started at that time. His interaction with ? was yet to come. This all happened some years later down the pike. He sold himself. He actually did a very good job of selling himself. By this I don't mean he oversold himself. He came and he said I really want to do this, I want to do whatever it takes to make....and throughout his early years, and I think it's still true, one of his claims to fame is that he likes doing the administration and believes he can both do administration and science both together as opposed to most of us cats who keep grumbling about this. He always came on by saying "No Way," and he ran his life accordingly. So that is an emendation to the out and out statement, but he really did. This is the only case I've ever seen, where the guy shows up and says I want it and he ended up getting it. Wee thought we were really taking a risk because of the number of unknowns. It worked out fine. That's all obligation. This whole thing with Cyert is just again an issue which says... you see, I poured large amounts of energy into the institution, and that's separate from the issue of whether you take any satisfaction from it. As a good protestant, you should know that. I'm not a Calvinist, but you should know that duty is a real thing and people can spend, in the worst case, which is not me, all of their lives doing just duty. My graduate students were duty. I'm an Episcopalian, so I don't come from that part of the world. The department, making it work was a duty. This whole NIH thing was a plain duty. I didn't want to do that. Just doesn't say that I was unmindful of all the things that are standard here, the fact that you get to know a lot of people, you get to... In fact that happened to me, but that's not why I did it and I would have sacrificed all of that in an instant. All of this is just response to duty. The only thing that counts is what you see in my research with Herb. It's not my personal research; it is this piece of research with Herb, and Cliff and myself, and then with Herb and myself. So, it's not turned out to be isolated research; it's totally the research of three of us. Other graduate students never got involved in it. Now they began to get involved in all kinds of ways, but in a way which never... This is not true now. Now my world is totally different. There is the SOAR effort. The SOAR effort is a community effort. I spend all of my time working that thing. Now I'm kind of proud of the whole community effort, because it's a fairly unique one. I've changed. Well, changed with respect to my graduate students in about 1980. I can peg that by John Laird and Paul Rosenblum, who are my current colleagues. I can't remember when I changed with respect to the institution because it had nothing to do with behavior. It was much more the question that when you sat down and asked yourself what you've done, the question of whether you actually wanted to count that [came up]. Up to that time I would have never counted that as an accomplishment; it was just not an accomplishment. My graduate students were not accomplishments. There are some funny quirks in that, I admit that, but it was very true, very true. In one sense, if you go after institutions, you don't get any science done.

NORBERG: I know that.

NEWELL: Right. Right.

NORBERG: Let me ask you one more question about funding for this period. In thinking about the 1964 proposal, and subsequent proposals, you put through DARPA, the thought occurs to me, were any of the faculty looking into NSF funding or NIH funding and so on? There were no projects of that kind. All of it was coming... Your shaking your head no.

NEWELL: The answer is no, but you have to remember that there was a big program NIMH Grant. I mean it wasn't big in ARPA terms, but it my recollection is it was like 300 or $350,000.

NORBERG: Per year, or three years.

NEWELL: Per year.

NORBERG: For how many years?

NEWELL: Oh, we probably had it for 7 or 8 years. It was totally programmatic, so in fact, in analogy with the ARPA thing you could decide to spend it. You didn't have to state what experiments you were going to do and so forth. Herb was the key guy in this. He became more the key guy in this as I became more involved in the ARPA thing. Al Perlis handled all of the administration for the ARPA thing. I didn't handle any of it. Literally. Now, when it came time to write a proposal, or something, I would grab hold of this and that is in fact my product, totally and completely. In fact, I never talked to any of the administrative types up there. I didn't know they existed. When characters floated through from the Air Force, who were our agents -- the agent system was still in effect -- I never talked to these guys. I probably talked to them once or twice, but it was Alan who was the host. It was Alan who had to worry about them. Alan did all of that. I just never touched any of that. This is all part of the silent agreement. I don't ever remembering deciding this with Alan. This was all part of the silent agreement that Alan and I had. I don't ever remember handling the NIH money. That was the time when NIMH was part of NIH. I didn't handle that until much later on, and then later on after Perlis left and when Traub came in, Traub was not in that world at all. Then I took control of ARPA. That's when I understood overheads and all that crap that goes along with it.

NORBERG: I'll come back to that because there's an issue in the '70s that I mentioned to you yesterday about proposals that I want to explore. Let me switch to Topic Number 2, and that is the relations with the DARPA people and the relations with IPTO specifically. You mentioned yesterday, when we were talking about the early period at Carnegie Tech, that you don't ever remember Licklider coming to the campus. What contacts do you remember with DARPA program managers or office directors or for that matter DARPA directors during the course of the 1960s?

NEWELL: None. I don't think I ever met a DARPA director until 1970.

NORBERG: Who would have been?

NEWELL: Well, let's see. The first one I met was probably the guy who was boss when Larry Roberts was there. Was it..? Just saw him the other day...

NORBERG: This would be a DARPA director?

NEWELL: Yes.

NORBERG: Lukasik.

NEWELL: Yes. Steve. I'm sure I met some of the earlier guys, but I have no recollection of it and there was never any significant conversation with them. Now, Lukasik was there before Heilmeier. I had quite a bit to do with Heilmeier. But then the Heilmeier period was one of extreme adversity.

NORBERG: Yes, but I don't want to go into that quite yet.

NEWELL: But with Lukasik, I remember Larry Roberts taking me in to talk to him once and it was just a little pleasant conversation. There was nothing of any substance that ever passed.

NORBERG: All right. How do you recollect the way in which the office proceeded then in the late '60s and early '70s, in terms of evaluating proposals? Or developing research areas or anything of that kind.

NEWELL: Let's see. It was always the view of ARPA as proactive and always a view of ARPA as passive with respect to the AI labs.

NORBERG: This is your view. You said the view.

NEWELL: Well, I think it was actually, I would bet, but I'm only a single data point, that that was in fact the general perception. It's only my view.

NORBERG: Well, that's fine. I just want to make sure what *the* referred to.

NEWELL: So, in fact, from my point of view, ARPA never had any plans for us. We were supposed to go be excellent. This was generally true in the two AI labs, which was actually busy generating a culture; that's what they were doing. They were doing a lot of research at the same time, but that wasn't part of ARPA's program plan for these places to go off and generate a sort of a unique culture. So I have a feeling of passivity. Proposals were like these, you talked about this, and you were the guys were the carriers of this and didn't care otherwise and so there were funds that flowed. Okay. On the other hand, Licklider was really interested in time-sharing; so in fact, there were clearly things in support of CTSS and the generation of Project MAC and the attempt to push SDC around a lot. SDC didn't perform very well, so, there was lots of awareness of them being pushed around on the issue of their time-sharing system. I think, in fact, that Al Perlis had a number of conversations with Lick about the time-sharing efforts, the time-sharing efforts here, none in which I was much of a party to but not one where Lick was very unhappy, because the evaluation was always: if you guys are doing some good things - I can't remember. I would reconstruct now by saying that it was always in the background that you had to go do excellent science. But if you did some excellent science, it didn't matter what. Accounting wise was this place was actually proceeding on this path. The same thing was true at MIT and Stanford. I believe, and you can check up on me, that the whole vision and robotics effort that was generated in the mid '60s at MIT and Stanford was totally internally generated; not generated out of DARPA. Now, there's a lot more communication between MIT and Lick for all the obvious reasons, so, probably 100 times as much; so we're sitting out here in the provinces. No one pays any attention to us.

NORBERG: So, when did they start paying attention to you personally now, not to Carnegie Mellon's interests, not to what was going on here, but to you personally, to get your advice?

NEWELL: Well, the first significant event was that Larry Roberts conned me into being on the speech program. This was an indigenous event that happened at one of the ARPA PI meetings in which the troops decided they wanted to go and do speech. This is one of the examples, I'm not sure how many there are, but this was certainly one of the examples in which people arrived at this PI meeting and there was throughout the meeting an undercurrent of interest, concern, negotiation and so forth about "Let's go do speech." Speech had been killed by Pierce at Bell Labs with a famous letter to the journal of The Acoustical society of America, in which he essentially said no one in his right mind would work on speech research and the only people that want to work on it are people that want to do it because they had grandiose ideas and this puts money in the pocket. Pretty incredible letter. Essentially, he then stopped all speech recognition research at Bell Labs. There are some famous things in there about you might want to use the words Gee and Horr in order to run horses, but why would you ever want to talk to a computer that way. He killed it. He killed it at Bell Labs and he pretty much effectively killed it in the field. So, there's this issue in which if you look beneath the surface of the guys, these are the old guys, the group at MIT and the group at Lincoln Labs especially who had been involved in depth in speech research and this was a resurrection. This was an attempt to resurrect it. So things had happened in the previous three or four years and they felt like they really wanted to go do this and wanted to try to go do this and they convinced Larry of that at this meeting. I'm sure there had been some talk before, but I was not a party to it. I hadn't been a part of speech research at all. Raj was. He had come here, and you can probably find out. I'm not sure how much preparation, because in one sense, the PI meeting was not looked at as a thing you prepared for in any manipulative way. It was, in fact, all a collection of the people in the program and you came and it was open and things could happen there. There was lots of activity and Larry came over at the end of that and said, "I can't do this program without someone telling me it's a good thing to do. This is too big." He said, "Why don't you chair this committee?" That's the first time ARPA had ever tapped me. I was a device for Larry. So I did that out of absolute obligation. A little bit of obligation to Raj, because Raj was, in fact, deeply involved in that. The next place where it really begins to happen is when Lick goes down there for a second time. Two things happen. First of all the network is involved. When the network was in place, ARPA was only minutes away and not hours and days away and that's really true. So that, in fact, I could get messages from Lick at 11:00 at night saying tomorrow morning I've got to go before Congress and I've got to have whatever. Now, in fact, from 11 to 4 was shot because you all of a sudden had to turn and try to do something to help Lick out. So that when the

ARPANET really moved the troops in the field much closer. The coupling got very much tighter. I remember being very aware of this. Now, at the moment I look back on this, I don't have much of a feeling of when that happens exactly in time. It is Licklider who really gets me tied in. Part of it because Licklider really had a very tough time with administration. He was fighting with Heilmeier all the time, and so there was a continuous kind of crisis. That is the beginning. There is a period of about 5 years of continuous crisis. Then there was under Kahn a relatively benign growth as well. If you talk to Heilmeier about this and he'll tell you that he was the stalwart friend of all these people and if they only understood what was going on on the other side that he was the bulwark for then they wouldn't think all these bad things. And then there was this benign period and then there was a period after Kahn left and strategic computer came in. It is less aversive now, but up until Jack Schwartz left and the guy who... I can't remember his name.

NORBERG: I want to say Fields, but that's too high.

NEWELL: It begins with T.

NORBERG: I can't pronounce the name.

NEWELL: Well, anyway there was a whole series of these people and Craig Fields was a big element in this. There was just years of aversiveness, but there was this previous period around Heilmeier which represents other sorts of pressure, when Lick was very aversive. I sort of became what I think of almost an advisor to Lick in the sense that I have a tremendous respect for Lick. Of course, there was a lot of self interest in that, but over and above that I just have an immense respect for Lick. So if Lick asked for something I'd try and do it. It turns out that I then ran the speech program.

NORBERG: In what sense do you mean that?

NEWELL: Well, I don't know how much you know about the speech program. There was this report, which actually was... It will be very interesting in how it seems from your side. This was the beginning of ARPA producing strong cooperative efforts and efforts where there were sort of strong criteria that had to be met.

NORBERG: Cooperative. You mean among institutions.

NEWELL: Among institutions and so forth. That's because in some sense the speech effort was sort of a success. So we wrote this report, which set this up. Then we created to run the program a steering committee. The steering committee was made up of a certain number of public members, but all of the people on the thing. All the individual investigators were part of the steering committee along with a series of public members. That steering committee had operational control. They didn't have any bucks control, because in the background are the ARPA program managers and you stayed out of that. They cut their individual deals, but fundamentally it was that steering committee which laid out the criteria and monitored it. We met periodically, forced cooperation, and did other good things. Tried to adapt and so forth. Not a steering committee separate from the guys, but sort of a working committee, as if the whole thing was in fact a joint effort. It was the epitome of trying to do cooperative and competitive research at the same time. All these guys were deeply competitive with each other and they were all committed to being deeply cooperative. The answer is, they were all cooperative until the end of the program and then as soon as the end of the program came they all bitched like hell about how terrible it had all been.

NORBERG: So, who else was on this group besides you? On the steering group.

NEWELL: Raj, Bill Woods, the guy at SDC.

TAPE 3/SIDE 2

NORBERG: Let's go back over these names just in case we missed any.

NEWELL: These names are all the authors of the Speech Understanding Report that was published by North

American after that report came out just as a report and got published. The first chairman of that steering committee was Lick. Then Lick became director of IPTO. I was a public member. I had said I didn't want to be on that committee. I had written the report. I didn't want to run the committee. When Lick left to become director, he clearly could not be chairman. Then I took matters and then I handled it for the next three or four years, at least. During all of that, I don't remember DARPA drawing on me very strongly in terms of getting involved in that and other things, but I certainly had lots more conversations then. You'd go to these things and there would be a couple of the ARPA program managers there, and you'd sit around and talk. For instance, the automatic programming initiative which was done by -- what's his name, the names are hard -- the guy out at ISI, begins with B and was my graduate student.

NORBERG: Balzer.

NEWELL: Balzer. One of my 1960s graduate students. He was the guy that did a sort of survey to set up an entirely similar thing to the speech program. The speech program was a success, so they used it like a cookie cutter. Let's go do automatic programming that way. It failed. The reason it failed, there were probably lots of reasons, but one of the reasons it failed, of course, was that all the people that participated in it viewed it in the typical way scientists do, which is they wanted to do just exactly what they were doing, and was sort of a device by which they could get their funds. Whereas the speech program was actually radically different. The speech program, everybody had a deep commitment to speech; nobody was doing speech, because those programs had all been killed. So there wasn't a single person, with the exception of Raj, who was doing any speech. They all wanted to, not all of them, some of them were new, but a number of people in there really wanted to go back and do speech. This was a device for doing it, so this was the program that was their lives. Now, that's a little separate from did they cooperate or compete. But, it was shunned in the automatic programming thing -- I've got my research program and I don't think I should really cooperate with this guy because he's got his own life and his own character. The speech programs were wired in completely. So the whole issue of what was going to happen in five years, and issues as, should we have a shared data space and other kinds of cooperative activities all focused on this thing. They could never make that happen. I'm not sure that anybody could. There were very unique situations with respect to the speech. From the science point of view, as far as I'm concerned, it was kind of a fabulous experience.

NORBERG: But yet, the Speech Understanding Program was terminated after five years.

NEWELL: Yes. Want the story of that?

NORBERG: Please. In my mind, at least, that contradicts what you said about its being a success.

NEWELL: No, No. The speech program was set up to be a five year program, which had these specified aims for systems that did things, and it met those aims.

NORBERG: How did it meet those aims?

NEWELL: Well, those aims were a list of you need to have a speech system in which the lexicon is so big. They didn't use the word complexity there, but essentially so much time, real time. This big list of specs. So a list of specs and there was a run off in which we went around, the steering committee went around, to every place, and every place ran it's system and we decided which ones met the specs and which ones hadn't.

NORBERG: The specs were something like 60% of human understanding of speech?
Is my recollection close there?

NEWELL: No, it was a list of about 15 items. It's very specific. For these kind of documents it's kind of incredibly specific. Therefore, in fact, it was quite easy to see when systems couldn't do it. It turns out that there were two CMU systems that made it. Hearsay II and Harpy, and no other systems made it. It was clear they didn't and as time went on there was some bitter dispute about the issue. This was, in fact, a research program in which they laid a lot of bucks on the line. Got this cooperation, had a set of objectives set five years earlier and at the end of the five years they tested against those objectives and they declared victory and the victory was real, because these systems actually achieved these results. The style in DARPA, I believe, you probably know better than I do, has always been... Another big contradiction in the ARPA world has always been programs only last five years. Five years is kind of a mythic method. Programs only last five years. At the end of five years, you don't want to become entrenched so you throw them out and you got to do something new. So they have that image and that image has always been there as far as I'm concerned.

NORBERG: I've never seen it actually expressed.

NEWELL: Is that right.

NORBERG: Yes.

NEWELL: Oh, over and over. Over and over. Therefore, the ARPA speech program had to be something new. It turns out the big contradiction to this was the AI thing. The AI thing, my recollection is that they talked this way the whole time and they ran the AI part but not other parts. They ran the AI part as kind of a continuous baby-feeding process. So, a great contradiction. And they really got beat up on it for a number of cases. When Heilmeier came into town, the notion was we should stop doing that. I mean, it's been going on for seven years or whatever it was. What had we had recently. And ARPA does not, in fact, continue to support basic technology development at the level that NSF should. Always contradicted by the facts, of course, but also contradicted by the fact that there was no way to conceive that NSF could ever be brought to produce this kind of support, so this was a kind of a continuous thing. There were, in fact -- oh, I have to think very hard -- there were, in fact, some programs that were killed and moved over to NSF. They had a year or two of funding, and then they were gone.

NORBERG: I remember climate dynamics as the principal one that is usually cited all the time.

NEWELL: There was one in the Information processing area.

NORBERG: Is it possible that this is sort of an ex post facto analysis that programs only last for five years? As a result of seeing it happen a few times under different directors.

NEWELL: I'm sure that as a result of seeing it happen a few times and I'm sure that it was a result of ARPA Directors and so forth treating the issue of programs in that fashion so that programs could be killed. Programs were not going on indefinitely. I have absolutely no idea what the record shows. I know what the record shows for AI. I know that so... Let me go back to speech. So, in fact, there was a Speech follow-on proposal that was written. I could find it for you, if you are interested. Which was written by the Steering Committee at the end. The Steering Committee lived for the whole time. Another guy who was on it was the Haskins guy. The guy, who with two or three guys, started Haskins. Not Lieberman; the other guy. Why can't I think of his name. It's all there, but this committee did not really change over the course of this whole five years. So, it became a club, cooperative and competitive. I'll get back to the other. It was part of the program that there would be a number of major things, major systems. The structure of the speech program was that there were to be some big system places who produced speech systems, and then there were a number of supportive pieces of research done by a number of places. So, I guess Dennis was not a public member. There was an MIT speech project. Haskins had a piece, which is why this guy -- I'm a good friend with, I just can't remember. On the other hand, Ed Nyberg, who's an IDA guy, speech guy, was in fact a public member. I was a public member. I could sit in the same institution as Raj and be declared a public member. A little inconsistency there. The plan was there would be all these systems things and half way through the program we would select out the best couple. Who was going to make that selection? The Steering Committee. And the Steering Committee did. The final vote was done only by the public members, but the Steering Committee went around to every place. We killed a couple of those places. We killed our friends. So this is what I meant by saying that this was a Steering Committee that had power, that had real power, but also included all the people on all of the projects. This was a participatory thing. We somehow tried to control the cooperative and competitive things, which never touched dollars. That was one of its key things. Always there were the program managers running around doing their independent negotiations, and you often didn't know quite what they had agreed upon with these given places. We did this and they stopped doing that. Of course, the actual stopping occurred in the only official way it could, mainly through ARPA program managers, who were part of all of this. The ARPA program manager stopped the funding, the follow-on funding for these guys.

NORBERG: So the Steering Committee wrote a report at the end of the five years which essentially said to continue the program.

NEWELL: Yes. It actually wrote a separate report called the Follow-on Report. It was not a report that summarized the program. It was, in fact, a proposal for what to do for the next five years. That was built on the image of the five-year program. You needed a new five-year program. The money vanished for that. There was money in the budget for that, to support that at some reasonable level. Can't remember what the level was now. Not quite as rich as the original speech program. I'll give you two stories. As far as I'm concerned Heilmeier killed it. Okay. Now, Heilmeier was against the speech program from day one. The speech program was underway. He had been involved in speech down at TI.

NORBERG: Who had been involved?

NEWELL: George Heilmeier.

NORBERG: At TI? No. He went to TI after the Defense Department.

NEWELL: That's right. Let's see. He had been involved -- now let me think... You're right. You're absolutely right. He had been involved in speech research... The name I associate with it was Dodds. He was a TI guy. Yes, because he came in from DDR&E.

NORBERG: Was it at RCA?

NEWELL: I don't think so. I might actually be wrong about that. He's a device guy. It may only be that it was this issue about system versus devices. But, in any event, he believed that the speech program was a boondoggle, shouldn't be supported, and attempted to kill it for most of the time that he was there. He never managed to do so.

Now, in fact, the other part of this story is that there is a famous guy in Congress, a staffer, called Batista. Know about Batista?

NORBERG: Oh, I've heard of him.

NEWELL: But he's no longer. He's now retired. But he was a staffer for some guy, I guess actually the Appropriations Committee, for a long time. He himself was a technical man. He was a computer engineer. So, he was, in fact, a guy with a lot of expertise. Did not believe in software. Meaning by that he certainly believed in software, he just didn't believe you could do research on software. He was actually a very sophisticated guy. But he really, as staffers do, or can do, got in a powerful position -- he exerted, in effect, for years and years, and I actually saw a film on him the other day, which was talking about how some of these great big weapons systems were finally brought to heel and so forth. Batista played a big role in that and he was actually on this film. There were a lot of interviews with him and so forth about the issue. So, he was a kind of ? ? guy, but, also, very positive, very pro, very pro, military systems guy, but had a lot of notions of who was pulling the wool over your eyes. Anyway, people in ARPA fought Batista for years, like 15 years. A piece of the history here is you go over to Congress and you have this program and when Batista gets hold of it and it's just torn to shreds and you come home with nothing. It turns out that Batista killed a software technology program. So Heilmeier went over with a technology software program and Batista killed it. The way Heilmeier supported that software technology program was by taking the money in the pot for the follow-on, took half, three-quarters of it, or something. That's what I heard. I think I actually from George. I can't quite remember. It's all a little dim. So that's the other side. There's Heilmeier doing his best to deal with this set of problems and making a judgment of whether it's more important to support software technology than do this follow-on, and probably not actually killing the follow-on with this first act. That only guts it; it doesn't totally kill it. There's still some chances, but then once it's a wounded bear, then the other money is kind of available at much less cost, because it's unlikely that program is ever going to rejuvenate. So, it never happens. On the other hand, I have this image of Heilmeier, who is adamantly against speech, adamantly against AI, and adamantly against speech, killing the Follow-on program could have been, from my point of view, a primary move. On the other hand, Heilmeier goes down to TI afterwards and, in fact, becomes very pro speech research. He goes down and touts it as a great thing. He gets TI heavily into it at a more device level. Becomes an AI expert system guy. Saw all these pictures in Scientific American and so on. So, at this stage of the game, I don't have the foggiest notion what the true George Heilmeier is. I had no independent access, I don't mean now. In those days, I could go argue with George, which I did a few times at crisis points, or whatever. But fundamentally, he was the director of DARPA and fundamentally, I was helpless and never saw myself in all of this as developing independent channels to Congress and other things that could benefit the speech program. A level of activity that one can decide that that's how one is going to proceed. Okay. I've never done this. I've never wanted to so. So that's why there wasn't a follow-on. You get several funny issues about the speech program. Speech program was a great success. It was an absolutely fabulous success technically speaking. The Follow-on didn't happen, but not because of any issues of the early program not being a success. On the other hand, the speech community was always strongly anti the speech program, which is not to say there weren't major members of that community like Dennis Clap, and the fellow from Haskins and at least one other person, who were major members of the speech community. But the speech community's view was that ARPA had come along, they were the scientific speech community, recognition community and speech, acoustics and so forth, their community was living on a couple million bucks a year. ARPA comes along with 10 times the money, grabs control, as opposed to putting that 10 million bucks and doing what they think ought to be done, which is to get the speech science going, puts all its money in this damn system, which none of these guys believed in anyway, and so there was an immense amount of envy coupled with belief that this was the absolute wrong judgment and a nouveau rich issue and a belief that none of these guys here understood anything about speech that was worth understanding. We got these in various combinations. There was a muted criticism during the course of the program. The program had made its move toward cooperation, and it had got, in fact, a number of these people. There were meetings of the Acoustical Society. There was a flow of papers. It did not create a separate technical world. Lots of stuff got published and symposiums were given, papers to the engineering speech community. Cooper was the Haskins guy. One of the very early engineers who moved over to speech research and did some of the very early instrumentation of speech. So, I lost the thread there. It grew to a post judgment that the Speech Program was sort of a failure. I'm not of that community so I don't know how strong that was. There was sort of three negative things. The follow-on didn't happen. The speech community could finally carp was more openly.

NORBERG: This is the larger community.

NEWELL: This is the larger community. The speech community in AI, the speech community in DARPA, of course, is only part of this. All the systems folk and the guys who seem to really control it, like the Raj's of the world, really are not members of that community, not fully members of that community. Within the little speech group, the Speech Understanding Program itself, as you took the lid off this forced cooperation, you got a certain amount of sour grapes. The feeling that the program would have been a lot better if these guys would just have been allowed to go their own way and do their own thing and not be enforced to. If you ask me that, I would tell you, in fact, that I think a lot of success of that program was that people were forced to keep their nose to the grindstone and the competitive aspects this whole runoff and so forth. And who was going to make it at the end of five years is forced science, but is a mode of science that really pays off.

NORBERG: Were you part of the group that evaluated the re-introduction of the speech program in the '80s?

NEWELL: Nope. I don't think a group did.

NORBERG: Oh, all right. I'm just testing here.

NEWELL: No, I mean, it might have, but I think, in fact, money gets picked up for that. Raj never got out of the speech business. The speech program got killed. Why you just see things about having environments versus having projects. When the speech money left, and simply this program just ran out, then a place like BBN which had a speech project, simply stopped doing Speech, Why? There simply was absolutely no money to pay for anything. Okay. Here, the money all went away and so Raj hunkered down and got money from a bunch of other little places in the environment. He shifted to worrying more about how to put more of this stuff on minicomputers and doing the more speechy things. He kind of kept going at a reduced rate. So, that he has, in fact, continued to move forward on the speech program until it finally woke up again in the '80s. He could do this because, in fact, there are no boundaries in this environment. So it was sort of possible to do that.

NORBERG: I'll come back to that in a minute because there is a question I want to ask you about the late '70s, here. How friendly were you, over the years, with Ruth Davis?

NEWELL: Well, see. At some important level to you, not at all. At another level, I'm not sure I ever talked to Ruth more than 5 times in my life. But there's a funny thing going on here. I'm essentially well known in this field. Consequently, there's a lot of people in the field who I have very positive relationships in which I know them and they know me and we get together and interesting things happen. Sometimes they're just good technical conversations, but without in some sense there being when you would talk about as a lot. In fact, I never had anything but sort of good interactions with Ruth, but I don't think I had more than 5 of them in my life. I remember that at one stage of the game I was working with Gordon Bell. We had just done a thing related to PMS -- Processors, Memories and Searches. We had done a research thesis and had a system for working with this and we thought this would, in fact, be a great specification language for how the world ought to specify computers. In particular, how the military or the government ought to make specs by giving PMS specification out to the world operationally and so forth. So this was the development. Who should support this? Well, Gordon and I sat around for a while and we said that it's clear that the Bureau of Standards ought to support it. So, I called up Ruth and we got ourselves a little grant to go do that. After she said yes, after everything had been approved, Nixon cut the budget for a lot of this stuff, including the Bureau of Standards, by 30%. It was an event. And this thing just went away. It went away within days of when we were going to get it. With all the approvals and it just coming down. I can't remember how broad that act was, but it was in the paper and everywhere; he stepped in and made a big cut and the big cut included a lot in the Bureau of Standards. So, it might have actually been focused on the Bureau of Standards. I just can't remember that now. But all of a sudden they dropped by 30% or something like that. So, all of a sudden we never got this. The issue is that I was able to pick up the phone and talk to Ruth and give her this idea and talk to her about this idea, and she thought it was a good idea. It wasn't very big. It was to be another year of development to see where things would go. And, you know, those are all sort of positive, make-do kind of things. At another level, I hardly knew Ruth at all. I suppose I should say "Why do you ask".

NORBERG: I'm going to tell you why I ask. You remember her at the Department of Defense?

NEWELL: Not really. That is, I'm sure I in fact interacted with her at the Department of Defense several times, but never in a way that coupled it with the Department of Defense.

NORBERG: Okay. In 1965, she was interested in sensing. An automata project was developed within her office, which was outside DARPA. Within her office, to promote this business of sensing for use in various military systems and Sutherland, who was then in the IPTO office, and you and some others, although those people are unnamed in the documents that I have, were called in to give some advice on this. And, in fact, early funding for AI at MIT anyway -- I'm not sure if any of it came here -- but early funding for AI, at the time it was splitting off from project MAC, was provided through the automata project, not through IPTO.

NEWELL: And I participated in that project?

NORBERG: Well, you participated in the definition of the project.

NEWELL: Yes, I participated in a meeting?

NORBERG: It doesn't stand out, obviously.

NEWELL: There's been a lot of meetings.

NORBERG: Let me ask you about another series of meetings, then.

NEWELL: In those days, one didn't have to have many, many meetings in order to get some things to happen.

NORBERG: I'm going to ask you about another series of meetings, which are relatively opaque to me. They may not be discussable because of the nature of them and that's the Jason Group.

NEWELL: Ah, right.

NORBERG: Now you were involved in some studies for the Jason Group, although you were not a member of the Jason Group.

NEWELL: Right. Was not and am not.

NORBERG: You did participate in some studies that they were interested in in computer science and it may even have been specifically in AI. Do you remember any of these?

NEWELL: Let's see. More meetings. I don't know much about the Jasons. I'm not wired into those communities and never have been. By the way, it's conjecture, but this goes back to this issue I made with respect to Ruth Davis, having this kind of -- pick any word you want -- august position in the field, as well as the position of being a person with integrity. There are all kinds of ways in which there are very sort of microscopic examples which don't represent from my point of view, really extended connection and so forth. You get involved, and then they pass in the night, as far as I'm concerned. They are clearly part of the process. Now, it turns out the Jasons wanted to know about AI. All right, Jasons want to know about a lot of things. Every time the Jasons get together their business is to know about things. So, they invent things to know about. So, Saul Amarel, who is a Jason, put together a program, and I went out and participated in it. So, I went out to La Jolla. And they didn't like what I said. That's what I remember mostly about it. So I talked to them about AI and that was it. In one sense, I'm an input to a Jason's study. I'm not a participant to a Jason study.

NORBERG: Well, wasn't a sub-group established to do this with people both from the Jasons and people from outside? That's the story I heard.

NEWELL: Well, that might be true. Was I a member of that.

NORBERG: Yes. Of the sub-group. It was done out in California. That is also part of the story that I have.

NEWELL: So maybe, in fact, we did sit around and. My first reaction is, I'll be damned. It certainly didn't exist to me as a very significant thing.

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NEWELL: Some of this is the deeper question of whether this flow of reports, studies, and so forth, which is a continuing activity, I don't want to say it that way. How significant a role; but that's not true. We know, I know, you know that, in fact, a random selection of those things have played incredibly strong roles. There was Project Charles and Project Lincoln, which I read when they first came out. I was at Rand when they appeared. The speech report is a good example. There are lots of them. Most of them don't ever mean a thing and so they don't usually stand out very much for me. So I don't really remember this as a thing that I participated in a report. It's entirely possible that when you dig up this Jason AI report, if you do, then I'll be an author on it. It would have happened; we were out there for several days. I remember that. Now, this might loom very much larger in Saul's mind.

NORBERG: Well, it does loom larger in Saul mind, which is where I got the story from.

NEWELL: Well, he would have been the guy that organized it and I believe, in fact, that

NORBERG: Yes, but it's not clear to me as to whether he participated in the study group, because I vaguely remember him saying and I'm trying to dredge all of this up now, I vaguely remember him saying that in order to evaluate the results appropriately for the Jason Group it was best not to participate in it or something like that.

NEWELL: Yea. I have a vague recollection of that, too, actually.

NORBERG: What I'm looking for is whether this had anything to do with Heilmeier's objections?

NEWELL: This seems to me post Heilmeier.

NORBERG: Is it. Post-Heilmeier, not pre-Heilmeier.

NEWELL: Yes.

NORBERG: That's interesting. Well, then we don't have enough information to pursue it.

NEWELL: Now, I have to be a little careful, but I don't see Saul, I don't know when Saul became a Jason, I really have no idea.

NORBERG: I don't even think he said. It was not really relevant to the interview that I did with him.

NEWELL: Sure. Heilmeier comes up in the early '70s.

NORBERG: Well, '75 is when he takes over at DARPA. So, it's from '75 to '79 where he has his biggest influence on the program. And what he said to me is that while he was in DDR&E that he did not have responsibility for DARPA. That the DARPA people were not reporting to him. So that he didn't have any influence on the program.

NEWELL: Yes. That's exactly right. He didn't exist for us.

NORBERG: Yes. So my recollection is it that when he starts making objections, one of the things he uses is the Jasons.

NEWELL: Well, that can easily be, I mean, in general. There's lots to ARPA besides IPTO, in one sense, really, and lots of ARPA style is not IPTO style. It's different and generates from other parts and of ARPA and its history of working on missiles. A lot of other things could happen, materials research and so forth. So that ARPA is not, my view is that ARPA is not characterized by IPTO.

NORBERG: Is IPTO characterized by ARPA, though?

NEWELL: Well, some of the ethos that you hear about is ARPA ethos and IPTO ethos. Those are identical.

NORBERG: I see those as identical.

NEWELL; Yes, but, in fact, there's a certain way in which computing is not assimilated by the rest of the defense community. Consequently, there is a certain way in which the whole IPTO thing is kind of an isolate, not really an isolate down there, but here's ARPA with all of its main business which is coupled in really heavily to direct military technology. And then there is this decision to go do something about information processing, which TTO and STO guys can maybe buy into in the abstract, but don't really see it as highly relevant to them. It sort of exists a little bit in isolation. Now, there are some pieces, there is more than one piece of it, that is similar to TTO, etc., but IPTO has a slightly different character. In particular, TTO and STO and so forth do not support basic science. IPTO builds this whole strategy for supporting and creating and nurturing a scientific field, and none of these other characters are into that at all. In fact, my belief is the fact that these programs last only five years is an ARPA kind of view, not an IPTO view. I mean, it becomes part of an IPTO view, but it doesn't generate in IPTO. It generates from doing these technology projects and trying hard to go do things and get out, put new money on doing something else.

NORBERG: Well, what does make the IPTO office different? I don't want to put words in your mouth, so let me call it different than other ARPA offices.

NEWELL: Well, that answer comes at two levels. First answer is, I don't know, because I didn't know much the other ARPA offices. So in one sense I answer that really easily. But I have a second answer. Second answer is, I did not see those other offices as run by people who were viewed as first class scientists in their field. It's always been part of the IPTO mystic, which we failed on a couple of times, but with much notice among ourselves, that the guys who ran IPTO were ourselves. Licklider, Larry Roberts, Ivan Sutherland, these are people who were no better than, they're not bureaucrats and they're not even people who are less scientifically than we are so they can get in, and Kahn, and get in and argue with you at a level where it is absolutely even field. So, the idea that IPTO in the form of Larry Roberts, and Bob Kahn, would go and simply create a research project called the ARPANET and simply do it themselves, meaning, not sit around and wait, perfectly good sense. No eyebrows were even raised about that, because they're just guys like us. Now, my belief is that none of the other characters floating around in TTO and so forth, have that character. Like, in fact, none of the people in ARPA have much of that character now. That's not true, we've got Barry Boehm there and Saul. But, less and less have you been able to get the top people, whereas, in fact, Lick and Larry and Ivan all had the, and some of the program managers that got in and this is probably duplicated; like Steve Crocker and Cordell Green. Cordell Green was the brightest AI scientist of his time. I mean, there is the typical kind of mythology that floats through computer sciences: what is the big Ph.D. thesis this year? Winograd's thesis in '72. Okay. Cordell's thesis in '69 was the big thesis that year. Then he went back into tank school, and when he came out and he became a program manager. Here's a program manager who is one of the brightest guys in the field. So again, there is this feeling in which there's scientific communion between the guys at IPTO and the guys out in the field. There's a Colonel... Jones, who becomes a director of IPTO. No, not Jones.

NORBERG: Russell.

NEWELL: Russell. David Russell. Shock waves occurred. Dave Russell. Not that people had anything against Dave Russell. It was not a clubby statement. It really is a kind of science, technical statement. What level is this guy at with respect to his ability to deal, or to put it another way, can you trust his taste with respect to the science. So that when he goes and does things and wants to push things, then you're sort of relying on him in the usual way you do. You don't usually rely on people totally. When I say I want to go off and do something, people have to take my taste seriously. Lick had formative taste. He already showed that in "Man-Machine Symbiosis." Formative taste. Ivan had taste, and Larry Roberts had taste, and so in one sense you might argue with them, but you got to go with them. You have to believe that his taste has just as good a chance as your taste as being right from a scientific point of view. That cast a certain flavor over the whole negotiation. The whole piece.

NORBERG: Do you remember any stories about why Russell got the job?

NEWELL: Let's see. He got the job during Heilmeier.

NORBERG: That's correct. Lick left about '74.

NEWELL: Lick was forced out in '74, '75, something like that.

NORBERG: Yes, it had to be late '75. And Russell came in several months later, not right away.

NEWELL: Right. The answer is, this is the choice of Heilmeier. I don't remember a lot.

NORBERG: I just wanted to know if there was any scuttle-butt in the community that you may remember.

NEWELL: Well, I can kind of remember, and it's not very devious. There is this antagonistic relationship between Heilmeier and the AI community. And in fact, between Heilmeier and the university community. Heilmeier's first announcement when he came in was ARPA isn't going to do any university research. Let NSF do university research. ARPA's business is to put money into...

NORBERG: You know that for a fact. He made that statement in your presence or in writing that you saw.

NEWELL: Nope. The answer is I don't think I ever saw it in writing.

NORBERG: I certainly have not seen it in writing.

NEWELL: I may have heard it from him. There was a famous meeting in which Heilmeier got all the people in the ARPA community together down in Washington. Shortly, I can't remember how long. And told them all off.

NORBERG: Tell me about the meeting. I've heard about the meeting before.
Tell me about it from your perspective.

NEWELL: It was a wonderful meeting. I can't remember now what the excuse was. It wasn't a devious excuse. It was, I would like to have a meeting about the nature of ARPA research, or something like this.

NORBERG; So it was not specifically AI. It was not specifically IPTO.

NEWELL: OH. Absolutely it was IPTO. It might have been AI. I'm looking around the room and it seems that there are too many people for it to be totally AI. It was absolutely an IPTO meeting. It had nothing to do with the rest of ARPA. It seems to me that there were too many people around the room for it to be just AI. But it might have been just AI. It really might have been. My recollection of the top order bit was that we all trooped into this room and he sort of lectured us on the way it was going to be, which was full of negative issues. I would give you even money that he stated some things like that at that meeting. None of us said a word and we all trooped out. That is, he totally cowed us. He totally cowed the community. The community did not fight back. He came on very strong. I don't remember what all the pieces were. It lasted for several hours. It was not a real short meeting.

NORBERG: Nobody responded? That's interesting.

NEWELL: Well, we certainly did not. We certainly did not respond sort of there in which people got up and took him on. Nobody took him on.

NORBERG: Was there a follow-on meeting, then?

NEWELL: I do not remember a follow-on meeting.

NORBERG: Because some people report that they did take him on.

NEWELL: At the meeting?

NORBERG: At a meeting. Nobody is very specific about what the meeting was. The meeting that I heard about, when they took him on, was held out at the Marriott, out beyond Dulles. Right out near the entry to Dulles.

NEWELL: I don't remember.

NORBERG: That's the one that I heard.

NEWELL: My recollection of this meeting.

NORBERG: Let's go at this a different way. There are other ways to recall Heilmeier than through such a meeting. I want to go back to...

NEWELL: I mostly recall us from that meeting. I was so amazed, because I participated in that meeting.

NORBERG: Okay. I want to go back to the Licklider years.

NEWELL: The second time.

NORBERG: The second time. '74 and '75.

NEWELL: My recollection is this. Heilmeier is not implacably hostile to all of this stuff. There is in fact a 4-year period where this stuff goes on. Okay. So things go up and down. But, in fact, he picks the successor to Licklider. Licklider is forced out. So there is a good deal of unhappiness. Therefore, it's not surprising to anyone that Heilmeier is not going to consult the field with respect to who we're going to get next. And, therefore, he goes and picks a person on his own. That's the sum and substance. As I recollect it, that's sort of obviously right and it's probably not any deeper than that. How he picked Russell rather than some other guy... Russell turned out to be a pretty reasonable guy. I don't think there was any hostility against Russell at all. I don't thing any of that. He was a Ph.D. Physicist from LSU.

NORBERG: In fact he became acting director when Licklider was pushed out. And it was several months before he was given the job on his own. Now, the idea that I had was that Heilmeier was looking around for somebody and couldn't find anybody as happened then later on when they were looking for people.

NEWELL: I'm sure that's right.

NORBERG: I'm not sure it's right.

NEWELL: Well, he was certainly not looking in any way that I knew within the community.

NORBERG: The community wasn't consulted. That's an important point here.

NEWELL: And he certainly did not consult the community. That doesn't mean he didn't consult some particular people. But, fundamentally, I didn't know. And he would not be likely to have consulted them given the sort of unhappiness that was floating around when Licklider got pushed out.

NORBERG: Well, do you ever recall being consulted before or after Heilmeier about a new IPTO director?

NEWELL: Oh yes. Oh yes.

NORBERG: Which time?

NEWELL: Saul Amarel. In fact, it turns out that getting the next director of IPTO was given to the community by Cooper. There was a little committee, of which I was chairman, no, I wasn't chairman, Keith Uncapher was chairman, but I did a lot of the work, I actually did most of the work with respect to Saul. This committee interviewed a whole bunch of people. We met in Washington on a couple of occasions. Saul happened to be on leave here at the time and I remember talking to him in this room about this whole thing. So, I probably had a fairly strong -- you probably heard this story from Saul as well -- but I have a fairly strong recollection of anguishing with Saul about the pros and cons of doing this. Trying very hard to get IPTO a guy from the community we thought would be pretty good. At the same time, since I'm an old, old friend of Saul's, I had to be fair and so forth about the down side. There was a standing committee that Cooper set up, which was to both deliver program managers, because, in fact, they were having a hell of a time getting program managers, and then to deliver a director to IPTO. I think that I was not at all consulted when Saul left. I think that's right. I don't remember. I think in fact that Jack Schwartz was a totally individual deal between Craig Fields and Jack. I was certainly asked a whole bunch of times about who would be a good ARPA director. Probably a hundred times. Every time this comes up, and likewise, it is a kind of constant litany that comes from there about let's get some program managers. We have to have more and better program managers. They ought to come from the field. This is your thing. Bill Sherliss [?] is a program manager down there now. He's an Associate Professor from CMU who was in this same era, and I was also involved in the sense that I had long conversations with Bill. Also after the fact, sort of believe that this is one of the things where CMU has helped pay its dues. In fact, I had another guy who worked with me by the name of George Robertson, who almost became a program manager. He actually agreed to go down there and then backed off. Backed off for the absolutely right reason. He was actually working with them, because he said he was coming in, and they were waiting for all this secret stuff. All the stuff that they do. And so he was spending, I don't know, maybe a day a week or something like this, and decided he couldn't stand to have all of his friends, all the people that he knew, treat him only like he was a bag of money, which is something those guys really have to put up with. It's terrible.

NORBERG: I remember that from NSF, when I was there.

NEWELL: There are things that smooth it over, but it's always there. It's just always there. People are always. Even when they're not you think they are. He went down and he talked to a whole bunch of people around the field he knew and they all treated him differently. After about a month or two of this he said I can't stand it and he backed out of it. I think absolutely rightly so.

NORBERG: Yesterday, we talked a considerable amount about the proposal process or the lack of the proposal process in the 1960s. I'd like to talk about it in the 1970s, because I have seen documents which are quite different than the proposals made in the 1960s. I'm thinking specifically of an exchange between you and Licklider in 1973 or 4, one of those years, in which you submitted by E-mail an outline for a proposal. I'm assuming you didn't expect that to be the proposal, but you submitted an outline for this proposal and Licklider sent back to you a twenty-seven page, hard copy, analysis of the outline, making a number of suggestions about what should be dropped and what should be included from the list plus a number of other things he thought ought to be included in place of things that he suggested dropping, because of the procedures he believed he had to use to get the proposal approved.

NEWELL: I don't remember that. You'd think I'd remember a twenty-seven page comment.

NORBERG: I'd think you'd remember twenty-seven pages. I know I sure as hell would.

NEWELL: I probably would have remembered it, if I reread it. We shift in the seventies to this year by year affair. So there is this hiatus on any proposals and then we pick it up and then things become much more... This is during Vietnam. Things become much tighter with respect to the whole proposal process. There also turns out to be a lot more. There's less money around. So, what characterizes the sixties is you never have... Oh dear, dear, dear, dear. I have a recollection of a conversation, one of the few conversations I ever had with Larry. See, I almost had no conversations with Larry during the course of his tenure. You have to again understand that I knew Larry as a graduate student at MIT, so there is this relationship in which very small amounts of communication end up both in understanding and a kind of a trust. One of the very few times I had a conversation he got me in to tell me that absolutely Carnegie could have no more money. He wouldn't stop it, but no more money. He said you guys are getting big enough now that you show up and therefore all kind of questions start getting asked and I've got to just hold you at 1.5. I don't believe it was 1.8; it was 1.5.

NORBERG: Actually, it dropped from 1.8 to 1.5. It ended up.

NEWELL: In fact, this is probably the same conversation where he said I've got to cut you. It had nothing to do with quality; it had nothing to do with anything except a decision, I shouldn't say a decision on his part, a decision on their part, I have no idea. By the way, the office was very small at that time. There were like 3 people in the office at the time, there's Larry and one program manager, his name was Barry something or other, and one secretary or something. Oh, and by the way, there was also screams from Larry, "Why can't you people get me some good program managers?" So, this is actually at some level, which is not quite the level of sort of inter-discussions, but the standard level in which Gordon Bell called me up yesterday. I got home and was stretched out for my nap. All of sudden Gordon Bell called me up from Microsoft. Why? Because he wanted to find out somebody to hire, or some damn thing. People consult you all the time, you know. You don't take it seriously, you help when you're on the phone and then you forget it. So, in fact, there clearly was in the '60s, this hitting of a ceiling, which I do not associate with limits on ARPA dollars. I mean there were limits for us on ARPA dollars, but it was related to justifications and so forth.

NORBERG: This would be the early '70s. Or the late '60s.

NEWELL: Well, 1.8 to 1.5. That was in the '60s.

NORBERG: No, it's about '72. He didn't leave until about '73 and he comes in about '69 as the head of the office.

NEWELL: Well, he's down there as chief scientist. Then takes over when who leaves?

NORBERG: Taylor.

NEWELL: Oh, then Taylor brings him in and Taylor leaves, right. right. That's right. So, that was actually in the '70s. I can believe that. In fact, it actually couldn't have been in the '60s, because it would have been Al Perlis communicating with ARPA. In the seventies I was doing it.

NORBERG: But you're right about the details. You're having no problem with the details. It's the dates that are wrong.

NEWELL: Just the dates! A lot of years in here. Especially when you've been doing business at the same old stand for so long.

NORBERG: So, what's the next step from that. The putting a cap on whatever money is going to come on an annual basis to CMU. Putting a cap on that and moving into Licklider's concerns about the way proposals are being justified. The justification now seems to be shifting back to the PI, whereas before it doesn't seem to have been in the hands of the PI.

NEWELL: Right, Right. One of the things I need to... There's an interesting negotiation. It was in this period, which must have been '71, which is one we decided to go do C.MMP. This after-the-fact session with the ARPA told in which Larry got pretty up-tight about this. It's still there at the point, so this is after the bucks have become to come back. See, I can't remember when we stopped submitting. So there's got to be a two-year or a three-year hiatus.

NORBERG: Stopped submitting. What do you mean stopped submitting?

NEWELL: As I told you, we were told to not submit proposals for either two years or three years. This is even computed from the fact of what we have. Therefore, we actually forgot how to write proposals, because all of a sudden for several years we didn't have to write any proposals. We simply chewed up the money that we had.

NORBERG: Which would be their commitment to your three years out.

NEWELL: Yes, three years out. Therefore, there were no proposals written for them. It just came.

NORBERG: So, was it possible that the proposal to Licklider, then, is the beginning of the new submissions?

NEWELL: No, no, let's see. Hell, you have to reconstruct it, because I can't. It starts with the Lyndon Johnson year in which he does not ask Congress for money.

NORBERG: That has to be '68 because he was gone in '69.

NEWELL: Right, so, that was the year. So, it's '68, '69 and '70. So, in the seventies we started writing proposals. I don't see those, and I can't remember, but it can easily be the case that those proposals in the next couple of years were now every year proposals, but they were not real proposals with all this other apparatus.

NORBERG: Because you had the money anyway. Yes.

NEWELL: We began negotiations, real negotiations, on how to get a little more money here, and a little more money there, at some time in this period, and I can locate the event for you. This was when Steve Crocker was program manager. Steve Crocker had CMU and it was still a case, I believe, when program managers had institutions. They have varied on that. There was a program manager for the whole institution. Later on, it got to where there are separate program managers for each of the things. Although they oscillated back and forth on that. I mean, they played administrative games like everybody else has to. But these things got too aggregated and then you go back and you put one guy kind of in charge to provide some coordination. I think that's what they're trying to do now, actually.

NORBERG: It's a question about what they're trying to do now.

NEWELL: Let me see. This was a negotiation with Steve Crocker in which if we would go to work on symbolic descriptions of computers, he could get us 200K more. He had a little pot that that would fit. So, I remember coming back and for the first time in my life going to Bill Wolfe and saying Bill, let's cut a deal, meaning, I've got a way of getting some money, can we think of ways in which your research can be...

TAPE 4/SIDE 2

NEWELL: ... cast in a way that we can now justify this. So, that was in fact a little dicey, because you don't do that. The answer, of course, is if you want the 200K bad enough, you do that. Dan Sieworek got involved, and out of this we generated this little program, which turned out actually to be the start of an immense amount of stuff that's happened here, including PQCC. All of this stuff grew out of this program and we hired Mario Gorbachi [?] as a research associate after his Ph.D. Mario has been here ever since and working on these things, and Dan Sieworek's stuff grew out of it. What we convinced ourselves was this was a pretty hot area. That is the issue of actually describing the machine so that you can do things like build compilers off of that and build other kinds of things. The whole ISP stuff. Is ISP familiar to you?

NORBERG: Yes.

NEWELL: The whole ISP starts out to be a notation that's created by Gordon Bell and myself for our book. There is a thesis done on it by Mario Gorbachi. Then some other things that Sam Fuller was involved in and actually relate to the Army and the Bureau of Standards, I think. It became a fair success story. This fuels a lot of that. We didn't start it, but we sort of twisted it to be part of that and that to be part of this. We named it the Symbolic Computer Description and brought a number of other things under it and actually ran it as a project for a while. We had this one guy Mario as sort of the glue as a research associate to help make things considerably smaller in scale across the whole board then. I remember that, in one respect, very much, because that was a real deliberate deal to break out of this 1.5. I don't remember this as a 1.5 thing. To get an actual increment of extra funds, so it was real piece of deal cutting. Lick was not involved in this. I have no idea whether he was involved in this in the background, but he was not involved later. Steve was involved. In fact, Steve went on and did his thesis in an area related to this. I remember mostly for the negotiations, and that's got to be '72. Just because it's got to be after C.MMP, after BLISS, those are things that Bill did earlier, moving on towards Hydra. He was beginning to do things to crank up the P2CC [?] as his next project. This is a sort of a start of that. By which I don't mean that this initiated that, but we all sort of twisted and that sort of helped shape the environment. From then on, proposals are every year; they are much more justified. I still have no recollection of that twenty-seven page memo. I can believe that it went back and forth on the ARPANET, although I guess it must have come back on the ARPA net and just didn't print it out.

NORBERG: No, it doesn't look that way. It looks as if this is something he did on his typewriter, actually, not on a machine.

NEWELL: That seems unlikely.

NORBERG: I think for reasons he didn't want it on the system. That's my guess.

NEWELL: I see.

NORBERG: You see, there are many comments in this criticism about what's happening inside IPTO.

NEWELL: This is '74?

NORBERG: Yes.

NEWELL: '74. Then this is actually before Heilmeier.

NORBERG: Yes, it is. So the questioning is already beginning, is my point.

NEWELL: Well, this whole era is a fairly rough era. Let's look at it from Heilmeier's point of view. I think he see's himself as a white knight. I don't know. Maybe only he does so on Thursday's. Several conversations I've had with him, in which I have approached at one level or another laying out some of my feelings, one response I've gotten in several other places in other arenas as well from him, had to do with what he was having to do on the other side to maintain things and so forth. I suppose we'll come to call this the Yerizelski [?] point of view. It's actually a very interesting issue. On exactly this issue. They can beat you up on this side, but they don't know what you had to do in order to get them to even beat you up. What I remember most about that period was interacting with Lick on the network late at night, and not always on proposals, but on all kinds of support, evaluations of AI, not always with respect to Congress, often with respect to just getting some ideas for just doing some things. I have a feeling that I actually would spend several nights a week on this. I have no idea at all whether that looms large in Lick's eyes in terms of the whole array of support resources he has. He had an immense set of these. A huge set of these from previous history at MIT and IBM. I have no idea, but I sure know that during that era I was really locked in to this, to this issue, and to become very unhappy with it.

NORBERG: Why.

NEWELL: Well, I'm supposed to be doing science, man. I finally took my first sabbatical in '76 or '77. I think it started in the fall of '76. We can get this tonight from the files. I think it's the fall of '76. My only description of myself that makes any sense at that time is that I'm "black angry". I'm just black on the inside. I have been working for 6 or 7 years trying to make all this happen. It's been high stress, and happy with it in one sense because I fulfilled obligations. I just got absorbed and it took over my whole life. You can look at the record to find out that it's quite true. It is in fact, my feeling is, it's 8 hours a day, every day, 7 days a week. I get a Guggenheim for that the sabbatical, and I didn't do anything for the whole. I fundamentally didn't do anything for the whole time. I played at doing things, I played at doing all kinds of things. If you look back, what really happened was that I sort of got myself back into work. I wasn't, as is always the case in those things, not quite aware of this until after you take the pressure off; after you take the pressure off then you find out. I suppose you actually re-interpret your whole past history. Local past history. That was a descriptor that I applied to myself. All toward the end and during the first part of the sabbatical I was sort of deeply angry.

NORBERG: Angry about what? About people, about events, about circumstances?

NEWELL: About having to do this. Angry about having to do this. I had my nose held to the grindstone for over 5 years. This is against this background of I take no positive reinforcement for any of that stuff. So, I'm getting nothing positive. Of course, that's not true; it's a damn lie.

NORBERG: But it's perception we're talking about.

NEWELL: It's deep feeling. Therefore, I'm doing this all out of obligation. It turns out later, and I can't tell when it turns over quite on the institutional side, I now have a lot of great feelings about this kind of environment and there's ways in which I count that as one of the things that I'm partially responsible for and I think that's a good thing. And I now turn out to be deeply involved in my graduate students. Deeply, deeply involved in my graduate students. I've got a whole pack of them -- nine or ten. I spend huge amounts of time with them. I spend time with them that relates to a very positive thing on my part about how to grow these guys to be great scientists. So there's lots of metatalk and all that stuff. Back in the days of Bob Balzer that just didn't happen. There's some lies in there, too. I mean, it turns out that I sort of said this happens with John Laird and Paul. It's true now in my life, especially since the cancer, I've got double excuses for focusing myself entirely on Soar. I do this, totally and completely. So I have these ten graduate students and I saw them once a week, but when the cancer came in and shot the time, I see them less. I saw them all once a week, as well as couple of other meetings, as well as a weekly meeting of the Soar project, as well as messages. Lots of contact, I date that from Laird and Rosenblum, but in fact, if I step back and then I observe that there's a couple of other of my graduate students in my life ? ? ? [names], and because they're coming around, two graduate students who came along in the early '70s, with whom I have continued a deep professional relationship all the way through. We actually did this whole work at Xerox PARC, which is outside your ken but you may know about it. So the story of my life can't be quite right, because there are a couple of characters, McCracken and George Robertson, but still that has changed. Part of the thing that has come along with that which wasn't true of George and Don, I just know absolutely, and I don't think it was true in terms of Tom, is that some of the things I have done in my life and thought they were worthwhile are creating graduate students who have gone on to become first-class scientists. And that's a piece of credit to take, where I wasn't doing that earlier. Well, I've taken you off to a sidelight.

NORBERG: Well, not quite, not quite. You came back from sabbatical. Had things improved in terms of funding, in terms of the department, in terms of structure?

NEWELL: No.

NORBERG: What do you remember about...?

NEWELL: There was nothing wrong with the department structure at anytime during this. These pressures were not because of the department.

NORBERG: No. That was not what I was trying to get at. In fact, there are two separate questions. Let's just stick with the funding. Had funding improved in the department?

NEWELL: Yes.

NORBERG: How did that happen?

NEWELL: I don't know. Meaning, Bob Kahn came in.

NORBERG: No, I'm thinking before Bob Kahn. I'm still thinking about the Russell period.

NEWELL: No, not until Kahn came.

NORBERG: Well, let me remind you. There's a summary of projects, funded projects for this department. 1977 to 1979, which total over 8 million dollars of support, that includes indirect costs. 8 million dollars of support. 6 million of which is coming from IPTO. 6 million. And you and Traub are the PIs.

NEWELL: Yes, that's right. Well, that's a departmental thing. Let's see, that's got the speech in it. Although, that's '78 and '79, speech should be gone.

NORBERG: Well. It doesn't say what's in it, it just says DARPA support, so I exaggerated when I said yes.

NEWELL: That was before Kahn came in? When did Kahn come in?

NORBERG: That was '79. So it summarizes the period just before Kahn. I haven't seen any proposals until 1980, with some... I can't remember what proposals were included.

NEWELL: Anyway, let me see. Let me back into this a little by talking about the other half of what I was going to say about Kahn. When Kahn comes in, there is a kind of a reversal in the availability of funds at that time. This is my perception, although I don't have a clear picture of what it actually is. This is not just Kahn's doing miracles, but it's related to inflation coming under control, and a number of these other things. I guess we actually get into the Reagan budget, I don't associate any of those things together. There was a certain set of enabling conditions for Kahn to have funds to begin to do things. That could easily have started before Kahn came in, as far as I know.

When I talk about Kahn, I don't associate this with somehow this is all Kahn. It's a kind of an era that starts, which has got to be fueled by changes in the basic availability. Just like, if we keep going back to Heilmeier to give him credit, I'm not sure why we should do this, but during those Heilmeier years, things really were aversive between ARPA and Congress and so forth. There were sort of base conditions there, that you work with, and those base conditions change, and the human agents that were involved can flow with it and do good things with it or they can't; that does depend on the human.

NORBERG: In fact, what I've noticed is that when Heilmeier goes to Congress he never talks about information processing.

NEWELL: No, I didn't know that.

NORBERG: He never talks about it. That's what rang an alarm bell. Why is he not talking about it? So, I asked him. He said if I would have talked about it with Congress they would have cut it.

NEWELL: I think the issue was $200,000 dollars a year, for a number of years for software technology program. That clearly got talked about. Now, it may not have gotten talked about in the official procedures of the Appropriations Committee. It got talked about enough for Batista to kill it.

NORBERG: But that's a little earlier. Isn't?

NEWELL: 1977. Yes. So, in fact in this big pot of money coming to CMU, there can't be any speech money. We're talking '77 to '79. Can't be any speech money. Would have had to have been that follow-on money. No way. So, what the hell was it?

NORBERG: Well, all that I was trying to get at was that three quarters of the funding for the department at that point according to this list, prepared by Traub, was coming from DARPA and only a quarter was coming from elsewhere, mostly from NSF. Now, what I want to ask you...

NEWELL: Again that issue of the percent for the department was again a invariant, I suppose at some time we were very close to 100%.

NORBERG: That's what you said for '60s anyway, that it was 100%.

NEWELL; I'm not sure it was exactly, because random things could come in.

NORBERG: Sure, sure. Virtually then. What I'm interested in asking here, is how did the rest of the members of the department react to this? Did they benefit from the 6 million dollars coming in from IPTO? Or not? Secondly, were they somehow left out of the picture, because they didn't bring in these large amounts of money?

NEWELL: No, no. The answer to that is no. Again, the IPTO funds, the DARPA funds, I never called them IPTO funds.

NORBERG: There probably DARPA funds.

NEWELL: The DARPA funds fueled the department. They are available in a funny way to the department as a whole. The department operates by a kind of oligarchy and with two kinds of principles, one of which said, whoever has a stake in a decision is going to get coopted into helping to make that decision. That was one principle. The other principle governed something called the budget committee. In the budget committee, all the stakeholders exist, which did not include the full number of full professors, and it did include a number of junior people. So, when that committee met, anybody who had bucks was sitting around the table and consequently direct negotiations could occur with respect to solving any problems because all the people with bucks were sitting around. This included, actually, some very junior people. And they weren't all faculty. Whoever it was. Although there are always limits with respect to what you can spend funds, the limits on ARPA were very much less. If you go back to the '60s, which was sort of a cultural thing, when you divide things into a 55/45, split, there are no distinctions.

NORBERG: That's true.

NEWELL: When that begins to go away, there are auditors in the world that we are aware of, so then gradually money becomes partly compartmentalized, ARPA gradually changed its contract. We had a single contract, which wasn't quite true for a while, we had actually a single contract and all other money that came in from ARPA for various things went into this. Once it went into this it was technically, actually spendable anywhere. So, we had this big umbrella and that has gradually disappeared in the sense that there is now strategic computer money, and there is other money, and there are boundaries between. A bunch of separate program managers. So that the culture that got set, and it got set early in the '60s when there were no distinctions, and only sort of gradually, very gradually, retreated, was that there was a single pot of money. It wasn't actually a single pot, because there were your NSF grants and different things. But ARPA was always much bigger, and so there was a big flywheel here that could be used. For any problem in the department, we would all sit around and solve that problem. Solve the problem of whether some character could come up with $100,000 that would help do this and that was okay. Usually you didn't decide in these meetings whether it was okay. There were these financial guys and Joe and myself would go off and worry about that. Actually, by the time I come back, Raj had taken over.

NORBERG: Joe has gone to Columbia?

NEWELL: Well, you see Joe never deals with ARPA.

NORBERG: He was a PI with you.

NEWELL: That didn't make any difference.

NORBERG: I see. Just like you and Perlis before.

NEWELL: Right. See, we agreed from day one that it was really important for Joe to be a PI. You can't have a head of a department who doesn't sit with a commanding power on the biggest chunk of funds. So we just did that. But, in fact, Joe didn't know ARPA, he wasn't from that community. So, in fact, the whole ARPA thing was delegated to me. You have to understand something about this department. We've had a bunch of department heads and I've never had any official power in the department. So, I sit to one side. I have always sat that way, and in fact, would make a terrible department head if that ever happened. It would never happen. Never would have happened. And that relationship always works. It absolutely works. It worked with Perlis; it worked with Joe; it worked with Nico; not so much with Bill (Wolfe). During parts of this as during the Perlis era, it was Perlis who did all the worrying about DARPA. During the Traub era, Traub never dealt with ARPA, never understood this, worried about other things, worried about the department. I worried about all of that. After that Raj took over. Raj was, in fact, very close to Kahn. So, when I came back I guess I didn't, I don't think I picked it up and then passed it on. I think, in fact, that during the year I was away that Raj took over and grew into that role. He carried that role until Dwayne comes up. You know him? Dwayne Adams? We have an Associate Dean of Research, Dwayne Adams; he was a deputy director of IPTO.

NORBERG: Oh that Adams. Yes, sorry. I didn't pick up the name at first, yes.

NEWELL: He has now been in this position for 5 years. Pretty amazing story. He lives in Washington. He still lives in Washington. He commutes back and forth. He seems stable. It works. It works just fine. Not right away when he came up here, but after about a year... When he first comes up here for a couple of years, there are very strong reasons why he shouldn't have too much to do with ARPA because there are conflict of interest rules. No matter what else you say, one must be sort of careful or you end up in Stanford's position. Very funny game. Everything is free and easy, but boy you really do better attend underneath at the next level to be sure you don't really screw things up because it may just hurt you at some point. So, in fact, we were acting quite clean on this. Dwayne, himself, is a Ph.D. from Stanford in Computer Science. He worked on DATAFLOW, the very early stuff on DATAFLOW SYSTEMS, although he was an Air Force Officer. He took the usual path. People flow into ARPA from the military. It's always terminal for their careers. Absolutely, they all know it now. I think every so often one thinks its not but its absolutely terminal and so then they retire out of DARPA and go off and do something else. So, then now for the last K years, and I think we would have to go back and see what K is, Dwayne has been essentially the main guy. We have now institutionalized this position, this sort of Associate Dean of Research, which we didn't have before. Raj sort of did it.

NORBERG: That's because this is now a School of Computer Science rather than a department.

NEWELL: No. School didn't make any difference.

NORBERG: Well, then how do you get a Dean, if you didn't make it a school?

NEWELL: Well, of course, it's a title change. This is a false statement, but it's very true. It made no difference to this environment when it became a department; it made no difference to this environment when it became a school.

NORBERG: Maybe, from your perspective it didn't. I don't know whether that's true.

NEWELL: It's a lie at some level, but at another level the really big things that have cast important things have not been these events. Have not been the event of becoming a school, not have been an event of becoming a department. Those have not been the big events. They are important shaping events, we've talked about some of them. It just turned out to be not those. Now, in fact, of course, once you become a school that entrains a number of things, so when you look five years and ten years down the pike, then it probably has some very large effects. As becoming a department in a field that is just getting departments is separate from somehow not being organized and now you've put together a school. The daily life, the students, the support, the teaching, everything else - zero change, when we became a department. We already had graduate students. We already had a faculty. It turns out that there was a young physicist, didn't quite have his degree, but he was a very bright guy. This was back in the '60s. We wanted to hire him as a faculty member, so we hired him as a Professor of Computation and put him in the

Computation Center, because we were not a department then. We already had a faculty; we had graduate students; we had support; we had an office with a head in it. It was actually the Computer Center office, but that didn't make a difference. So, every piece of apparatus that comes with a department we had, and therefore, they finally went through and got the approval. We had degrees. There were the Systems and Communication Science degrees, of course. It was...

DATE: 12 June 1991

TAPE 5/SIDE 1

NORBERG: What is the story behind the annual reports that the department produced?

NEWELL: It turns out we were faced with writing progress reports. There was some famous progress reports like the RLE [Radio Laboratory for Electronics] progress reports, and we said we can't stand to do that. We just absolutely can't do that. It's not our style. It's not anything. So, before this really caught up with us and we were forced into this, we decided what we would produce and do our own thing. We would do it our own way and so forth and that's what these are.

NORBERG: How do they differ from the RLE reports?

NEWELL: They are a series. A subset of people wrote essentially *Scientific American* level essays. Many of them are essays. We got more and more that way, because that's the natural tendency to be reports on research. They are, therefore, there to communicate to the larger audience, but they are technical, they are not abstracts. There was actually another piece of culture that was, the last thing we wanted in our life was to tell anybody everything we were doing. We wanted to make a selection and we did not wish to in some sense be boxed. That was joke in one sense. It was in fact real enough to actually motivate us. That wasn't the nature of the environment. It was open and you did all these different things. Not that we were concerned about accountability, that was not an issue in those days at all.

NORBERG: Is there any chance that these are either shorter versions of papers that were published elsewhere or reprints?

NEWELL: No.

NORBERG: These are new.

NEWELL: Yes. I can't swear that's true as we get on. In fact, the last one you've got there, which is the one with the clocks on it, it died after.

NORBERG: So, '80 and '81 is the last one.

NEWELL: Well, then finally when Katherine Copexis [?] comes on, we picked it up, finally, but it took five years. There was just not the push to try and make it happen every year. It was going to happen, it was going to happen. There was no decision, but it was going to happen. Other things got in the way.

NORBERG: I understand that.

NEWELL: We've maintained it for 10, 12 years.

NORBERG: Well, I have had a very interesting day talking to your two colleagues.

NEWELL: Oh, this was Raj and Bruce.

NORBERG: Correct. In fact, the hour that I spent with Bruce this morning was on the same global issues that I would like to discuss with you from a different perspective altogether. The discussions with Raj were for the most part about either his research or his involvement with DARPA or his activities here, in terms of interacting with other people on the campus and so on. So, I would like to return to the global issues, if you don't mind.

NEWELL: It's your nickel.

NORBERG: But first I want to pick up one thing from the last session, yesterday afternoon, and a second which has been in the back of my mind now for several months and I want to ask you about it. I've been asking others as well. The first is, did you participate in the advisory committee to Robert Cooper in the early 1980s?

NEWELL: Yes.

NORBERG: What did you remember about the committee?

NEWELL: Let's see. I participated in the advisory committee. This is related, quite strongly, to the issue we were talking about, about the community taking hold of the hiring of a DARPA director. Literally delegated, not officially delegated, of course, because it was only advisory. But to the place where it did the contacting; it did the interviewing and only after that whole process happened was there anything happening down at DARPA. The people who were on that committee were people from this larger committee that Cooper set up. I remember that committee meeting several times. I remember meeting with it. I don't remember a damn thing it did that was significant. But that, of course, isn't true. It was full of talk. Talk is the kind of talk that always happens. It's been happening for 30 years. It was talk about the current things and the current problems. No fundamental issues. That's not true. There were all kinds of issues. I just don't remember any of these issues. Now, let's see, hang on a moment. Let me divide this into two eras. There was an issue of creating the Strategic Computing Program. This is well before this committee you are talking about.

NORBERG: Is it? I don't know. I'm asking you.

NEWELL: Yes. There are two committees then. What I remember from the other one is that there was a community effort nurtured by Bob Kahn, very carefully nurtured by Bob Kahn. In the sense of, by carefully I don't mean manipulatively, I only mean that lots of energy, lots of talk, lots of discussion with Bob being the agent for this. And that is Bob Kahn. Then as it began to mature a little bit.

NORBERG: This is SCI? The plan for SCI?

NEWELL: Yes, The Strategic Computing Initiative. Then there was a time where Bob Cooper and Bob Kahn went around the country and they spent a day here and they spent a day at Stanford and they spent a day at MIT. I don't remember how may days. It was their schedule. The subject was essentially, it wasn't called Strategic Computer then, in fact, it was probably called Supercomputing, because these cats finally woke up to the fact that physicists owned the word supercomputing and you couldn't have Supercomputing, and so it got changed to Strategic Computing. You just recognize that the name was going to be a problem. So it was called SCI and was kept SCI and changed from Super to Strategic. That happened fairly early. So, it didn't have a name at that point, as far as I can remember. A whole days worth of discussion. A great day. I assume it was similar at the other places. You have to ask Bob Kahn or go ask Bob Cooper. I don't know if you've talked to him [Cooper]. This was, in part, the education of Bob Cooper, in part, his asking what could be done. Bob Cooper had a number of things, in my view, that he brought to the table, and the biggest one was, it's time for you cats to grow up and be big science.

NORBERG: Cooper was delivering this message?

NEWELL: Yes, yes. Again, he was a NASA type. He comes out of that world, and he see us as piddling along. The issue is that to do that you have to in fact shift the scale at which you operate. That implies that you have to go to Congress and get a program for this. There's no way in this environment, this was the environment in which we were doing this, there's no way that you can shift this just by somehow letting the title thing go higher. It will go a few percent higher, but it ain't gonna go much higher than that. This was half a positive proposal and half a warning. If one does this and one needs this to push the field and you guys are ready for this, then, in fact, you need to really get an additional pulse of funds, and that's going to change you guys a lot, because you're now going to move into this new era. As far as he was concerned, that was a good thing. I think he was saying this both as his own vision, but also in terms of... In fact, that turned out to be a major issue within IPTO after the strategic computing money came in. The period of chaos, as I always think of it, the period of chaos in DARPA starts... Its the Peter Principle all over again. It starts when you finally get this Strategic Computing money and then there are issues of you have to organize this office differently, you can't run it out of your hip pocket, and Bob Kahn's strong feelings that he can run this out of his hip pocket and additional people coming in who by misfortune, not by misplanning, it's hard to tell, turned out that it deeply politicized the place. It became much more Machiavellian and we ended up with Lynn Conway. We end up in the era of Craig Fields. Right in the very first stages of this, Cooper ups and leaves. His deputy ups and leaves too. They all do this on two weeks notice practically. So, they just gut the upper echelons and go set up this thing, and leave this whole program.

NORBERG: Set up what thing?

NEWELL: They set up a company, which I think they're still running. I don't remember the name of the company. It has a name like all the companies have. And so they just destroy the whole top level of understandings and support for this thing, which is separate from whether in fact it would have been better if they had stayed or not. We've got to divorce those two. But nevertheless, it just comes totally out of the blue. So, we all of a sudden find out, and I think it was three weeks notice, or two weeks notice.

NORBERG: But Kahn is left behind.

NEWELL: Kahn is still there.

NORBERG: Kahn is presumably a very articulate spokesman for the new program.

NEWELL: I have to remember if Kahn was left behind or not.

NORBERG: Well, he was there until '87. Or '86 rather, when Saul came in.

NEWELL: When did Cooper leave?

NORBERG: Well, I was just trying to remember that. It seems to me that it was somewhere around '83. I don't think he was there more than a couple of years.

NEWELL: Cooper was.

NORBERG: Well, he came in in '79.

NEWELL: Yes.

NORBERG: Is that right? Or is it Fosse that comes in in '79? I can't remember which one precedes the other. Cooper came in after Heilmeier. That must have been '79. And Cooper stayed until '83.

NEWELL: That's four years.

NORBERG: Right. Fosse came in and supplanted him. Kahn became head of IPTO in '79, the same year, and then he stayed on until '85 when Saul came in.

NEWELL: Yes, not '87.

NORBERG: And Saul was there from '85 until '87 as Kahn's successor, and then Jack Schwartz in '87-'89 and Barry Boehm until the present. That's the sequence. So Kahn is still there for at least a year, let's say.

NEWELL: So, Kahn is still there. There was pressure to change the organization from the sort of informal organization, or the hip pocket organization that Kahn had, to one where it was much more structured. There were sub-organizations and people in charge of sub-organizations. That fight with Cooper had been going on, not from day one, because, in fact, from the early days the big thing that was going on was the planning and shaping of all of this. After that, that was really Cooper that did that. So, in fact, if he leaves in '83 and Kahn stays on for two years, I have some historical reconstruction to do.

NORBERG: Let's just stick to the material.

NEWELL: When did Lynn Conway come?

NORBERG: I don't know. I can't answer that. But if we stick to that early period, it was sometime around '82. Well, I've seen a document written by Robert Kahn in September of '82 proposing this major program, which is put into the Presidents Review for the Budget for the following fiscal year. Fiscal 83.

NEWELL: Was that by Kahn or Cooper?

NORBERG: It was written by Kahn and in the IPTO section presented to the director of DARPA. What the director of DARPA did from there, I can't tell you, I haven't seen any of those records. That summarizes for the most part the program that he is proposing to do starting with the work in the VLSI, building on the VLSI program already in progress and adding these components for AI research in fast processors, and super computing and so on. That goes on to the Congress in that year and essentially gets funded. Now, it's my understanding it was Cooper who made the decision to split these programs across several offices within DARPA. That is, IPTO was not going to continue. It gets changed to ISTO along the way there.

NEWELL: That happens when Saul was there.

NORBERG: Doesn't that happen before Saul comes?

NEWELL: No.

NORBERG: Saul told me there were two pots of money. He tried to put the two pots of money put back together again as a condition of his coming. He didn't succeed.

NEWELL: He did not succeed. The story is not so linear, which is to say, there is an immense amount of to-ing and fro-ing with respect to how to organize. There certainly was this decision that the money was not going to IPTO, but there was no issue early on that IPTO was going to change organizationally. IPTO was not going to control all the money.

NORBERG: I see. Okay.

NEWELL: In fact, the IPTO/ISTO thing happened quite late in the game after for instance the creation of... (oh, names). I recollect three stages of this organizational affair. The first one is there is just the same organization. There is no reorganization except that there is now a special assistant or something like this off the director's office for this program. There always was that. Money goes to IPTO. Chunks of money go to TTO and STO and DAS.

NORBERG: EAO.

NEWELL; No, EAO is the Engineering Applications? That didn't exist.

NORBERG: Okay, that comes later.

NEWELL: That's the second phase. So, the first phase is no organizational change, but money is spread around, not all the money under IPTO control. Throughout this period, and I don't have a clear vision, because I never have any view of what money goes to TTO and so forth -- there all kinds of things that I'm not wired into. And if it isn't wired in, I've forgotten you. It doesn't make that much difference. It's just all the same story, sometimes better, sometimes not.

NORBERG: It's going to make a difference in just a few minutes with the next question that I'm going to ask you.

NEWELL: I was involved in a lot of this. There were conversations and discussions of various kinds, a few net messages and so forth. And so, I think of myself as playing an absolutely peripheral role, in one sense, but certainly am not excluded from odd pieces of this, in the sense that whenever there is opportunity there will be a discussion about these issues. I've engaged in innumerable discussions all through this period. They've all washed away. Maybe that's not true. If we drudge them, we'll find some of them. Of course, I haven't found that twenty-seven page memo from Lick yet!

NORBERG: Well, I have it. I'll send you a copy.

NEWELL: Anyway, at one stage of the game they created EAO. There is a pre-decision that happens, which is Cooper's decision and not Kahn's, which is for Congress, you gotta lay some applications on the line. This creates these three applications. You know why there are three - the Army, the Navy and the Air Force.

NORBERG: Yes, right.

NEWELL: The Marines never count in this kind of thing. The community created those.

NORBERG: The community? Your community, computer science?

NEWELL: Yes. Not totally, but, in fact, there were some intense 12-hour sessions down at DARPA where characters were called in from the community to define this one or this one or this one. There had to be three. There were clearly some guidelines and some ideas and so forth, but the issue of trying to put some flesh on that was up to, not a committee, but a bunch of people pulled in, but it was a committee in some sense, pulled in to go lay flesh on that, to create in a hurry this strategic computing document, to give it some flesh. Although they were constrained, they also had some freedom. They were better ideas, but they just had to be these kinds of things. That created within the whole thing a level of technology -- they were called some technologies, technology things, with these three things to focus it. That was done over Bob Kahn's objection, I'm sure he's told you that, if you've talked to him about it. Cooper insisted that that was politically required. That was the kind of thing you could talk to Congress about. I'm sure that I've heard that from Cooper a couple of times. It makes perfectly good sense.

NORBERG: Sure. And that would be consistent with the previous 10 years history and what sorts of things are talked about when they go up on the Hill as opposed as to what is actually being done in the laboratories and universities. But what I find a little puzzling about this whole thing, Al, is that your statement that the community participated in the definition of these three application areas puzzles me. Who in the community knew about battle management programs? Or doesn't that matter?

NEWELL: There were undoubtedly a number of people from the community and that includes the DARPA community who knew something about it. But its also a question you had to talk about AI in this. This was a community that knew about AI.

NORBERG: Well, I could see the participation in AI discussions, but when somebody comes along and says I want to do a battle management program and it has the following sets of overall characteristics that the Navy can define, then the people in the community can say that yes, we could probably mechanize this and we could probably mechanize that.

NEWELL: Maybe I'm wrong about this, but I just don't think I am.

NORBERG: That's all right. I just want to make sure I understand the point.

NEWELL: It was not in one sense the customer who laid out a set of requirements and then went to the AI people to see if they could meet them. A set of people who were familiar with the military and deeply involved in it sort of came up with the kinds of things, battle management and so forth, that could do and then the issue of fleshing that out and making AI a part of it, is something the community could do.

NORBERG: Sure.

NEWELL: This was because it was the community that was proposing. The community was not about to build, I mean, I don't want to say it that way as if there was ever a choice, because this relates again to ARPA's whole view of how to build this. You had to put this in, but this was still an ARPA program and therefore not one for which you got a bunch of customers. Now, ARPA goes and tries to find customers for all kinds of things all the time. The idea of getting a bunch of people in who sort of now control that program because it was to be viewed as meeting their specifications, that never was there. That just never was there. Again, how many informal discussions with how many Colonels and how many Admirals and how many Captains, I have no idea. I think there were lots, in order to hone in on what were the sort of things... But if you go look at what's in that document the material in the Strategic Computing thing on these things I think is largely written by these committees. Quasi-committees. Now, some of that was. We only got four days before we have to get this thing in. We don't have any of this done. So some of this is ARPA simply reaching out and commanding from the community, which the community was at one level quite willing to give because in one sense this is the community's program. It's your program. It's your thing. This is what's going to provide the bucks to do all of these things, so this is not forcing. The general question of do you wish to do this, which goes back to Cooper's kind of thing, was answered silently and affirmatively as one went along. Wrong word I suppose, but the community was co-opted. How could it be co-opted? In one sense, there are the funds for this community to live and prosper on. The community couldn't understand and didn't understand, because in fact Cooper and Kahn didn't understand it, was what were going to be the kind of conditions that this would put on you. You could say some of them like you're moving up a level and this is going to be more bureaucratic than you can say in a number of these things, but who knows what their real semantics is for you.

NORBERG: Okay. I'll come back to that in a minute. I want to keep on with...

NEWELL: Can I say a word before you do.

NORBERG: Yes, go ahead.

NEWELL: This set the stage for leaving IPTO and creating an EAO which then controlled these... So the functional division had been created back there in these three applications and provided a natural... Now, they are separated. And now when it comes time, because it just isn't working, to try to find some other organization that puts them back together, then, they said we can't put them back together and just call this IPTO, for silly reasons, for silly reasons, so we'll give this a new name so that both of these go out of existence.

NORBERG: I want to raise an objection, I want to make believe that I'm sitting here as Robert Kahn. I'm going to say to you that I don't remember the story that way at all. I don't remember the community participating in that. I did this. I developed this proposal, because I saw the need to upgrade the IPTO program in 1979 when I came in as head of the office, and the only way I was going to get funds to support the basic research activity in universities was to make sure I put an application component on this. So, I went ahead and developed this...

NEWELL: I don't believe it. I believe the following. I believe half of it. The half of it I believe is that Bob Kahn says I did it. And I believe that Bob Kahn believes he did it and I believe he's right, meaning that I don't believe that Cooper initiated this. I believe that Kahn thought this up. I believe things like the tour, nine day tour or something. Kahn and I talked about it casually, talked about it when he was here, about all the strategy that one has to proceed to do this and educating Cooper and so forth. This was part of Kahn's plan of what it took. If the issue is that Kahn says I did it, then he's right, but he didn't do all of it. He got some of us to go help, and furthermore, Cooper is not a patsy. In an organizational structure like this if this guy who is part of your organization begins to generate something interesting and you see this also through lots of conversations as a great thing to do then you join up and join in and it now becomes a team of two of you. From Kahn's point of view he may have just co-opted you. From your point of view, you are now doing your thing and you are making your own input. Now, it is my recollection -- this is why I said wrong, it is my recollection that it was Cooper who insisted at some stage of this process that there had to be those applications and that Kahn would have placed his bets on his ability to somehow get a program through that was much more amorphous in its character, and therefore, have much more freedom on the inside. It did not involve the pre-commitment on the congressional level for particular application areas and that Kahn actually watered down these applications to where they weren't applications, they were not customers, they were only generic customers. They weren't actual customers who would get in and then control it, and control it by insisting that they had the right to say what aspects of these specs were important and when push came to shove they had the right to shear things off. So they were not generic technologies, but they were generic applications. They were applications areas. I believe -- and this is quite remote, in terms of this being hearsay from me, I have the feeling that, in one sense, Cooper was on the more hardened application side and that Kahn would not have done it that way at all and in fact succeeded in watering it.

NORBERG: Would Kahn have been able to achieve 225 million a year his way?

NEWELL: Cooper says no. I'm not sure how strongly Kahn said yes, actually. I'm not sure how confident Kahn was. It's just that that's what he wanted. I think that would have been a very tough thing to do. I don't have a good enough feel for Congress, that's not my world. I don't know.

NORBERG: We've heard many comments. I'm on to my second point. I'll come back to SCI in a moment to round this out. We've heard many comments about the existence of a DARPA contractor community. Emphasis on community here.

NEWELL: Yes.

NORBERG: What's your view of the make-up of this community, especially for the early years? Was there such a thing? You might even define for me what the DARPA community is as you understand it.

NEWELL: I'm thinking back into this a little bit. I'm not sure about how much of a community I would have said had existed in the '60s. What I would have said and will say now, and I'm trying to get back to how I would have perceived it, is that in fact there were infinitely close working relationships with a few places, of which again MIT and Stanford and CMU were sort...

TAPE 5/SIDE 2

NORBERG: MIT and Stanford were the key ones.

NEWELL: Right. This was defined pretty much because Lick came out of MIT. Stanford came out of MIT, which is to say that John McCarthy came from MIT. The crazy thing that happened at Stanford was that McCarthy went out to Stanford and created the AI lab out there, which was independent of the Computer Science Department. Worst thing that could have ever happened to them. I mean, absolute organizational disaster, because it was a rich, in those days a really rich, AI lab that did it's own thing and as far as I could ever see not a single penny ever floated to the rest of the department. That was partly because they shoved it all out in the power lab out into the boonies. It had all kinds of positive effects, if you believe all these cultural growths were positive effects. Therefore, the cooperation between Stanford and MIT is very close. Decisions on machines and all kinds of things. They form a close-knit community, because John McCarthy came out of there. He and Marvin were in one sense a duo to try and make artificial intelligence happen. And so, the AI lab phenomenon occurs not here at all, but at those two places, and is joined then by the fourth one which is SRI, which is sort of an off-shoot of the Stanford situation. It's not really, because Charley Rosen, who created it, is a different kind of guy. I mean, comes from a different kind of place but the whole early years is total struggle on the part of Charley to make anything happen, to keep anything going, get any funding from anybody. I think he had a little AFOSR funding, nothing major. Gradually with the Stanford AI Lab there, things move in the direction of being another AI lab. Charley, is I believe, the first head of that, and then Nils goes over there, and then you get graduate students, you get Cordell Green and a number of other people. Jeff R... and so it becomes another AI lab in the image of Stanford, I mean, not quite, but fundamentally. Now, there are four AI places, and three AI labs. We are not part of that growth at all. We are not an AI lab. We don't have that culture. We never do get that culture. That whole thing kind of becomes imported again with the ARPANET and with Raj. Raj and the ARPANET come together. You can't unconfound those two variables. When Raj arrives here, he brings with him a couple of graduate students from Stanford. They use SAIL; they bring SAIL with them. They live on the network in terms of systems work on SAIL goes on out there, but some of it goes on here and they use it out there, but now we are talking about the ARPANET. There's a watershed here, which does relate to the ARPA community as defined by the ARPANET, which builds up the communicating people who don't just communicate with messages. There's a lot of flow of programs, and so forth, common systems and all this sort of stuff. So that systems that get created at one place get used at another, and actually the culture of free system use, which is now represented by Stahlman (?), in the open (free) Software Foundation. The symbol of this is the fact that for K years, you have to say what K is, K is maybe 5 years or 6, no passwords were required on the MIT machine. Anyone could go into the MIT system from anywhere in the world and get access to the operating system kernel. There was no barrier anywhere in that system. Marvin, for instance, is inordinately proud of the fact his frames paper existed on this machine and was stolen by all kinds of people all around the country that he had the foggiest notion about. They came in, found it, read it, took it away. He didn't know. The whole imagery of free distribution and so forth. Finally, the security problem got bad enough that ARPA forced them to put passwords. There were no passwords and no inter things at all on the first PDP-6, whatever, I can't remember. That is, in fact, and that kind of community is generated by the ARPANET. It's not only the ARPANET, if you would have taken a bunch of completely separate places, we know this about networking in which in general people who do not know each other do not become a strong network. You take this community, which had, in fact, lots of personal relationships and computer relationships and then you produce the communication and you get this effect. I've got to go back to the '60s and talk more about this. This is not done by a great Whoopee by the community. As I told you, Larry Roberts, at least my image of Larry is his lecturing us like a Dutch uncle about, I mean, he wanted to say, this an interpretation, he wanted to say look, I need you guys, if you guys don't get in and help me to learn how to use this network then this whole thing is going to fail. But he couldn't bring himself to do that because he took the view that he couldn't push the AI labs around. They had their own money given to them. He wasn't providing extra money for any of this. Consequently, the growth of the use didn't happen by a kind of administrative means. It didn't take very long for it to happen. It was, in fact, a much more natural occurrence in this community, because the various people, the various significant people in the community, did not instantly turn and begin to devote all of their time, unlike time-sharing, in which there's a piece of mythology, which I think is true, that AI took a hit in the '60s, in the early '60s, because a number of its significant people devoted their time not to AI but to time-sharing -- John McCarthy in particular. But what was going on at MIT, when they should have been doing AI -- should have been doing AI; of course they were -- was concerned with all sorts of time-sharing, and tablets, and communications and so forth. I don't mean network communications, but how the humans were going to act. John McCarthy's great claim to fame is that he has had a kind of a correct perception of what you ought to worry about about 5 times in his life. He had it for time-sharing too. The AI argument was we aren't going to be able to do AI unless we finally get so we have this sort of communication. Therefore, it is really worth it for me, John McCarthy, to go off and devote big chunks of my life to do it, which he did. He did it at BBN -- a PDP-1 timesharing system. So, my model was there was actually there were a fair number of people in the community who attended to those issues and did not attend to AI for a couple years. I call it myth because it's not actually clear to me that we could measure it. It could be just kind of a perceptual thing. But certainly some of the people did. I suppose that happened with networking, but certainly there was a kind of a reluctance around in which McCarthy equivalents didn't stand up seeing the vision of this thing even though Larry Roberts was there preaching the vision at them and say wow, I'm going to go off and do this. It happened naturally enough, but it didn't turn immediately. So much of the issue of the community arrives with the ARPANET, so there is, as I have said in answering a number of your questions, 1970 is a kind of a key issue. It is one kind of community afterwards and another thing before. I think of it as all tied together, because even though I'm not part of, I'm some kind of part of it, but because CMU is allowed to be part of it and go its way, but it doesn't get the same kind of machines. Stanford and MIT get the same kind of machine. Sitting in the background is BBN. BBN is a service company to DARPA and I don't know when that starts because that's all in the mist of the Charles River. Those guys go up there and they do their thing in their own way and there is a Charles River mafia. That's all sitting behind us, so it's inconceivable that Lick would go down to ARPA and create this thing and not have a big piece of this. That's not a bias. MIT is one of the great technical institutions in the world. It's not just another institution; it really is that kind of thing. That's what the mythology says and there's plenty of evidence that indicates that that is right. The issue is you go down, and whatever game you play you end up doing a lot, except in this case, of course, it's supported by the fact that these guys go over to each other's houses in the evening and talk. They all know each other. They all know each other at BBN. The conversion of BBN from an acoustics place, which it was, but you know Lick was a vice president of BBN. The conversion of BBN comes along naturally at this sort of level. So these guys all know each other. McCarthy is part of that. He isn't part of it originally, a young student at Princeton, and I'm not sure how much a part of it he is at Dartmouth, when he's a young faculty member. But by the time he goes down to MIT and he and Marvin joined forces, then in fact this begins to generate. Stanford becomes a pseudo-pod of MIT. We're not part of that. But we're not out of it either, I mean, we're not isolated. We're just allowed to go our own way and we have our own independent sources of inspiration; organizational theory, cognition, management science, I mean, all of these things which are not the world that these guys live in. We are not tied in with MIT at all in a real way. It doesn't mean we don't know some of these guys and that we don't talk to them. The bond that exists in the early days is the bond of people starting a field. All of them. So, I'm not talking about Marvin and Oliver. One of my standing historical gripes is that there really were five founders of AI. Oliver Selfridge is one of them. He dropped out and there is a story there. It is a very personal story in terms of his own life as well as his own style. He's a gadfly and not a builder, so to speak. In the early days, Oliver was every bit as central an intellectual person as was Marvin, as was myself, and John. Herb's a little different. All these guys are exactly the same age. Herb is a little older, and himself a man immense substance with a whole world of connections. And Perlis is the same way. Although, I've got to be careful. Perlis was from MIT. Perlis was an MIT Ph.D., I believe. I always have trouble remembering what he is. I think he is an MIT graduate. He knows all of those guys. He roomed with Ed David up there. So there is a level of communication, except that Perlis is focused on Computer Science. See, in again, this place is not an AI place. This place is a computer science place. And AI is a piece of it, but it's always a piece of the whole thing and the focus of the department is never on AI. The focus of the department is always on computer science of which AI is a part. So, we do in fact go our own way. The community, as it exists, as I see it, is sort of over there, with us sort of here and not strongly tied in and not isolated either.

NORBERG: Yes, but what you're talking about now is the AI community and the question I asked was the DARPA community.

NEWELL: IPTO community. Because there has never been a DARPA community.

NORBERG: I think that's what people mean when they're talking to me.

NEWELL: Yes, they do mean that.

NORBERG: So, what is the IPTO community before the network?

NEWELL: I don't know and I don't know it very well. Now, you have to remember that BBN is not in fact part of the AI community. I mean it is part of the infrastructure. Now, an infrastructure thing which comes into its own as the network, but it starts well before the network. The other part of this is ISI, which was the West Coast version which doesn't start until, what did we say?

NORBERG: '71.

NEWELL: '71. So that happens after that. After this other set of patterns. I can't remember. When were the first PI meetings:

NORBERG: Well, the first one we know about definitely is '67. There was probably one in '66, but not before that.

NEWELL: Yea, I wouldn't think so.

NORBERG: The one in '67 is the one in which the network subject came up and that's why it sort of stands out.

NEWELL: I probably didn't attend that one, because it didn't seem to me important. The fact that those bastards wanted to make all the PIs come to one place seemed kind of silly to me.

NORBERG: Well, I've made a kind of issue of that, but I won't.

NEWELL: It's a funny business. If there is a community, and we can play this out with the SOAR community if you want, if there is a community, then when the community gets together, besides to all meet at some place, there can be key decisions. But mostly it is simply a continuation of this community continuing to communicate with each other. In fact, for the members, except when there turns out to be a few decisions, if you are the Mafia and you finally decide to let a contract out on some character or something like this, there's nothing that distinguishes that meeting except you were all there, you all went to lunch together, you all had a lot of talk, there were a lot of things that happened. You were talking to the same people you talked to... Not always, there were always some new people. In fact, there were lots of people there who were not AI people. It does have some of that. As opposed to the Dartmouth thing, which is a piece of AI mythology, but it was in fact the case that it was the first time, although it never came close to John McCarthy's expectations -- you talked to John about this?

NORBERG: Yes.

NEWELL: Nevertheless, they got a bunch of people together and created something, something of a sense of identity. So that played a quite different role than the ARPA PI meetings which turn out to be reinforcement devices to a community thing which is actually supported by the contracting. The contracting thing and then the Charles River mafia.

NORBERG: But I would like to say, Al, that in the late 1960s, '67 let's say through say 1970 or so, that at the PI meetings when everybody was giving their summaries of progress over the past year that basically everybody was addressing the same problem. Now, that's a strong statement to make. Well, what I mean is that we've got a problem with representation and the AI people want to look at representation in one way and the people who are developing hardware and want to develop a new architecture to handle that representation say in a new LISP machine, which comes along eventually, are going to look at that same problem but in a different way, from the architectural point of view. You've got data structures people who are interested in how people are going to take the data in that representation.

NEWELL: It seems to me to be a false picture, which is to say that, in fact, this community was dealing with the range of problems in computer science and a lot of the programming people were not very concerned with AI. The divisions which exist in the computer science world or in the forming computer science world, existed then still exist, which is on the part of the programming community a rather deep suspicion on whether AI is real and so on, and ought to be taken seriously and so on, was certainly evident there as well. There is always a very funny issue here for the field as a whole, which also was in my view replicated within the ARPA community in which the AI people are high status people. They contain some of the highest status people, yet the programming types remain deeply suspicious about whether AI ought to be a part. They'd kind of like to kick the whole part out.

NORBERG: Well, does this say then that the IPTO community is just a subset of what we called the Computer Science community? And the only thing that distinguishes them is the fact that they have money from DARPA.

NEWELL: The IPTO community would say it the other way. They were the computer science community.

NORBERG: But that's not fair, is it?

NEWELL: Now we'll make some cuts in this. There is the supercomputing community. The LARC and Stretch. There is the DOE, the atomic energy building of high performance computers, which is controlled by the physicists and shows no sign, ever, even unto this day, of becoming a part of the computer science community. It is now called computing science rather than computer science. It is controlled by the applications people. And it's controlled under a piece of hubris, which is on the part of the physicists, there isn't anything to be known about that that they themselves don't know. I was going to ask you, are you by any chance a physicist by training?

NORBERG: Yes. That's why I'm laughing. You're absolutely right.

NEWELL: My bachelors is in physics.

NORBERG: I have two degrees in physics and then I went back to graduate school in history.

NEWELL: I identify myself as a physicist, so I know exactly where those guys come from.

NORBERG: So the IPTO people were...

NEWELL: So, this was a separate community, but this was not computer science. They existed all the way back. The coming of a real computer science community where there was concern with programming, programming constructs, AI and representation, all of these things, and then communication, mythology says, and so some of us say it with the mythology, was that, in fact, my standard view is, DARPA created computer science, meaning this institutional thing and therefore the collection of these characters. The collection of these characters, who represent -- by the way, it was not strong on architecture in the beginning. Architecture comes much later because there was a deep suspicion early on everywhere about the ability of anyone except people in industry to build hardware. They'd sort of been through this, had watched the failures of this in the university. One of the reasons why Larry was as pissed off as he was about our building this computer was that what right did this university have to go down this path which was clearly a loser. Universities could not build machines. That's a fallacy. And then there was a particular point when ARPA denied it's history and took on ILLIAC. They poured 50 million bucks down a rat hole. But, in some sense, they did that through a series of rational arguments. It was appropriate, but it was not in their nature. The doing of it was in their nature, I mean, you should just go off and do things like that, but it was the wrong decision made out of the wrong set of instincts. I guess both Ivan and Larry had something to do with that.

NORBERG: Oh, yes. It starts in '65. Incidently, just to complete that thought, is it possible that the perceived the failure with ILLIAC-IV was what raised Robert's ire about C.MMP?

NEWELL: Oh, absolutely. There is both the general consensus around the whole field that only industry can build machines. When universities do it they take a hell of a long time; they aren't worth anything; you can't get them up before industry has gone past you and so forth. Finally, there is this move on the part of ARPA to get into the architecture game with ILLIAC-IV. This goes back to the BBC machine we were talking about last night. To buy into an existing machine and somebody else's story, Dan Slotnik's story. And that fails. An expensive failure. Not that there aren't some side effects that come out of it; it was still an expensive failure. So that now confirms the view that ARPA has taken its fling, has tried to do it's own thing that looked like they had a shot. It didn't work, and, therefore, they are real unhappy about -- I don't mean anybody else in a turf sense -- but about other things that go beyond that, I mean, that architectures were, in fact, not to be built. No doubt about it.

NORBERG: Why, then, in the 1980s did they go again against the grain and start thinking about architectures again? The Butterfly Warp, Thinking Machines...

NEWELL: Because the technology changes. I mean, the world is not the same. This is in fact Gordon Bell's thing, because he said you can't -- I don't remember if he actually said this -- you shouldn't go build this with PDP-10s, but you can build it with PDP-11s and its right and its worth it and we're in that part of the technology space where we can do that. And so in fact there has been some views in which you go through an era and you can build now architectures in the universities and then you can pass out of that era and then you can't do it again for awhile. It is true of other technologies as well. There are inventions that change this. This is the Lynn Conway thing. We struggled, we were just one of several, but I can remember a lot of struggling here about how to deal with VLSI. How to deal with all of this stuff. Having sessions over here where we get guys up from industry and we really sort of go after this about what good things can be done, and what it would it take to go produce special purpose machines, all this kind of stuff. And the upshot was always the same. These guys would say, "Sure, this would be all kind of possible, but we're the only people that can do it, we the people who have the facilities. But you couldn't pay us to do it, literally. It's going to cost a million dollars. If you provided us with a million dollars, we would return it to you because it isn't worth it for us to spend that million dollars for you when we could spend that million dollars for us and make 20 million dollars on it."

NORBERG: Okay, so the market's driving their decision.

NEWELL: Yes. "All you do is rob us of the opportunity costs, so there's no way you can pay us to work on this, and we own this." I mean, this wasn't sort of a legal statement, but, "We are the only..." And there was immense frustration in this community when we cast around -- guys like Raj and so forth -- and when we cast around for how to move, I mean, the opportunities that were sort of obviously apparent. And then, MOSIS... MOSIS is second, but the whole issue of trying to find a way, and then to produce. Things like MOSIS are community mechanisms, as well.

NORBERG: Yes.

NEWELL: It came along much later after the whole...

NORBERG: But let's not lose this notion of community. Get back to this definition, because you just said that you believed that from IPTO's point of view the computer science community in the 1960s was the IPTO community. There wasn't anybody outside of that. But that leaves out a whole range of people in industry, for example.

NEWELL: Right, who were not trying to make a computer science field. Almost none of them were. There is the creation of computer science, not just computers. And one of the peculiarities in the whole history of this thing is - well, you ought to know better than anybody else - is we start out with programming... Let's go back to day one. We start out with programming being something that little girls do. Then we get that programming is something that mathematicians do, but they don't make any science out of the program. The only thing they do is numerical analysis, and then there is some programming. The mathematicians never get an image of computer science. The engineers never get an image of computer science. And it would never occur to them to. And so there isn't anybody building a science or envisioning a science of computer science. And that's what goes on in this community.

NORBERG: What distinguishes this community, this IPTO community, in the 1980s from the rest of computer science? Because now there is a much larger context that call themselves computer scientists.

NEWELL: There sure is. I am not sure that there exists an ARPA community in the 1980s in the sense that I believe that there are 250 ARPA contractors that are spread all over hell and gone.

NORBERG: Both geographically and topically?

NEWELL: Yes. They don't know me and I don't know them, and they cover a whole range of things up and down the range of computer technology, computer science. They cover how to get seismic waves over from Norway - things like this, you know. And they're dealing with the geophysical community. And so, in fact, the ARPA community has grown too big to be an ARPA community. That does not mean that there are not some ARPA communities which represent the...

NORBERG: Yes, I see.

NEWELL: I can't guarantee this is right. I only have the sensation that there are all these other characters out there and I don't know them and they don't know me, and there isn't necessarily any reason for us to know each other. It is certainly not the case that the ARPA community, I mean, the ARPA community now as a somewhat narrower cultural thing that comes up historically, owns and controls computer science. Computer science is its own thing now, in all kinds of ways. If you want to take measurements on this go look at how many people from the ARPA community are on the curriculum committees that the professional societies create. Ask how many people from the ARPA community are presidents of the ACM. And the answer is, "Once upon a time; not now." And that's exactly what's appropriate, you know. The control of the field finally goes to the department - an absolutely true statement of all sciences that gradually go to the departments - backed up a little bit by the professional societies.

TAPE 6/SIDE 1

NEWELL: It is the case that the premier computer science departments are still the ones that are DARPA-supported, and that the number 4 department became strongly DARPA-supported.

NORBERG: Number 4?

NEWELL: UC Berkeley. I'm not sure. I mean, who knows about these rankings? All I am saying is, you rank the departments, and there is Stanford and MIT and CMU still, as far as I can see, very solidly in there, in the saddle with respect to their excellence and so forth.

NORBERG: How does one define excellence in this case?

NEWELL: I don't know; you wait until *Business Week* ranks them. [laughter]

NORBERG: They only do that for management schools. [laugh]

NEWELL: You wait until the National Academy does a ranking of departments -- I don't know. How do you define excellence? The answer is you define excellence fundamentally by who is producing the great results that push computer science. So you define it retrospectively by going back... I mean, that's not the way it's usually defined. It's usually defined by reputation measures. So you sort of say, "You can rank departments," or, "You can't rank computer science departments, but I can because I live here. So therefore, I should rank all the other departments except CMU, and so we can just play the ranking game." But when you try and ask what's that based on the fundamental issue is those places -- and there's sort of two or three measures -- those places which produce the great scientific results, which become part of the living field are the great places. And the secondary measure is those scientists who produce those things are the great scientists and the departments where they live are the great departments. And the third measure is, where do the graduate students come from? So if this place turns out all the great graduates... Why is he a great graduate student? He's a great graduate student because he goes to some place and produces great results. I mean, there isn't any question about this. It's perfect standard. It's just hard to measure. It's not too hard to measure looking backwards. It's not at all hard to measure what the great results of the 1920s were, the chaff falls away.

NORBERG: We may be too soon to do that for computer science.

NEWELL: Well, no. Why don't we ask on that same scale whether the ARPA community has done things. Then you put into the scale things like the ARPANET, because the ARPANET is not just a piece of technology. The ARPANET is a concept of packet switching. And it's not terribly... It's a little important from the standpoint of the historian. It's not terribly important that the actual issue of where the packets first came from turned out to be these guys over in England. That's true, but it was all early enough so that from a science historical point of view you have got to lay the development of that whole technology and the whole conceptual structure that comes from packet switching at the doors of ARPA and its millions. Insofar as you believe that there's anything significant about AI, it all happens out of the ARPA community. But that's not the only kind of things that happened. The graphics... timesharing. So you count these up, and that's the same game you're playing. I mean, it's exactly the same game. And you say, "I don't have to wait 50 years to know that packet switching and the ARPANET were important. I don't have to wait 50 years..." Maybe you do have to wait 50 years to know whether expert systems are important, but probably not. So there is, in fact, a lot of variability in the tracking measure, as any historian will tell you.

NORBERG: Yes.

NEWELL: But not completely. Not enough so you have to say, "You can't tell it all until 50 years later." When Napoleon was defeated at Waterloo you didn't have to wait 100 years to know it was a significant change.

NORBERG: Well taken; I accept the point. Let me go back then to SCI and the sense of this DARPA community if it existed in the 1980s. What changes were produced in computer science, so far as we can appreciate them at this stage, due to the funding that came from SCI? Anything discernable yet?

NEWELL: Well, you see, I don't separate the funding from SCI from anything else. So my view of the SCI funding is it provides part of this pool that this community lives off of. If you are a historian of SCI, which you are a little bit at the moment, then of course you are very interested in trying to play some of this tracking. So, for instance, around here you can sort of say, "Well, it turns out I actually sort of know. The big things that are SCI supported are all of Coons' work and all of Mach. Mach is a creature of SCI, sort of." I mean, it really is. It wasn't in its first incarnation; it was just out of the ARPA stuff. And the decisions about whether you can support stuff out of SCI and so forth is a kind of a quasi-political decision. So it doesn't have a sharp boundary as far as I am concerned.

NORBERG: Then why raise the point that Cooper says that if we go for this and we get it, then we're going to change you guys into something different?

NEWELL: Because the scale goes up. The scale is the sum. So you are being supported at... And actually, you were sort of technically wrong, which is to say the scale was about a couple of hundred million a year and now the scale is like 400 million a year, and that's only a factor of two. And if you...

NORBERG: Oh, but it went up from 50 million to 225.

NEWELL: No, sir, the field was not supported on only 50 million.

NORBERG: Well, that's my impression.

NEWELL: Well, my impression... Okay, you might be right, and if that's the case...

NORBERG: The budget in 1980 was 50 million for IPTO. Now there may have been some other money coming out of various other places like VLSI, but...

NEWELL: No, no. Not much. So it's 50 to 250. That's big enough for Cooper to be right. I had a feeling that the actual ratios were sort of getting...

NORBERG: Then it goes from 200 to 400, but now it's back down to something more reasonable -- 180.

NEWELL: Well, the SCI's over. Now we are waiting for the high performance computing. That's the next device. ARPA played a significant role in the Michigan net, as you undoubtedly know.

NORBERG: According to Saul.

NEWELL: Well, Saul has conned you about that, right?

NORBERG: [laugh] Well, he only talked about the discussions he participated in.

NEWELL: Right. In my view he was instrumental in getting those original FCSST meetings going, and so forth. But, of course, that doesn't mean a bunch of other people weren't instrumental too. So Cooper's statement is actually based on the total bucks in the field, not the SCI bucks. The SCI bucks were the thing that pushed you up, and not on the fact that SCI was a separated thing. But the fact that it was separated in various ways and the fact that it had these applications in it was all tactics. Fundamentally what was happening to the field was that it was now growing by a factor of five according to...

NORBERG: But isn't it true that a lot of the increase above 50 million went to industrial firms, which participated in SCI?

NEWELL: Right, and insofar as that happened then you don't expect this other transformation to occur. And it was not given that that would happen. I mean, it was certainly given that some of that would happen, but when Cooper made these remarks he's talking out of this other view, absolutely. And the fact that it didn't happen... In fact, actually it has make a big difference. There are lots of things, lots of pretty fundamental research. One of the problems with computer science is you can't distinguish research from development from application very well. And that's a character of the field. It's not a character of the guys who conceive of the field. It comes out of its inner nature, which is to say that everything in computer science is related to use. You think you're working on basic algorithms, you know, and as soon as you get something that's significant, it's significant in part because it relates to computing, which is how things can get done. So everybody in this game... There aren't any pure computer scientists who can make the statement that my physics professors made, which is, "If I can smell..." I had a professor by the name of Webster, who worked in x-rays.

NORBERG: David?

NEWELL: Yes, David Webster. He wasn't my professor, but he was a great friend of the family's. And when I was down at Stanford -- I used to go to Stanford -- I used to wander up and talk with him. And on several different occasions he would say, "If I could smell any possibility of this being useful, I would not do it." [laughter] That was his touchstone, and he was not...

NORBERG: He supported the klystron work though. [laugh]

NEWELL: Yes. He was kind of a Victorian in many ways. Did you know him?

NEWELL: No, but when I was out at Berkeley I interviewed a number of people who worked with him. So we talked a lot about David Webster.

NEWELL: [laugh] You see, my father was a professor of radiology at Stanford, and he and Webster and Kirkpatrick and all of those guys worked together. Well, I actually worked with Paul. When I went down to Stanford, Paul had an idea about how to do x-ray microscopy, and as an undergraduate, sophomore, freshman undergraduate, I got somehow associated with that, and spent my whole undergraduate years designing x-ray microscopes. It worked out very well, but that was... Along with Joan Baez's father.

NORBERG: Yes, I do know Al.

NEWELL: You do know Al? Where is he?

NORBERG: Well, he was at Berkeley when I was there and we got very friendly with him.

NEWELL: Right, but he was back as a graduate student getting his Ph.D. having taught high school physics in Illinois and now was back. So Al was a graduate student. I somehow came on this project as an undergraduate. I don't think I ever got paid for it.

NORBERG: Quite customary in those days. But let's go back to computer science here.

NEWELL: Right, sorry.

NORBERG: No, that's all right. You just raised a very interesting notion, which is troubling me, and that's the lack of a separation or lack of distinction among basic research, development and application in computer science. One of the arguments that I am trying to make in writing this history in terms of the program now, the IPTO program, as various directors devised it, is that in roughly the 1960s -- these are not determined by actual decadal dates -- but in the 1960s, there is a focus on fundamentals in this field. And that's what I have called it, a focus on fundamentals. I didn't talk about basic research or anything like that. But as we slide into the future, as we move into the 1970s and definitely into the 1980s, there is an increasing emphasis on applications.

NEWELL: You're absolutely right. No, you're absolutely right. And that's from the point of view of society, and by society I include things like Congress and things like the environment of DARPA and things like DARPA itself, and things like IPTO itself, but DARPA is bigger than IPTO, and exerts pressures and so forth. And so, therefore, we have been pushed towards being applied. A response I have, which is not an attempt to change that, that is exactly what's happened. CMU has gone along with that in spades. We started going along with that in the 1970s. I can remember having conversations with Howard Lackler [?] as we were sitting there under the fixed funds with inflation and still trying to get an extra PDP-10. We were allowing that environment to shape us up so that we were not in fact anything like? And we became project-oriented all in that period. We were not project-oriented in the 1960s at all.

NORBERG: But you knew that was happening to you.

NEWELL: Absolutely! Absolutely knew it was happening and accepted it. The corresponding thing is the cats that want to play that game -- the Raj Reddys, the Canatis, and so forth, the Coons -- [find] the game they play is how to do that and do basic science at the same time within the same budget.

NORBERG: Winston said the same thing to me, from the MIT point of view.

NEWELL: ARPA community. No, this is not a unique point of view. If you can play that game then you can get the resources to do great science, because no one is going to give you the resources just to go do it. Now, maybe it's all our fault that we didn't sit back and say, "We refuse to do anything." Of course, it would have created a different computer science. And my belief is that computer science doesn't allow itself to be created as a basic field untied to applications.

NORBERG: That gives purpose to a rather nebulous concept.

NEWELL: If we try to describe the 1960s that way, what do you think was happening with ALGOL? Do you think ALGOL was just basic research?

NORBERG: No.

NEWELL: No, ALGOL is an attempt to provide the language that we all use. ALGOL, on the other hand, was a vehicle, and very strongly ARPA-influenced, as you probably know. Two of the people who played the strongest role from the U.S. side were Al Perlis and John McCarthy. Guys coming right out of the ARPA ethos with a whole bunch of ideas that are quite different. So a guy like Al Perlis almost never does anything that wasn't tied to languages. And, in fact, the ethos in the field is you can't just go out and design programming languages that aren't used. There's a lot of scorn in which, "You can't have a thesis as another programming language thing, because you don't build programming." Now, that's not true. Some people build some of them, but the whole ethos is that the big success stories, the big things that are our basic results, the Alto. You know about the Alto?

NORBERG: Yes.

NEWELL: There was never a scientific publication issued on the Alto. Dan Siewiorik and I, not me very much, but I know talking with Dan, in a second edition of the Siewiorik, Bell and Newell book finally got those characters to write an article that we could put in there as a reprint so there would at least be one scientific article some place that described the Alto. Because the Alto, in fact, was a very important thing for computer science.

NORBERG: Incidentally, would you say that that was influenced by DARPA ideas and principles?

NEWELL: Yes, absolutely. That's because Xerox PARC is an extrusion of DARPA.

NORBERG: By accident?

NEWELL: No, by design.

NORBERG: Why do you say by design?

NEWELL: Because Bob Taylor went out there and hired all the people, and as far as Bob Taylor and his funny point of view, the only good people in the world to hire are ARPA people. The causal relationships run the other way, that you must know the counts of the number of CMU folk and MIT folk...

NORBERG: Oh, yes.

NEWELL: ... and so, de facto. And Butler Lampson is an ARPA critter. BBC is sort of an ARPA spin-off. And they viewed themselves as a part of ARPA. They couldn't take any money from ARPA, except a $40,000 contract, which was there only so they could get on the ARPANET. They took the contract because then they could be an ARPA contractor and then they had full access to the network. If they had put up their own $40,000, which they would have much preferred to do, and they wouldn't have been beholden in any way, then they couldn't have been. So they became a contractor.

NORBERG: Let me switch, Al, to a couple of other topics, because I don't want to run out of time before I cover a couple of other things. And where these notions of the DARPA community and so on begin to trouble me again as I try to rewrite that section, I can do this on e-mail, I think, to get clarification of that. I want to shift to two other, what I think are important areas. One of them has to do with the relation of AI to the rest of computer science over the last 25 years. Now, no need to do a detailed history of that, but my reason for asking the question is that one of the things that I have noticed in the literature is a frequent -- I don't know whether I could go so far as to call it a compulsion, but a frequent stance taken by people in AI justifying their field before their computer science colleagues. Now, what's the reason for that? What's the relation of AI to computer science generally?

NEWELL: By the way, I was talking about that earlier.

NORBERG: Yes, you were; I want to enlarge that description a little.

NEWELL: You're going to have to start at that place again, because the relationship is one of suspicion and antagonism on the part of the central computer science folk, self-designated, I suppose, which is the programming field. The programming field sort of owns computer science. It doesn't quite, but that's where it comes from. The people who built the machines continued to view themselves as engineers. For a long time they were in engineering department, so they did not participate in the growth of computer science. And it is, in fact, programming, which is completely central and basic, and in some sense is the one new thing on the face of the earth that wasn't around anywhere before and therefore no one else owns it, and so, "We own it and that's us." That's a computer science statement.

NORBERG: Yes.

NEWELL: So in some sort of obvious way, which does not represent a turf battle or an ideological struggle of any kind, there is just a recognition that programming is of the essence, and therefore, in some sense, the programming language, and ultimately the programming systems types. are the heartland of computer science. And then all the other parts come in. There's got to be some theory. All the theory is irrelevant for a long time, until we finally do some analysis of algorithms. Deep suspicion all the way along that AI is intellectually dubious, probably morally wrong. It comes about from a basic proposition that says that computers are to serve people, and if you are trying to work with computers in their own terms it doesn't relate to how they serve people. You're just trying to build an intelligent system. So a continuing theme, all the way from the late 1950s, is articles -- which I suppose with a little historical work I could dig them up -- that say, "I wish to build systems that aid the human in doing something," as opposed to these cats in AI, who just want to build intelligent systems. "That's actually morally reprehensible." Mild, but that's the flavor. So this suspicion is always there. It was there at the opening, and it remains. And therefore, you get interesting little snipes all the way along from the programming community, and you get justificatory remarks from the AI community. At the same time, you get presumptions from the AI community that say, "I don't have to justify myself. Screw you." I mean, you get both kinds of things. Now, the peculiarities come from some of the great events that happened in computer science being sort of AI-like events. And some of its most visible... If you will allow me to use visible now in maybe even its pejorative sense... the most visible people being AI people. And so there is a lot of those AI types who get early Turing awards.

NORBERG: Yes.

NEWELL: Which is to say that if you line up the characters... I mean, different committees are doing this each time... But if you line up the characters, you say, "We can't give it to him because we have got to give it to him." And in fact, the boundary is not tight. A guy like McCarthy works across it. A thing like LISP becomes deeply important, but not right away. It takes quite a while.

NORBERG: Deeply important to what? To AI or the rest of computer science?

NEWELL: No, no, the rest of computer science. The issue of functional languages and so forth. So it was done. So that when McCarthy gets the Kioto prize or something that he's gotten, sitting behind that is not AI. Sitting behind that is the role of LISP in the whole thing. Let's see, what else is to be said about that? There is a thing that exacerbates this, which comes about because ARPA sort of defines... The ARPA community defines computer science, not the total of computers or computer technology, or even all the supercomputing stuff, which lives a life of its own. And that community gives immense resources to the AI types. Furthermore, the institutions at the top of the prestige hierarchy turn out to be institutions that have strong AI. And so, in fact, one of the things that's really interesting in some of the early listings... As you go down the list you sort of say, "Well, the top three here are MIT, Stanford and CMU. They all have strong things," and now you start looking for an AI department that's worth anything. And you go down the rest of the list, sort of in order, and you have got to get way down there before you begin to pick them up again. And so the rest of the field eschews AI. That is a field that is essentially full of programming people as the central people. It has filled this whole community, which is defining... Because these are the departments that are beginning to define and take control. Perfectly standard; I don't think there's anything exceptional about this at all. They take control of the discipline. And the AI people don't. The ARPA community plays slowly less and less a role. But not quite. They're all high prestige institutions. So you get a piece of envy mixed up in all of this. These are the guys that have all the resources as well. So in some respects, the success of AI, relative to some other things, is in part due to the fact that somehow they have all these resources conveyed on them and the rest of the field has to suffer, epitomized, by the way, by the Stanford situation, in which the department was a pauper's department, while the AI lab was rich. I had enumerable conversations with George Forsythe in which he would anguish over his problems, just anguish over his problems about how to convince the administration to give him another half... Nothing surprising to you, but standard, especially at Stanford, you see, where the rest of Stanford is run by all these great guys, Nobelists and so forth, and what do they care what happens to this little upstart. It has no standing at all, and Forsythe was struggling with this, and not at all unhappy with John. I mean, this is just Forsythe struggling. He understood this. He thought this was great to be happening, so it wasn't an issue that he was sort of fighting him. But it does illustrate this whole issue of the resources. Now, of course, within the ARPA community resources were conveyed for lots more than AI. In fact, one of the things that's interesting is to actually ask why Michigan did not become a member of the ARPA community. Michigan was, you know, if you go back and look. And the characters at Michigan were all involved in it. There were contracts let to Michigan. All I know is, well, they were there last Sunday and somehow, this Sunday they aren't around anymore. [laughter] Well, that's because the real activity always occurs between program managers and research contracts and no one else is privy to this, except at the gossip level. Just like in the speech program in which we had all this activity. But in some sense the real activity was that the program managers went and negotiated with the individual things, because the committee couldn't have any power at that level and just stayed away from it. Totally ignored it. So Michigan was there for a while. I don't know whether it was a stodginess of those characters, the guys that worked on MAD essentially, Bruce Arden and...

NORBERG: Herzog...

NEWELL: Herzog...

NORBERG: Galler...

NEWELL: Galler. Bernie Galler... Who were -- maybe I shouldn't call them stuffy -- staid would do.

NORBERG: It's a bit of a problem, because I am trying to remember now whether it's Roberts who told me the story or whether it was Taylor, but it was one of them. The way they tell the story is that a contract was given to Michigan to develop graphics, input, output graphics materials and display using the IBM 360/67, I guess. They had this contract to do that, but they had a significant amount of difficulty, apparently, in making the time-sharing portion of that machine work at Michigan.

NEWELL: You bet. Everybody did.

NORBERG: And the program director got angry with that and felt that it was a contribution to IBM that he didn't wish to make, and terminated the contract.

NEWELL: As opposed to somehow taking an initial contract and parlaying it into full membership.

NORBERG: That was his side of the story. I wanted to talk to Galler and Herzog the last time I was up there, but it was a holiday weekend and they were both away.

NEWELL: No, that doesn't sound unright to me, but I didn't know anything about it. There was an attempt, deliberate attempts in the ARPA community, in the ARPA front office, to talk about how to bring additional universities in.

NORBERG: How early would you say that was?

NEWELL: I don't know. I don't believe the Michigan thing happened that way because they thought you ought to try and get another one in. I thought that it happened just because the fact they were open to all universities to come participate. I believe, in fact, that the Berkeley and the Columbia ones were motivated in part by the attempt to see if they couldn't add another couple of universities.

NORBERG: But isn't that much later? The Columbia one?

NEWELL: Yes.

NORBERG: Oh, yes. There is a determined effort to bring in more universities later on; that's true.

NEWELL: But my belief is that that's, again, a long-standing... the long-standing positive policy attitude of a number of the people in the office at various stages of the game. It gets to be more or less worrisome and assists various places on the priority scheme, relative to whether there's bucks around and so forth. So it clearly isn't happening...

TAPE 6/SIDE 2

NORBERG: ... stick to the principles that Licklider established for the program and therefore continue the time-sharing activities and things around time-sharing in the few universities that are involved with it, and add to that the networking. And until...

NEWELL: There were no graphics people.

NORBERG: Well...

NEWELL: But Licklider didn't start the graphics.

NORBERG: That's true. Sutherland starts the graphics. But it's a very small program in comparison to the others. There's not a lot of money going into it in comparison to things like time-sharing and then later, networking. And certainly, ILLIAC swamps the whole thing with the kind of money there. So those people claim, Sutherland, Taylor and Roberts all claim, in different ways, that they were simply carrying out the program of Licklider. And it's only after Roberts that things begin to change and that the program gets to be some...

NEWELL: With Roberts come the networking, because networking...

NORBERG: True, but also with Roberts in developing speech understanding and then moving into distributed information systems and so on. So towards the end of Roberts' term you begin to get a shift and a response to application for military purposes. Now, that's the way I see the program.

NEWELL: Well, I can believe the... What I can't see, and you can, is you couldn't get it from me that there was not response to application going on. That's because, again, two separate stories were told.

NORBERG: What do you mean, two separate stories? One to Congress and one to the community?

NEWELL: Well, Congress and the military, much of the military is Congress, about what this was worth and what it was good for and what it was doing, and then a separate dialogue between ARPA and the community, which I told you was sort of upfront.

NORBERG: Yes, but I am reluctant to explore that too much.

NEWELL: I don't blame you.

NORBERG: And the reason that I am reluctant is it makes it look like the people in IPTO were two-faced about this.

NEWELL: But there is a general view, which I actually have, which is that that's the proper role of someone. That is, that they are brokers. And they have to be honest brokers.

NORBERG: All right. From that point of view one could probably take that position, and then when you read Licklider's justifications for making awards to MIT, CMU, or whoever, he is putting it in a context that this is going to provide certain kinds of things, somewhere down the road. He didn't promise them right away.

NEWELL: Right. But from my point of view, then, I haven't got the foggiest notion what construction is being made of this research from these guys with respect to the military. That's what I meant.

NORBERG: Sure. Then from your point of view, looking at this operation now over the last 25 years or so, what do you perceive were the guiding principles of the IPTO office over time? They don't have to be constant.

NEWELL: I believe the guiding principle started out to be the creation of computer science, called information processing, not called computer science. That Lick saw clearly the whole development of this field and set out to do that with the standard justification of the 1960s that out of that would flow good things, *really* good things for the country and the military. Those did not have to be separated in those days. That that's what he set out to do. The issue of more and more direct coupling to application has, in fact, proceeded over time to where it's relatively averse [?], leading to this sort of adaptation on the part of the community of which Pat Winston's is an expression, and I just gave you an expression, which is an adaptation of the community. That's not the attitude we had in the 1960s. There have been two antagonistic participators in that within the ARPA community... within the ARPA office. And I include not just IPTO, but somehow the whole surround of concern down there. One group sees this as a tactic in order to continue the original goal. And the other one sees this as, in fact, the proper bringing into relevance of this engine, for which, although there are lots of detractors, there are also lots of people that believe that ARPA... now, ARPA, not IPTO... ARPA has been a generator of a large number of good things and can do things, that's the only way you can get them done, and that's a good thing for the Defense Department and it needs to have, and it needs to be brought to produce those good things. The game is to get these things produced. And so a number of people, a very substantial number of the powerful people at ARPA from time to time have that view totally and completely, bringing ARPA to heel. It is mixed with the view of, "I want to get the product," and a view of, "I want to bring this organization to heel." And you don't have to separate those if you are playing this game. You can have it that its been free-wheeling too long. As Heilmeier said, "We have spent $35 million getting this started. Now, what have you done for me lately? Now it's time to deliver." There is a piece of that that isn't just, "I want the products." There is also a thing that says, "You have had your vacation; now it's time. The palmy days are over." But there's another set of people down there, mixed in with them, whose vision is really related to the original vision, although it shifts, because when Lick starts it there is no such computer science thing, and then, in the 1980s, it exists already. The issue is fundamentally one of this same long-term growth of information technology, and the application stuff is all tactics. The environment forces you to do this. Like the justification for quoting that thing in the Congressional stuff, you wouldn't put it in there unless you had to, but that's what it takes to get the 250, so it's playing the game. And both these kinds of characters have existed throughout ARPA's later history.

NORBERG: If you don't play the game, do you reduce the base? The money that would have gone to this research element that we talked about before?

NEWELL: Let's see. According to whose belief? My belief?

NORBERG: Well, according to...

NEWELL: Or my notion of what Lukasik believed?

NORBERG: Well, let's just separate those two and get two answers.

NEWELL: I mean, my belief is mixed up. My instinctive belief is, in the short run, no; in the long run, yes, except that I believe that computing is so essential to the society. You see, that there are a lot of other forces that come to bear, and I believe that you could actually destroy ARPA, throw it away, and you wouldn't get ARPA back. You get a very different... You might get a Bureau of Standards back or some damn thing. But there would be the resources there, as long as the nation believes, as it still does, that science and technology is a...

NORBERG: Well, you remember De Solda Prices analysis of the budgets for science back in the 1960s when he was writing *Little Science, Big Science*. He made claims about when the budget for science reaches one percent of the

Gross National Product that people are going to begin to look at it, but before that it's probably not very significant, and it turned out -- whether they read his book and decided they better look or not is hard to say -- in any case, that sort of happened. Do we see the same sort of phenomenon, perhaps, in computer science?

NEWELL: No.

NORBERG: Now that budgets for computing have reached such levels across the federal government, is it time that Congress took a more careful look? And therefore, is it possible that research budgets will indeed be cut? I am asking for a prediction, I suppose, rather than an analysis.

NEWELL: Right, right, we left history. Always suitable to the last bit of... The answer is no.

NORBERG: They won't look at it.

NEWELL: Well, of course they will look at it. I am just saying where they will end up. The high performance computing thing is an example. They did look at it in high performance computing. Now, one of the ways in which computing differs radically from some other things, but not quite all, is its breadth, that is, the number of different things. Now, I suppose if you were being political you would also say the number of different constituencies. But it's not sort of a single component.

NORBERG: Oh, but neither is physics and neither is medicine.

NEWELL: Right, and in fact medicine still does pretty well.

NORBERG: For obvious reasons, I think.

NEWELL: Right. But, in fact, of course, at this end level of conjecture there are really interesting issues of what's going to happen to medical research by the time the costs of medicine get to where they induce a national reaction that says, "The only way we're going to bring costs under control is to stop producing new stuff so we aren't tempted." A perfectly reasonable...

NORBERG: It is.

NEWELL: ... crazy reaction, but certainly possible. And in fact, the physicists, who at some level... the expensive physicists, who at some level have not been contributing to anything for a long time, are probably really in a lot more danger then they think they are.

NORBERG: Yes, well, between you and me, I hope they are, not because I don't support basic research, but...

NEWELL: Not because you don't want your little cut of that...

NORBERG: [laugh] No, I will stay with the DARPA community, thank you. When I asked you the question about guiding principles you took it in the direction of overall management of the program. And what I was sort of asking had to do with what sorts of areas they were going to support. But I didn't get down to that level to see how you would answer the question first.

NEWELL: Right, I thought what I was answering was what I really thought were guiding principles. Now, you don't like it when I say this, but it is a DARPA view that one does in fact open up brand new areas, and, therefore, one doesn't know these areas to begin with. Okay. And that is in conflict with the continued support of other areas, which now become like the national debt, that become tar babies. AI is one of these and networking is another. And so, in fact, again, this is related to the five year model, but no longer with a kind of a time scale in it, the issue is when you enumerate the big DARPA successes, mostly what you enumerate -- not you, Arthur Norberg, but you, DARPA apologists -- what you enumerate are all these new things that are brought onto the face of the earth -- networks, graphics, time-sharing -- partly because new things are easy to count. But there are other things. When you write the history of computer science, not the history of DARPA, they represent major things, partly because computer science does not have the equivalent of the discovery of laws and so forth, that sort of substitute for these, so that the big event is finally getting Maxwell's equations written down. There are not very many Maxwell's equations floating around in computer science. Again, that's not the nature of the scientists; I believe that's the nature of the field. DARPA doesn't have to invent areas; the community will invent them for it. Now, I don't know whether characters buy into that, but I think they have bought into that at various times.

NORBERG: They certainly have.

NEWELL: A real ethic. Well, but they buy into it at the same time. Now, I will give you an apologist's point of view from Larry Roberts. At the same time that Larry Roberts says, "One of the things I am most proud of is that we, the DARPA office, started networking. Oh, but that was the field because I am a member of the field." [laughter] But that's not a contradiction of the general point of view that things are not controlled from the top. Those are controlled from the top. It's just that I, Larry Roberts, have every bit as much right to push the art as anybody else does.

NORBERG: That's not an unreasonable position, do you think?

NEWELL: Well, I don't think it's reasonable for Eric Bloch in NSF. I think Eric Bloch is up there as a statesman and an administrator and a policy guide, but I don't think that Eric sits down and does Eric's research and then gets NSF to implement it, whereas I do believe that Larry Roberts can sit down and do Larry Robert's research, and inside Larry Roberts, with a little checking off from his boss, go do networking. The story is not anything quite that simple, but at a certain level it is. And this is related to what is the relationship of the guys at DARPA to the field. And what happens when you bring a guy like Russell in, that changes, because Russell doesn't have the right to do that. That doesn't come with the office. It comes with your standing in the field. So Russell doesn't have... Barry has, within his field. Well, you see, again, there are some sort of more general and less general guides. Thus, Lick, Ivan, in a very peculiar way, because he is in one sense quite specialized, yet now perceived quite that way; Larry Roberts, ditto; all have sort of a right to go do their thing. By the time we get to the Saul Amarels and the Jack Schwartzs they don't have that right.

NORBERG: Now, why is it they don't have that right, because the field has changed, because the office structure has changed, because of the personalities of those men?

NEWELL: Because of the scientific... I can't say scientific stature, because in some sense the scientific stature... The scientific stature of Saul is sort of medium. The scientific stature of Jack Schwartz is really pretty good. So it really relates to the acceptance of the field, of whether they will tolerate that. And I actually might have been wrong about Jack Schwartz, except that Jack was so antagonistic with the community.

NORBERG: I would like to say in our report that one of the interesting things about the IPTO program is that the things they supported changed the face of computer science.

NEWELL: That's the mythology.

NORBERG: Well, but I...

NEWELL: It's part of the mythology.

NORBERG: ... I think there's lots of grounds for that position.

NEWELL: Absolutely right.

NORBERG: And one of the things that I...

NEWELL: I use mythology partly as a way of saying, "That's how we shape up the full complexity of...

NORBERG: [laugh] Because we are in the position now of writing a statement about what computing was like in 1960.

NEWELL: Right.

NORBERG: And we will do the same thing, and it's much more difficult we're finding, for 1990. And the way in which we are going to show the transition from those two is to be able to point out what in 1990 is a result of the activities that came out of DARPA-supported program.

NEWELL: And that's absolutely identical to the argument I was making about how you tell what's great and what's not great. If you can, you go to the field and you try and make an independent assessment of product -- Maxwell's equations, superconductivity.

NORBERG: But you see, by our doing this, we don't have to make judgments about excellence, or quality or any of that. All we have to be able to say is that, "Now we have timesharing; then we didn't. Now we have graphics; then we didn't," and so on.

NEWELL: You're right.

NORBERG: But they're gross terms; we can argue it that way.

NEWELL: Right, but there's an implicit thing that getting time-sharing is a significant event in computer science -- meaning positive event. It's like Maxwell's equations. There is a belief that each of those things are the stuff out of which the field is made. That is, you in some sense can enumerate... Here's a way of saying it: you can enumerate computer science by enumerating its technologies, its micro-technologies, just like you can enumerate physics by enumerating its laws. And in physics you sort of list all the great laws and the great empirical discoveries.

NORBERG: Well, how does this relate to the list of items that you have in the UTC book, ranking the various areas of computer science, from the bottom with switching systems through to knowledge systems?

NEWELL: Orthogonal. Totally orthogonal.

NORBERG: They are.

NEWELL: Yes. This is simply a description of the different levels of description that you can give. In computer science, there is a set of developments at the level of circuitry. And there is a set of developments at the level of programming. And the fact that you can list all the developments at the level of programming doesn't mean that there wasn't something called VLSI that came along. And so, in one sense this is a geographical, it turned out to be sort of vertical, geographical description of the field, which says that there is simultaneously work going on at all of these levels. And one of the peculiar things that we observed about computer science as a developing field was at some of these lower levels, to begin with, but not now, totally, were deeply ensconced in electrical engineering departments, and thus did not participate in the development of the field. Only slowly, later, did they participate. They participated later when software invaded them. Software invaded the construction of computers. It turns out that the way that you build computers today relates to all kinds of design tools. And they are an essential feature. Therefore, in fact, people who are deeply imbedded at this computer construction level... Some of these guys are deeply imbedded with building compilers for simulators. They are deeply imbedded for building the tools that allow you... those guys that are working at that level. But they are working with the same kind of sophisticated software as these guys. So all of a sudden you can't be a computer engineer without in one sense living in the same software. And as soon as you do that, then you are much more a part of the field. And this got expressed in a bunch of rhetoric about there's no difference between software and hardware.

NORBERG: Yes, I see that.

NEWELL: I mean, various ways of expressing this at different times. Once you are part of the field, then, of course, we still had it divided over the departments. So, in fact, the sort of institutional struggle for how it's been defined goes on, because there's no way you can divide a field over departments and have it stable, because departments control things.

NORBERG: Yes, although you tried to tell me yesterday that departments don't make a difference.

NEWELL: I tried to tell you that here a department didn't make a difference, because that was simply a label that got put on an existing state of affairs. I did not try to tell you that in general.

NORBERG: There's still so many questions.

NEWELL: We have got to quit.

NORBERG: Yes, we do.

NEWELL: But you can have another 20 minutes.

NORBERG: Well, I don't think I will take another 20 minutes, but I will take 10. Do you subscribe to the view that AI stimulated many, if not all, of the frontier developments in computer science?

NEWELL: No. Many, maybe, but not all, by all manner.

NORBERG: Which ones did it stimulate? The field of AI?

NEWELL: I'm just thinking about that. So let me try a number of things on you, and maybe it's okay with the word "stimulated" as opposed to the words "was totally responsible for," because for many of these things there are other parts of computer science and parts of technology that are also have had big effects on them. So then, in fact, the time-sharing, list processing, which in one sense it owned, the interactive computing. Much less sure about networking, and now, of its role in networking. But, we actually have two levels of networking. We have the original ARPANET and then we have local area networks, the Ethernet, and so forth. The answer is, probably not at all, to any significant extent. So that all of interactive computing. Its relationship to graphics is very odd. So I am not quite sure where I would put this relationship to graphic. And I can tell you a story about that. Ivan comes about as close to a pulse with his thesis. Not done an isolation, he was sitting right there in the heartland of the heartland. When Sketchpad comes on line and is demonstrated, the world is thunderstruck. No question; a great thing. It turns out the reason it's a great thing is that it's two breakthroughs at once. Most of us can't get one breakthrough; when you get two of them at once it's pretty good. One breakthrough was the whole issue of graphic structure. The second breakthrough was constraint programming, in which you programmed this thing by just putting things into place and laying constraints on them and the figures -- in. The real demonstrations were this way. The last thing he did when he gave a talk was to build a linkage system. Pull on this thing and had all the things kind of turned. You had to be careful not to exceed the computing capacity or else you broke the thing. But if you did it right, you could make a great demonstration. Now, the world really responded to this, and in some respects there are three things that are going on there, but I want to put two of them together. There is, in fact, the graphics. There is, in fact, list processing, the system called Carl [?] to support those graphics. And the thing that made this thing possible was underlying this processing structure. And then the other one was this whole constraint satisfaction system. This is really AI-ish.

NORBERG: The constraint satisfaction.

NEWELL: Yes. The list processing is just what it was, although Ivan kind of asserts that he invented it all himself.

NORBERG: So we have three claimants here.

NEWELL: Well, I don't think he claimed it that strongly, but I remember talking about it on several occasions in the

1960s and he would not see this as going and borrowing, but simply as a response to what was required. As a matter of fact, from a historian's point of view that didn't make any sense at all because he was sitting here embedded in the world of 1962. Now, what happened in the future was that the whole world picked up the graphics and didn't pick up this at all.

NORBERG: Didn't pick up the constraint.

NEWELL: Right. Until you finally come up to the constraint languages and Thing [?] lab and a bunch of things here in the 1980s. Absolutely did not pick it up, but picked up the graphics like mad. And so there was a very funny split in which the programming community cottoned to the graphics stuff very strongly, abandoned the AI part. So in one sense it's not clear. If they had picked up this other part I would have said that AI had probably a pretty strong influence on graphics. I think the answer is probably not.

NORBERG: Now, from the 1980s, did they come back together again, and did that make graphics a somewhat larger field than it might have been?

NEWELL: No, they had come back together again. It's a ? that they have. I mean, it's just one more kind of putting things together and things are happening and that helps and so forth. And there was a little development of constraint languages, some of which again happened at MIT, and Sussman. The development of constraint languages is, in fact, all out of AI, the second wave of development after Ivan was down. Then Sussman brings it back and a bunch of students like Guy Steele do AI theses fundamentally and build those kind of constraint-based systems. And they become fairly popular. In fact, they now have invaded programming, because it turns out that you can do all kinds of simple things with these constraint languages. You take object-oriented programming and constraint languages and you put them together and you get a pretty powerful engine. You see it in graphic systems. You've got one around here. Garnet. Object-oriented and constraint-based. You build the whole thing by laying in constraints. So, yes, the answer is having an influence, but we are all taking about mature technologies sort of invading each other. I mean, we are talking about synergies. Now, I didn't give you expert systems, because I think of expert systems as an application of AI and, therefore, off the other side. And expert systems has not influenced computer science very much. What else has computer science really done that influences the rest of CHU? Well, in fact, object-oriented programming...

NORBERG: Comes out of AI.

NEWELL: Well, it's a funny story. I mean, the answer is no, the original one comes out of SIMULA, *the* original language. But the language that kind of picks up SIMULA again and does it all in a way that lays it out is SMALLTALK. But SMALLTALK is completely open and overt. SIMULA is a really programming... A bunch of Swedes doing programming languages and built a system called SIMULA, and it really is object-oriented. I mean, so that really is the source of it, but if you ask what really made it happen -- it was something like five or six years later -- maybe even longer... No, it can't be... everything is compressed... Alan Kay comes in. Alan Kay is fundamentally a creature of the AI world, a mixed character in the sense that working on SMALLTALK is not doing AI; it's building systems, but he was essentially at the Stanford AI Lab for five years or something like this just out of Utah.

NORBERG: Yes, in graphics to begin with.

NEWELL: So with graphics to begin with out of Utah, then came to the Stanford AI Lab where he was sort of main honcho. I mean, the Stanford AI Lab shrunk pretty strongly after a while. So in one sense there were ways in which there was sort of a significant guy there, because one of the things that happened is that -- and you have got to know this better than I do -- was it went froma single contract to fractionation into pieces. Terry Winograd got his piece, and Ed Feigenbaum got his piece, and so forth, and never the twain shall meet. And, therefore, the lab....

END OF INTERVIEW